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WELLS, J. LELAND....Gillis & Geoghegan, 875 East 169th Street, New York City.
WEST, THOMAS D....Supt. Cuyahoga Works Foundry, 41 Birch Street, Cleveland, Ohio.
WESTINGHOUSE, H. HERMAN..V. P. Westinghouse Machine Co., 17 Cortlandt Street, New York City.
WESTON, EDWARD..............................645 High St., Newark, N. J.
WHEELER, FREDERIC M........George F. Blake Mfg. Co., 95 & 97 Liberty Street, New York City.
WHEELER, HERBERT A................Washington University, St. Louis, Mo.
WHEELOCK, JEROME.................. ....Worcester, Mass.

WHITAKER, EZRA T ...Chief Eng'r U. S. N., Steamer "Adams,"
Care U. S. Consul, Panama, U. S. C.
WHITE, H. C.........Mang'r in N. Y. for B. W. Payne & Sons, 107 Liberty St., New York City.
WHITE, JOSEPH J......Mang'r H. B. Smith Machine Co., 925 Market Street, Philadelphia, Pa.
WHITE, MAUNSEL..Bethlehem, Pa.
WHITHAM, JAY M........Prof. App. Math., Ark. Ind. Univ., Fayetteville, Ark.
WHITEHILL, ROBERT ....Newburgh Eng. Co., Newburgh, N. Y.
WHITING, GEORGE B......Bureau Steam Eng., Navy Dept., Washington, D. C.
WHITING, S. B........Gen. Mang'r Phil. & Reading Coal and Iron Co., Pottsville, Pa.
WHITNEY, BAXTER D ....Winchendon, Mass.
WHITTIER, CHAS............................1176 Tremont St., Boston, Mass.
WIGHTMAN, D. A..............Supt. Pittsburgh Loco. Works, Pittsburgh, Pa.
WILCOX, STEPHEN......Babcock & Wilcox, 30 Cortlandt Street, New York City.
WILDER, MOSES G..................1533 Centennial Avenue, Philadelphia, Pa.
WILEY, WM. H..............................15 Astor Place, New York City.
WILLIAMS, J. NEWTON......V. P. Automatic Bank Punch Co., York and Washington Sts., Brooklyn, N. Y.
WILLIAMS, J. R.............................620 No. Main St., St. Louis, Mo.
WILLIAMS, SAMUEL T...Gen. Man. Steel Works Dept., Disston & Sons, Tacony, Philadelphia, Pa.
WILLIAMSON, WM. C....Williamson Bros. E. & B. Works, Richmond & York Streets, Philadelphia, Pa.
WILLSON, FREDERICK N..........Prof. Graphics and Tech. Drawing, Princeton College, Princeton, N. J.
WINTER, HERMAN........Const. Eng'r & Naval Architect, Pier 11, N. R., New York City.
WITHEROW, JAMES P..........................Lewis Block, Pittsburgh, Pa.
WOLFF, ALFRED R..............Consult. Eng., 85 Astor House, New York City.
WOOD, SAMUEL A........................Troy Steel and Iron Co., Troy, N. Y.
WOODBURY, C. J. H....Insp. Manfr's Mut. Ins. Cos., 31 Milk Street, P. O. Box 112, Boston, Mass.
WOODBURY, L. S ....2d Asst. Supt. Calumet & Hecla M'n'g Co., Calumet, Mich.
WOODS, ARTHUR T....Asst. Prof. Mech. Eng., University of Ill., Champaign, Ill.
WOODWARD, CALVIN M....................Washington Univ., St. Louis, Mo.
WOOLSON, OROSCO C...Newark, N. J.
WORSWICK, THOMAS..Guelph, Ont.
WORTHINGTON, CHARLES C ....86 & 88 Liberty Street, New York City.
WRIGHT, JOHN Q.......................Putnam Machine Co., Fitchburg, Mass.
WYMAN, HORACE W ....30 Bradley Street, Worcester, Mass.

ZELL, ROBERT R....Safety Steam Gen'r Co., 112 Liberty Street, New York City.
ZIMMERMANN, WM. F ....Mech. Eng'r, 98 Fourth Ave., Pittsburgh, Pa.

Associates.

ALLER, A .... Steam Jet Engineer, 109 Liberty Street, New York City.
BAILEY, E. B .... Mang'r The E. Horton & Son Co., Windsor Locks, Conn.
BAILEY, JACKSON .... *American Machinist*, 96 Fulton Street, New York City.
BAILEY, W. H .... Agt. Am. Tube Works, 20 Gold Street, New York City.
BROWN, DAVID W ...... Asst. Ed. *Sanitary Engineer*,
140 William Street, New York City.
CAMPBELL, ANDREW C .... Draughtsman Wheeler & Wilson Mfg. Co., and
128 E. Washington Ave., Bridgeport, Conn.
CLARK, FRANK M .... 128 Purchase Street, Boston, Mass.
DOUGLAS, E. V .... 713 Chestnut Street, Philadelphia, Pa.
EMERSON, B. F .... Supt. Copper Falls Mine, Copper Falls, Mich.
EVANS, EDWIN T .... L. S. Transit Co., 189 North Street, Buffalo, N. Y.
GIBSON, WM., JR .... Publisher *Sanitary Engineer*,
140 William Street, New York City.
HALL, JOHN H .... T. R. Pickering & Co., Portland, Conn.
HAWKINS, GARDNER C .... 115 Purchase Street, Boston, Mass.
KEPPY, FREDERICK .... 38 State Street, Bridgeport, Conn.
LOW, FRED. R ...... Associate Editor *Jour. of Commerce*, 128 Purchase St,
Boston, Mass.
MILLER, HORACE B .. .. *American Machinist*, 96 Fulton Stree,
New York City.
MOORE, CHARLES A .... 111 Liberty Street, New York City.
MOORE, LYCURGUS B .... *American Machinist*, 96 Fulton Street,
New York City.
PORTER, GEO. A .... Porter Mfg. Co. (Ltd.), Syracuse, N. Y.
RIDGELY, CHARLES .... Springfield Iron Co., Springfield, Ill.
ROCHE, JOHN A .... 207 Lake Street, Chicago, Ill.
RUSSELL, C. M .... Massillon, Ohio.
SHARPE, LUCIAN .... Brown & Sharpe Mfg. Co., Providence, R. I.
SPERRY, CHARLES .... Port Washington, Queen's County, L. I., N. Y.
STOCKLY, GEO. W ... . Pres't Brush Electric Co., Cleveland, Ohio.
WAINWRIGHT, C. D .. .. 65 Oliver Street, Boston, Mass.
WHITNEY, WM. M .... Winchendon, Mass.
WOOD, WALTER ...... R. D. Wood & Co., 400 Chestnut Street, Philadelphia, Pa.

Juniors.

BABCOCK, W. IRVING .... Prov. and Stonington S. S. Co., 177 West St., and
P. O. Box 3011, New York City.
BEACH, CHARLES S .... H. E. Bradford & Co., Bennington, Vt.
BURDSALL, ELWOOD, JR .... Portchester, Westchester Co., N. Y.
CALDWELL, ANDREW J .. Hyd. Works, Van Brunt & Rapelyea Sts.,
So. Brooklyn, N. Y.

Conant, Thomas P......The Electrical Accumulator Co., 44 Broadway, New York City
Cromwell, J. Howard....................................Cranford, N. J.
Darlington, Frank G....Eng'r Maint. of Way P. C. & St. L. R'y Co., Arch Street, Allegheny, Pa.
Day, F. M..Milford, Mass.
Dent, Edward L.....................1413 G St., N. W. Washington, D. C.
Foster, Ernest H.......................................Englewood, N. J.
Greene, Isaac C.........Columbian Iron Works, and Dry Dock Co, Baltimore, Md.
Guthrie, Edward B.......................Asst. City Eng'r, Buffalo, N. Y.
Higgins, Geo. F.........Asst. M. E. Amoskeag Manuf. Co., Manchester, N. H.
Hill, William...................................Collins Co., Collinsville, Conn.
Marx, Henry ....Hill, Clark & Co., 805 N. Main Street, St. Louis, Mo.
Moore, W. J. P.........H. R. Worthington, 114 Queen Victoria St., London, Eng.
Moran, Daniel E...........................85 State Street, Brooklyn, N. Y.
Neftel, Knight...........................32 Liberty Street, New York City.
Norris, R. Van A. ....173 Academy St., Wilkesbarre, Pa.
Owen, Edward H., Jr., S. B.....Supt. Atlantic Cotton Mills, Lawrence, Mass.
Parsons, H. De B....................Room 73, 35 Broadway, New York City.
Ridgley, Wm. Barrett.................Springfield Iron Co., Springfield, Ill.
Rowland, Chas. B.................Continental Iron Works, Brooklyn, N. Y.
Smith, Albert W.............................Ithaca, Tompkins Co., N. Y.
Smith, John Wm.........................Solvay Process Co., Syracuse, N. Y.
Stevens, Wm. N......H. R. Worthington & Co., Van Brunt and Rapelyea Sts., Brooklyn, N. Y.
Stone, Wilbur M.........................6 Townley Street, Hartford, Conn.
Suter, George A........Eng. N. Y. Exhaust Vent. Co., 45 Fulton St., New York City.
Torrance, Kenneth......Mech. Eng., 184 Columbia Heights, Brooklyn, N. Y.
Trump, Edward N...........Mech. Eng. Solvay Process Co, Syracuse, N. Y.
Van Duzee, Harold...............................Box 29, Bergen Point, N. J.
Walden, Lienau.....................202 South Front Street, Philadelphia, Pa.
Whiting, Charles W.......................P. & R. C. & I. Co., Pottsville, Pa.
Williams, Harvey D...............Yale & Towne Mfg. Co., Stamford, Conn.

Deceased.

Henry R. Worthington..Dec 17, 1880.
Theodore R. Scowden...Dec. 31, 1881.
Alexander L. Holley..Jan. 29, 1882.
Erastus W. Smith...June 12, 1882.
Peter Cooper, Honorary Member................................April 4, 1883.
James Park, Jr..April 21, 1883.
W. K. Seaman...July 2, 1883.
Redmond J. Brough..July 21, 1883.
C. W. Siemens, Honorary Member.................................Nov. 20, 1883.
Henry F. Snyder..Nov. 25, 1883.

O. HALLAUER, Honorary Member............Dec. 5, 1883.
WILLIAM ATWOOD............Feb. 16, 1884.
WILMER G. CARTWRIGHT............Feb. 23, 1884.
THEODORE H. RISDON............May 19, 1884.
ISAAC NEWTON............Sept. 25, 1884.
J. H. BURNETT............Jan. 31, 1885.
HORACE LORD............Feb. 28, 1885.
D. H. HOTCHKISS............April 29, 1885.
HENRI TRESCA, Honorary Member............June 24, 1885.
HENRY H. GORRINGE............July 6, 1885.
WILBUR H. JONES............July 29, 1885.
FREDERIC E. BUTTERFIELD............Sept. 5, 1885.
WM. CLEVELAND HICKS............Oct. 19, 1885.
D. S. HINES............Nov. 9, 1885.
THEODORE BERGNER............January 5, 1886.
EMILE F. LOISEAU............April 30, 1886.

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

[*Adopted November 5th, 1884.*]

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART. 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else he must have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of or connection with applied science as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART. 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of membership. Honorary Members and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. Every candidate for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom he must be personally known, and he must be seconded by two others. The proposal must be accompanied by a statement in writing by the candidate of the grounds of his application for election, including an account of his professional experience, and an agreement that he will conform to the requirements of membership if elected.

ART. 10. All such applications and proposals must be received and acted upon by the Council at least thirty days before a regular meeting, when the Secretary shall at once mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate, and may, at his option, return to the Secretary such ballot enclosed in two envelopes, the inner one to be blank and the outer one endorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings.

ART. 14.—Candidates for admission as honorary members shall

not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

ART. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the initiation fee before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

ART. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered, at which thirteen negative votes shall be required to defeat the candidate.

ART. 17. Persons desiring to change the class of their membership shall be proposed in the same form as described for a new applicant.

FEES AND DUES.

ART. 18. The initiation fees of members and associates shall be $15, and their annual dues shall be $10, payable in advance. The initiation fee of juniors shall be $10, and their annual dues $5, payable in advance. A junior, being promoted to full membership, shall pay an additional initiation fee of $5. Any member or associate may become, by the payment of $150 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

ART. 19. Any member, associate or junior, in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year. Such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

ART. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, and a Treasurer, who shall be elected from among the members and associates of the Society at the annual meetings, to hold office as follows:

ART. 21. The President and the Treasurer for one year; and

no person shall be eligible for immediate re-election as President who shall have held that office for two consecutive years; the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

ART. 22. A Secretary, who shall be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting, a President, three Vice-Presidents, three Managers and a Treasurer shall be elected, and the term of office of each shall continue until the end of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or by the Society. The Council may, in its discretion, require bonds to be given by the Treasurer.

ART. 25. The Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office. All such vacancies and those occurring by death or resignation shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided* that the said appointment shall not render him ineligible at the next annual meeting.

ART. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes. Absent mem-

bers of the Council may vote by proxy upon subjects stated in the call for a meeting, said proxy to be deposited with the Secretary.

Art. 27. The President on assuming office shall appoint a Finance Committee and a Publication Committee and a Library Committee of five members each. The appointment of two members of each Committee shall expire at the end of each year. The Secretary shall, *ex officio*, be a member of all three Committees.

Art. 28.—The Finance Committee shall have power to order all ordinary or current expenditures, and shall audit all bills therefor. No bill shall be paid except upon their audit. When special appropriations are ordered by the Society, they shall not take effect until they have been referred to the Council and Finance Committee in conference.

Art. 29. It shall be the duty of the Publication Committee to receive all papers contributed, to decide which shall be published in the *Transactions*, and which shall be read in full at the meetings.

Art. 30. It shall be the duty of the Library Committee to take charge of the collection of all material for the Library of the Society, and to supervise all regulations for its use.

ELECTION OF OFFICERS.

Art. 31. At the regular meeting preceding the annual meeting a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send to the Secretary the names of nominees for the offices falling vacant under the rules. In addition to such regularly appointed committee, any other five members or associates, not in arrears, may constitute an independent nominating committee, and may present to the Secretary, at least thirty days before the annual meeting, all the names of such candidates as they may select. All the names of such independent nominees shall be placed upon the ballot list with nothing to distinguish them from the nominees of the regular committee, and the Secretary shall at once mail the said list of names to each member and associate in the form of a letter ballot, it being understood that the assent of the nominees shall have been secured in all cases.

Art. 32. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or dis-

tribute them among more, as he chooses. Managers shall be voted for in the same way.

Art. 33. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. No member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

Art. 34. The said blank envelopes shall be opened by tellers at the annual meeting, and the person who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

Art. 35. The annual meeting of the Society shall be held on the first Thursday in November of each year, in the City of New York, unless otherwise ordered, at which a report of proceedings and an abstract of the accounts shall be furnished by the Council. The Council may change the place of the annual meeting, and shall, in that case, give timely notice to members and associates.

Art. 36. Other regular meetings of the Society shall be held in each year at such time and place as the Council may appoint. At least thirty days' notice of all meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

Art. 37. Special meetings may be called whenever the council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

Art. 38. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

Art. 39. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

Art. 40. At any regular meeting of the Society thirteen or more members and associates shall constitute a quorum.

Art. 41. Unless otherwise ordered, papers shall be read in the

order in which their text is received by the Secretary. Before any paper appears in the *Transactions* of the Society a copy of the paper shall be sent to the author, and, so far as possible, a copy of the reported discussion shall be sent to every member who took part in the same, with requests that attention shall be called to any errors therein.

ART. 42. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Publication Committee and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall cause to be printed, with the authors' consent, copies of such papers, "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest circulation possible, with the view of making the work of the Society known, encouraging mechanical progress, and extending the professional reputation of its members.

ART. 43. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and all members shall have the right to order any number of reprints of papers at a cost to cover paper and printing; *provided*, that said copies are not intended for sale.

ART. 44. The Society is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 45. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

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* Presented by Title only. Republished in full in *Van Nostrand's Engineering Mag.*, Aug., 1886.

LIST OF ILLUSTRATIONS, VOLUME VII.

PAPERS

OF THE

BOSTON MEETING

(XIIth),

NOVEMBER, 1885.

[Sixth Annual Meeting.]

CLXXXVI.

PROCEEDINGS

OF THE

SIXTH ANNUAL MEETING

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

Boston, November, 1885.

LOCAL COMMITTEE OF ARRANGEMENTS :—E. D. LEAVITT, Jr., *Chairman ;* C. J. H. WOODBURY, *Secretary ;* Messrs. Norman, Lincoln, Hammond, Parker and Stone.

THE opening session was called to order in the banquet hall of the Hotel Brunswick at eight o'clock on Tuesday evening, November 10th. In the unavoidable absence of the chairman of the local committee, Mr. C. J. H. Woodbury welcomed the Society and its guests to the convention, and introduced Mayor O'Brien, of the City of Boston. The Mayor made a brief speech of welcome, referring to the debt which his city owed to the profession of mechanical engineering, and made way for General Francis A. Walker, President of the Massachusetts Institute of Technology. The President spoke warmly of his pleasure in welcoming the Society to the Institute Buildings, in view of the special interest and activity in the course of mechanical engineering in that school, and his salutation was most cordial. Mr. Edward Atkinson, President of the Factory Mutual Insurance Co., followed with well-chosen references to the benefits which successful engineering had conferred on mankind, particularly in those places where the soil was rocky and the ground sterile. Mr. Woodbury presented a letter of regret from Governor Robinson, of the Commonwealth of Massachusetts, that he was prevented from attending this opening session, and then gave place to President Holloway of the Society, who delivered his Annual Address.

At the close of the Address, Messrs. S. A. Hand and A. Swasey were appointed tellers to count the ballots for officers of the Society for the coming year, the Secretary made some announcements, and the session adjourned. Supper was served in an adjoining room and the evening was much enjoyed. The Boston Society of Civil Engineers were invited to join with the Mechanical Engineers, and added much to the pleasure of the evening.

Second Day, Wednesday, November 11th.

The meeting was called to order in Room 14 of the new building of the Massachusetts Institute of Technology, which had been put at the service of the Society by the courtesy of the Trustees and Faculty of the Institute.

The Secretary's Register contained the following names of gentlemen in attendance on the meetings, and quite a number of ladies were also among the visitors:

Alden, George I. .... Worcester, Mass.
Anthony, Gardner C. .... Providence, R. I.
Ashworth, Daniel .... Pittsburgh, Pa.
Babcock, George H. .... New York City
Bailey, E. B. .... Windsor Locks, Conn.
Baldwin, Stephen W. .... New York City.
Baldwin, William J. .... New York City.
Bancroft, J. Sellers .... Philadelphia, Pa.
Bayles, James C. .... New York City.
Beardsley, Arthur .... Swarthmore, Del. Co., Pa.
Billings, Charles E. .... Hartford, Conn.
Bond, George M. .... Hartford, Conn.
Boyd, James T. .... East Boston, Mass.
Brooks, Edwin C. .... Cambridge, Mass.
Brooks, Morgan .... Boston, Mass.
Campbell, Andrew C. .... Bridgeport, Conn.
Capen, Thomas Wells .... Stamford, Conn.
Church, Wm. Lee .... New York City.
Churchill, Thomas L. .... Boston, Mass.
Clarke, Samuel J. .... New York City.
Cole, J. Wendell .... Columbus, O.
Colwell, Augustus W. .... New York City.
Corbett, Charles H. .... Brooklyn, N. Y.
Cowles, William .... New York City.
Crosby, George H. .... Boston, Mass.
Crouthers, James A. .... New York City.
Cummings, A. G. .... Harrisburg, Pa.
Davis, Isaac H. .... Dorchester, Mass.

Dean, F. W	Scranton, Pa.
Deane, Charles P	Holyoke, Mass.
De Kinder, J. J	Philadelphia, Pa.
Douglass, Wm. M	Easton, Pa.
Durfee, W. F	Bridgeport, Pa.
Du Villard, Henry A	Providence, R. I.
Egleston, Thomas	New York City.
Fisher, Charles H	Lowell, Mass.
Galloupe, Francis E	Boston, Mass.
Good, William E	Reading Pa.
Grant, J. J	Fitchburg, Mass.
Grinnell, Frederick	Providence, R. I.
Hall, Albert Francis	Boston, Mass.
Halsey, F. A	New York City.
Hammond, George W	Boston, Mass.
Hand, S. Ashton	Toughkenamon, Pa.
Harmon, O. S	Jersey City, N. J.
Harrison, Wm. H	Braintree, Mass.
Hawkins, Gardner C	Boston, Mass.
Hawkins, John T	Taunton, Mass.
Hayes, George	New York City.
Henderson, Alexander	Washington, D. C.
Henthorn, John T	Providence, R. I.
Higgins, Milton P	Worcester, Mass.
Hill, H. A	Boston, Mass.
Hill, William	Collinsville, Conn.
Hoadley, J. C	Boston, Mass.
Hobbs, A. C	Bridgeport, Conn.
Hollingsworth, Sumner	Boston, Mass.
Holloway, J. F., *President*	Cleveland, O.
Holmes, George M	Gardiner, Me.
Howard, Charles P	Hartford, Conn.
Hutton, Frederic R., *Secretary*	New York City.
Hyde, Charles E	Bath, Me.
Illingworth, Joseph J	Utica, N. Y.
Jacobi, Albert H	Newark, N. J.
Kent, William	New York City.
Kettell, Charles W	Charlestown, Mass.
Lanza, Gaetano	Boston, Mass.
Lewis, Wilfred	Philadelphia, Pa.
Livermore, Charles W	Boston, Mass.
Main, Charles T	Lawrence, Mass.
Manning, Charles H	Manchester, N. H.
Martens, Ferdinand	College Point, N. Y.
May, De Courcy	Philadelphia, Pa.
Miller, Alexander	New York City.
Miller, Horace B	New York City.
Moore, Charles A	New York City.
Morgan, Charles H	Worcester, Mass.
Mudge, Benjamin C	Boston, Mass.
Murphy, Edward J	Hartford Conn.

Murray, S. W .... Milton, Pa.
Norman, George H. .... Boston, Mass.
Parker, Charles H .... Boston, Mass.
Parker, Walter E. .... Lawrence, Mass.
Phillips, Franklin .... Newark, N. J.
Powel, Samuel W .... Hartford, Conn.
Pusey, Charles W. .... Wilmington, Del.
Raynal, A. H. .... New York City.
Richards, Charles B. .... New Haven, Conn.
Robinson, S. W. .... Columbus, O.
Root, J. B. .... Greenpoint, N. Y.
Sancton, Edward K. .... Scranton, Pa.
Sanderson, Richard P. C. .... Roanoke, Va.
Schleicher, Adolph W. .... Philadelphia, Pa.
Schuhmann, George. .... Reading, Pa.
Schwamb, Peter. .... Boston, Mass.
See, Horace. .... Philadelphia, Pa.
Sheldon, Thomas C. .... Boylston, Mass.
Smith, C. A. .... Pawtucket, R. I.
Smith, Oberlin. .... Bridgeton, N. J.
Smith, Sidney L. .... Boston, Mass.
Snell, Henry I .... Philadelphia, Pa.
Soule, Richard H. .... Frankfort, N. Y.
Spies, Albert. .... New York City.
Stearns, Albert. .... Brooklyn, N. Y.
Stone, Joseph. .... Lawrence, Mass.
Stratton, E. Platt. .... College Point, N. Y.
Swain, George F .... Boston, Mass.
Swasey, Ambrose .... Cleveland, Ohio.
Sweet, John E. .... Syracuse, N. Y.
Thurston, Robert H. .... Ithaca, N. Y.
Tilden, James A. .... Boston, Mass.
Towne, Henry R. .... Stamford, Conn.
Uehling, Edward A. .... Bethlehem, Pa.
Upson, Lyman A. .... Thompsonville, Conn.
Vanderbilt, Aaron. .... New York City.
Ward, W. E. .... Port Chester, N. Y.
Warren, B. H. .... Boston, Mass.
Webber, Samuel. .... Lawrence, Mass.
Webber, William Oliver. .... Lawrence, Mass.
Webster, John H. .... Boston, Mass.
Weeks, George W. .... Clinton, Mass.
Wellman, Samuel T. .... Cleveland, O.
Wheeler, Frederic M. .... New York City.
Wheelock, Jerome. .... Worcester, Mass.
Wightman, D. A. .... Pittsburgh, Pa.
Wiley, William H. .... New York City.
Wood, Walter. .... Philadelphia, Pa.
Woodbury, C. J. H. .... Boston, Mass.
Worthington, Charles C. .... New York City.
Wright, J. Q. .... Fitchburg, Mass.

The Secretary read the Report of the Council to the Society as follows:

The Council would present the following Report of its Tellers:

NEW YORK, *Nov.* 4, 1885.

The undersigned, who were appointed a committee of the Council to act as tellers to count the ballots cast for or against each of the persons proposed for membership in the Society of Mechanical Engineers, to be voted for previous to the annual meeting, 1885, hereby certify that they have met this day at the office of the Society and proceeded to discharge their duties.

There were cast in all 272 ballots, but one ballot was thrown out because of informality, the name of the member voting not having been affixed, and all the persons whose names appear on the following list were duly elected in accordance with the rules to their respective grades.

CHARLES T. PORTER,
WILLIAM H. WILEY,
Tellers.

MEMBERS.

Allderdice, Winslow.
Brooks, Morgan.
Christie, James.
Cremer, James M.
DeArozarena, R. M.
Hartshorne, Wm. D.
Hunt, Charles W.
Jenkins, W. R.
Main, Chas. T.
Müller, Maurice A.
Nicolls, Wm. J.
Parker, Charles H.
Rommel, Chas. E.
Sprague, Wm. W.
Tilden, J. A.

JUNIORS.

Babbitt, S. S.
Dent, Edward L.
Norris, Robert V. A.
Rowland, Charles B.
Williams, Harvey D.

PROMOTION TO MEMBERSHIP.

Warrington, James N., Jr., A. S. M. E.

ASSOCIATES.

Bacon, Earle C.
Campbell, Andrew C.

The Council therefore report the following summary of the membership:

Honorary Members			11
Life "			5
Members			532
Associates			24
Juniors			27
Total			599
Increase at this meeting:			
Members		15	
Associates		2	
Juniors		5	
			22
Total			621

The losses by death since the last annual meeting have been as follows:

J. H. Burnett	New York.
Horace Lord	Hartford.
Henri Tresca (Honorary Member)	Paris.
Henry H. Gorringe	New York.
Wilber H. Jones	Wilmington, Del.
William Cleveland Hicks	New York.

A total of seven.

The addresses of Messrs. Willard B. Roberts and Norman W. Wheeler are unknown to the Secretary, letters to their former addresses returning unopened and indorsed "not found."

Since the report made by the Council to the Society at the XIth Meeting in May, 1885, the sessions of the Council have been occupied with routine business, receiving reports of the standing committees and scrutiny of applications.

The Secretary has been directed to prepare suitable memorial notices of members deceased during the society year, for publication in the Transactions.

The Council has directed that members delinquent in dues at the end of the Society year, who take no notice within thirty days of communications apprising them of their indebtedness, shall be considered as not caring to retain their membership and shall be dropped from the roll, under Art. 19 of the rules.

The following resolution is reported to the Society:

Resolved, That the name of General Francis A. Walker be placed on the next ballot-list as a candidate for honorary membership of the Society.

Respectfully submitted,
By the Council.

The Secretary then presented the reports of the Finance Committee and of its sub-committee, which examined the books of the Society. They were as follows:

REPORT OF THE AUDITING COMMITTEE.

The sub-committee appointed by the Finance Committee of the American Society of Mechanical Engineers to examine the books of the Society and to audit the accounts of the Treasurer, would beg leave to make the following report:

They met pursuant to an agreement at the office of the Society, on 4th November, 1885, and the account books, vouchers, etc., were submitted to them. They find that there was:

Received from Charles W. Copeland, retiring Treasurer, November, 1884	$751 03	
Receipts to November 1st, 1885	8,822 33	
		$9,573 36
The total disbursements ending November 1st, 1885	8,783 93	8,783 93
Leaving a balance on hand		$789 43

Of this balance, $600 stands to the credit of the Society in the Bleecker Street Savings Bank, New York City, on account of subscriptions to the Library fund, and $189.43 is cash in hand in the possession of the Treasurer, as by the bank-books submitted.

The total receipts above noted are to be apportioned among the following accounts:

Annual Dues	$5,547 90
Badge Account	135 96
Binding "	382 14
Engraving Account	153 35
Initiation Fees	1,005 00
Library Permanent Fund	435 66
Library Expense Account	164 00
Sales Account	998 06
Profit and Loss	26
	$8,822 33

The disbursements during the same period are classified as follows:

Binding Account	$335 50
Expense "	1,097 85
Engraving "	1,099 46
Furniture and Supplies	74 31
Library Expense Account	12 75
Meetings	621 25
Postage	180 65
Printing of Transactions	2,705 13
General Printing and Stationery	746 64
Salaries	1,870 00
Traveling Account	40 39
	$8,783 93

Respectfully submitted,
STEPHEN W. BALDWIN,
FREDERICK M. WHEELER,
Committee.

The Finance Committee, in submitting this report of its sub-committee, would call the attention of the membership to the fact that the expense of the volume which has been just issued has much exceeded the cost of any preceding volume, both in size and in number of illustrations. They would also mention that there is still $542.22 due from members who have not paid what they owe to the Society for the year just closing. It is hoped that the great bulk of this is collectable, but the Council has been notified of this delinquent list, and has directed that the rules in respect to non-payment of dues be enforced in those cases where the non-payment seems to be the result of indifference. There are a number of bills which have been incurred for this meeting which have been audited by the Committee, but do not appear in the above enumeration; others also have not, as yet, been presented, but as these are chargeable to the income of the coming year they are not embodied in the accounting.

Respectfully submitted,
By the Finance Committee.

Mr. Towne.—On behalf of the Council I would like to say, in connection with the report of the Finance Committee that has just been read, that, as emphasized in the report of the sub-committee to that Committee, there is a somewhat serious question before us

as to ways and means. The expenses of last year have been in excess of the income of the Society. That fact is wholly accounted for by the unusual size of the volume just issued, which consists of more than nine hundred pages, whereas in any preceding year no volume, I believe, has exceeded more than half that size. The value of the volume to the membership is beyond question. No volume of equal size and containing material of equal value is purchasable, I believe, in any way, for any such price. The membership pays ten dollars a year, and has in return for that the privilege of attending our meetings, of getting advanced copies of papers discussed there and of receiving this volume. The actual cost of printing, of preparing the engravings, and of binding that volume amounts to more than one-half of the present income of the Society. In other words, for every dollar a member pays into the treasury of the Society, more than fifty cents is expended for printers' and engravers' bills, and it comes back to the member in the book received. A large part of the other expenses—the salary of the Secretary and his assistant, and our office rent and other incidental expenses—are necessarily parts of the preparation of our book, and should be included as part of the cost. It is within bounds to say that, of every dollar paid into the treasury, each member thus receives back seventy-five cents.

The question has been discussed in the Council as to how the deficiency thus arising shall be met, if it is to be a continuing one. If that were the case, of course there is but one answer. The expenses of the Society must be reduced to meet its income, or its income must be increased to meet its present rate of expenses. There is one way, however, in which the question can be evaded, and which we hope may become operative, namely, an increase of the membership. At present the cost of preparing this large volume is borne by a membership of about six hundred. The cost of preparing a volume for a membership of a thousand would be very little more, and distributed over a larger membership, the expenses would be easily borne and met out of our present rate of dues. For this reason and for the promotion of the prosperity of the Society in every way, it is exceedingly important that every member should do his utmost to increase the membership by bringing in desirable candidates for election, and in that way to extend the usefulness of the Society, enable us to maintain our present high standard of efficiency, and maintain also the present perfect manner of preparing and editing our volumes, and yet accomplish all this

without increasing the amount of our annual dues. This can be done if each member will do his best to increase our membership during the coming year by presenting desirable names for election.

The President.—In supplementing briefly what Mr. Towne has so well said, I may add that as the bound copies of the Transactions of the past year have not as yet been presented to the members of the Society, of course very few of those who are to receive them in this form know the extent or value of the volume you are awaiting. Being in our Secretary's office not long since, he said to me that we must either do less talking, and have fewer papers than at our last meeting, or we would bankrupt the Society. I think there are none belonging to the Society who would wish either of those things to be curtailed in the least. Our Transactions are certainly exceedingly creditable, and I am quite sure that there is no member of this Society who would feel that we ought to curtail in that respect. We make it a point, and one of which we have reason to be proud, I am sure, that in so young a society as this, we have in so few years arrived at a point where the published Transactions are not only very large, but very valuable; and it gives me great pleasure to acknowledge how well and how carefully they have been edited. The suggestion made by Mr. Towne is certainly a very good one, that all these increased expenses may be met by a further increase of membership. There are yet abroad in this land many men who, by reason of their experience, knowledge, and mechanical attainments, are eligible as members of this Society. It is desirable that such persons should become members, and every one here knows of at least one or more acquaintances whom he would deem to be entirely satisfactory as a candidate, and it would be well for every one to make of himself a committee of one, for the purpose of soliciting such persons to become members of the Society.

While speaking on this subject, I simply want to say (not because there has been any occasion for any such remark), that in the broad and liberal spirit in which this Society has been organized and carried on, there cannot be any one who would permit any personal spite or ill-will to operate against a gentleman, which would prevent him from voting for him as a member, if otherwise eligible. We certainly want no one as a member who is not properly qualified, but it would certainly be a misfortune if any personal matter should be allowed to come into any election of candidates.

There are many not now members who would be an honor to the Society, and who, I dare say, would be glad to join, if their names were solicited.

The Secretary read the Report of the Library Committee as follows:

Report of the Library Committee, American Society of Mechanical Engineers.

November, 1885.

In accordance with the resolutions adopted at the last annual meeting of the Society, active measures have been taken for the creation of a library in the manner recommended by the report of the Committee submitted at that meeting.

To this end, the Secretary issued a circular to the membership explanatory of the proposed scheme, and soliciting contributions in any of the three following forms, viz.:

(*a*) Special subscriptions, to a permanent fund, of $10 and upwards. (Payable in installments if preferred.)

To this there have been responses from 23 members, whose subscriptions aggregate $613.40.

(*b*) Annual subscriptions of $2.00 payable at the same time as the annual dues.

To this there have been responses from 83 members, whose subscriptions for the current year have amounted to $169, including one subscription of $5.00 per annum.

(*c*) Direct contributions of books and papers relating to mechanical engineering.

To this there have been responses from 3 members, the contributions from whom are enumerated below.

There has thus been subscribed to the Library Fund, during the past year, $782.40 in money, and certain books and papers of value. This commencement is highly encouraging, and indicates that the subject has attracted the interest of the membership, and that it is a question of time only when the Society can be possessed of a library of mechanical engineering equal, if not superior, to any in the country, and of corresponding interest and value to its members.

The method of obtaining funds by means of a small addition to the annual dues, made voluntarily by members desirous of

promoting the growth of the library, has proved highly successful. It is hoped that this plan will still further commend itself, and that the list of annual subscribers to the Library Fund will, before long, include all of the members whose circumstances make it convenient for them to assist in this important matter by the small increase of $2.00 in the amount of their annual subscriptions. The committee would also urge upon the members the importance of sending to the Secretary copies of books, reports and other engineering papers of which the members of the Society may have been the authors, so that the library may include a complete file of all such productions by members. In this connection it may be mentioned that a contribution of any, or all, of Vols. I. to XII., and Vols. XXVIII. to XXXIII., of ENGINEERING, would be particularly acceptable, as completing the file already belonging to the library.

Appended hereto are detailed lists of the contributions to the library during the past year. The Committee begs to call the attention of all members to the value and importance of the work which has thus been so favorably commenced, and earnestly to solicit their continued co operation in the future.

Very respectfully,

HENRY R. TOWNE,
C. J. H. WOODBURY, } *Committee.*
F. R. HUTTON,

CONTRIBUTIONS FOR THE MAINTENANCE OF A LIBRARY FUND.

Bartol, B. H	$10 00
Bauer, Charles A	10 00
Church, Wm. Lee	50 00
Copeland, C. W	50 00
Couch, A. B	100 00
Fritz, John	10 00
Gordon, F. W	10 00
Hand, S. A	10 00
Hollingsworth, S	50 00
Holloway, J. F	25 00
Murray, S. W	10 00
Morgan, Chas. H	3 40
Norman, Geo. H	25 00
Sellers, Coleman	$10 00
Smith, Oberlin	25 00
Stockly, G. W	10 00
Thomas, John	10 00
Towne, H. R	100 00
Townsend, D	10 00
Ward, W. E	10 00
Whitaker, Ezra	15 00
Wood, Walter	10 00
Woodbury, C. J. H	50 00
	$613 40

DUES INCREASED ANNUALLY BY $5.00 FOR THE BENEFIT OF THE LIBRARY FUND.

Robert H. Thurston.

MEMBERS WHO HAVE INCREASED THEIR DUES BY $2.00 FOR THE MAINTENANCE OF THE LIBRARY FUND.

Babcock, Stephen E.
Bailey, R. W.
Baldwin, William J.
Bergner, Theodore.
Bond, George M.
Booraem, J. V. V.
Boyd, James T.
Brown, Alexander E.
Burdsall, E., Jr.
Bushnell, R. W.

Capen, Thomas W.
Cheney, Walter L.
Church, William Lee.
Cole, J. W.
Copeland, George M.
Cushing, George W.

Danforth, Albert W.
Davis, E. F. C.
Day, F. M.
Deane, Charles P.
DeSchweinitz, P. B.
Dixon, Charles A.
Dobson, W. J. M.
Donovan, William F.
Duncan, John.
DuVillard, H. A.

Edison, Thomas A.
Emery, Charles E.

Francis, W. H.

Gardner, E. LeB.
Geer, James H.

Halsey, Fred. A.
Hawkins, John T.
Henning, G. C.
Hewitt, William.
Hillman, Gustav.
Hoadley, J. C.
Hobbs, A. C.
Hollingsworth, S.
Holloway, J. F.
Hugo, T. W.
Hunt, R. W.

Johnson, C. R.
Johnson, Lewis.

Kirby, Frank C.
Knight, Charles A.

Lipe, Charles E.
Loiseau, Emile F.

Mason, William.
Mattes, W. F.
Moffat, E. S.
Morgan, T. R., Sr.

Nason, Carleton W.

Patton, William H.
Payne, David W.
Perry, W. C.
Porter, Charles T.
Porter, H. F. J.
Pratt, F. A.

Randolph, L. S.
Reed, Edward M.
Renwick, E. S.
Robinson, George H.
Robinson, S. W.
Roby, Luther A.
Root, John B.

Scheffler, F. A.
Schwamb, Peter.
Scranton, W. H.
Smith, Oberlin.
Smith, William F.
Soule, R. H.
Sperry, Charles.

Tabor, Harris.

Upson, L. A.

Vanderbilt, A.

Waterman, J. S.
Webster, Hosea.
Weeks, George W.
West, Thomas D.
Wheeler, Herbert A.
Whiting, S. B.

CONTRIBUTIONS OF BOOKS TO THE LIBRARY.

From Charles W. Barnaby, Salem, Ohio :
G. Weissenborn's American Engineering, complete.
From John H. Cooper, Philadelphia, Pa. :
Engineering, volumes 13 to 26, bound.
" " 27 and 28, unbound.
Engineer and Machinist's Assistant. 2 volumes.
Railway Locomotives. 2 volumes. D. K. Clark.
American and European Railway Practice. Holley.
Catechism of the Steam Engine. Bourne.
Hand-book of the Steam Engine. "
The Steam Engine, and Machinery and Mill-work. Rankine. 2 volumes.
Technical Dictionaries, English, French and German. 3 volumes.
Weisbach's Mechanics of Engineering.
Engineering Precedents. Isherwood.
Life of Fulton. J. F. Reigart.
Mill-Geering. Thomas Box.
Boiler Chimneys. Wilson.
Compound Engines. Turnbull.
Boiler Explosions. Robinson.
Land and Marine Engines and Boilers (plates). Burgh.
From Thomas D. West :
Modern Foundry Practice.
The Moulder's Guide.
From Local Committee of the Boston Meeting :
King's Hand-book of Boston.

LIST OF EXCHANGES.

Zpravy Spolku Architektu a Inzenyru. Prague.
Ingeniors-Foreningens Forhandlinger. Stockholm.
Mining Institute of Scotland. Hamilton.
North of England Institute of Mining and Mechanical Engineering. Newcastle.
Institute of Mechanical Engineering of Great Britain. London.
Institute of Civil Engineering of Great Britain. London.
Institute of Civil Engineering of Ireland. Dublin.
The Mechanical World. London.
Engineering. London.
The Engineer. London.
Iron. London.
Electrical Review. London.
Master Car Builders' Association. New York.
Engineers' Club of Philadelphia. Philadelphia.
Engineers' Society of West Penn. Pittsburgh.
United States Naval Institute. Annapolis.
Franklin Institute. Philadelphia.
American Society of Civil Engineers. New York.
American Institute of Mining Engineers. New York.
Associated Engineering Societies. New York.

American Journal of Railway Appliances. New York.
Electrical Review. New York.
Chicago Journal of Commerce. Chicago.
Boston Journal of Commerce. Boston.
Industrial World. Chicago.
American Engineer. Chicago.
Manufacturers' Gazette. Boston.
American Machinist. New York.
Mechanics. New York.
Railroad Gazette. New York.
Engineering and Mining Journal. New York.

The Report of the Tellers being next in order, that report was read as follows by Mr. Hand:

REPORT OF THE TELLERS.

The tellers appointed at the meeting last evening have to report that they have finished the duties assigned them, and report the ballot as follows. Whole number of votes cast, 324:

PRESIDENT.

Coleman Sellers	322
Scattering	2

VICE-PRESIDENTS.

Olin Landreth	313
Horace See	330
Charles H. Loring	331
Allan Stirling	306
Scattering	2

MANAGERS.

Hamilton A. Hill	313
William Kent	318
Samuel T. Wellman	334
Scattering	2

TREASURER.

William H. Wiley	322
Scattering	2

S. Ashton Hand,
Ambrose Swasey,
Tellers.

After a few pleasant words by President Holloway, expressing his pleasure in the result of the vote, and his regret that the President-elect was not at the session, some further announce

ments were made and the reading and discussion of papers was begun.

Professor John E. Sweet, of Syracuse, read the first paper on "The Unexpected which Often Happens."

Messrs. Towne, Bond, Durfee, Rogers, Hammond, Ashworth, and O. Smith took part in the discussion.

Mr. Samuel Webber, of Lawrence, presented the next paper on "The Frictional Resistance of Shafting," and Messrs. Henthorn, O. Smith, Babcock, Schuhmann, and Towne spoke on the subject.

The Secretary read Mr. Barrus's paper on "A New Form of Calorimeter." Messrs. Babcock, Lanza, Thurston, and Rogers participated in discussion.

After some announcements by the Secretary the session adjourned.

The afternoon was devoted to an excursion to the Improved Sewage Works of Boston, tendered to the Society by the City. Conveyances bore the party to the steamer *J. Putnam Bradlee*, at the wharf near the South Ferry, and they were thence taken first to the pumping station, where the Leavitt and Worthington pumps are installed. After an hour here the steamer went over to Moon Island, where are the reservoirs in which the sewage is stored, to be released only on the ebbs of the tides. The trip concluded by a complimentary dinner, tendered also by the city, at the Parker House. Mayor O'Brien presided, and a number of city notables were in attendance. This excursion, not being particularly congenial for the ladies of the party, they were entertained at lunch and for the afternoon at the house of Mrs. General Francis A. Walker. The time of the evening session was somewhat curtailed by the formalities of the dinner, but time was found for the joint paper by Messrs. Trowbridge and Richards, entitled "The Rating of Boilers by Horse-Powers for Commercial Purposes." Messrs. Babcock, Kent, Webber, and Thurston took part in its discussion.

After announcements the session adjourned at a late hour.

Third Day, Thursday, November 12th.

The Society convened again at half-past nine. The Secretary read a series of resolutions passed by the Council at a recent meeting to expedite the presenting of papers. They were as follows:

"*Resolved*, That to facilitate the discussion of papers and the dispatch of business the following rules are adopted :

1. That members speaking in reply to papers shall have priority to the floor in the order in which they notify the Secretary of their intention to speak.

2. That members who have reduced their remarks to writing shall be entitled to not exceeding ten minutes at one time, and that all others speaking in discussion shall be limited to not exceeding five minutes at one time.

3. That no member having thus had the floor shall again claim it until all others who desire to speak shall have had opportunity to do so."

It had been decided to spend an hour at this time in Topical Discussions. Ten of these topics were brought up, and received discussion by Messrs. Emery, Church, Towne, Lanza, Durfee, Bancroft, Bond, Stetson, Powel, Soule, Baldwin, Kent, Schuhmann, Phillips, and O. Smith.

At the expiration of the hour the Secretary read a paper by Mr. Thorne, of Philadelphia, on "Twist Drills." Messrs. Stetson, Hawkins, O. Smith, and Bond took part in the discussion. The paper by Mr. F. E. Galloupe, of Boston, on "Rapid Transit and Elevated Railroads," giving a description of the Meigs Elevated Road, was next read, and was discussed by Messrs. Durfee, Kent, Halsey, Schuhmann, and Hutton, and the paper on "The Basic Bessemer Process," by Professor Thomas Egleston, of New York, was discussed by Messrs. Holloway, Kent, and Durfee.

At this point Professor Lanza presented his two papers, on "The Course in Mechanical Engineering at the Massachusetts Institute of Technology," and on "A Series of Experiments on the Transmission of Power by Belting." He also presented Professor Peabody's paper on "Steam Engine Tests" in the laboratory of the Institute of Technology. These received no discussion, but were presented previous to the hour assigned for a visit to the mechanical laboratories of the Institute. An hour had been chosen at which the students would be engaged in their regular work there, and the methods and appliances were examined with interest.

The President, in calling the session to order at three o'clock, presented a telegram announcing that illness would prevent Prof. Rogers from presenting his paper on "The Microscope in the Workshop." It was presented by title only, and the microscopic caliper was examined afterward by those interested, but without exhibition. The second paper was that of Mr. William Hill (Collinsville, Conn.) on "The Crystallization of Iron," which had printed discussions by Messrs. Hutton and Thurston, and Messrs

Egleston, Hawkins, Durfee, Sanderson, Webber, Kent, Nicholson, Sweet, Cole, Stetson, Crouthers, and Harrison took oral part.

Mr. George M. Bond, of Hartford, presented a paper on "Standard Pipe and Pipe-Threads," illustrated by samples to show the necessity that some action be taken to correct the present state of divergence in practice. Messrs. Schuhmann, Grinnell, Baldwin, and Stetson spoke in the discussion, and finally Mr. Kent made the following remarks:

Mr. Kent.—Reference has been made to the difficulty of connecting the American system with the English. I would mention the fact that in the experience of the Babcock and Wilcox Company they had to send a complete set of pipe-fitter's tools to England for the use of their European branch. They find that the standards in England and on the Continent are worse than they are in this country in regard to irregularity. I do not know that we can help the matter by discussing it any further. As I have had a sad experience recently as a member of one committee of the Society, and as I would like to see somebody else subjected to the same punishment, while declining to serve on a committee myself, I move for the appointment of a committee to consult with pipe-makers and users, and the makers of pipe-taps and dies, and investigate the subject of standard pipe-threads. The committee might correspond with those similarly interested in foreign countries also, and may be enabled thus to report on a standard system which may be adopted in all English speaking countries.

The motion was duly seconded by Mr. Oberlin Smith as follows:

Mr. Oberlin Smith.—I heartily second this motion. I had considerable experience in a pipe mill in my younger days, and I know the evils of the present non-standards. I do not think we can do a more important work than to go on in this line, and I suppose the first thing is to appoint this committee. I suppose they should deal with oil pipe and brass pipe while about it. All we can do now undoubtedly is in our own country.

The President.—Will Mr. Kent make a suggestion as to how this committee should be appointed?

Mr. Kent.—The committee, I think, should be appointed by the chair, but I would suggest that it should include men representative of pipe manufacturers and of pipe users, with perhaps one representative of the sprinkling system and certainly one of the manufacturers of taps and dies.

The motion was then put and carried. The President subsequently announced the committee as follows:

Mr. Frederick Grinnell	Providence.
" George Schuhmann	Reading.
" Wm. J. Baldwin	New York.
" B. H. Warren	Boston.
" Geo. M. Bond	New York.

At this point Prof. Egleston, Chairman of the Committee of the Society on Uniform Methods of Test, presented his report as follows:

Prof. Egleston.—The Committee has been actively at work since its appointment a year ago and wishes now to report progress. In a very short time we shall have prepared a uniform method of recording tests that will be proposed as a tentative one for reporting all tests. Your Committee has made a large number of experiments with a view of determining what is the best method of testing and also the best method of making testing-pieces, but they are not yet prepared to report on the subject. The Committee hope soon to report the tentative method of uniform recording of all test observations. We find on discussing this subject with those persons who test in this country and abroad, that there is no uniform method of recording operations, and that the first thing required to be done is to have such a method. Your Committee therefore beg to report progress and to ask to be continued.

Prof. Egleston.—The Committee on the United States Testing Commission wish to report that, as they reported last year, the bill has again failed to pass Congress, but that they are not discouraged. It is a very curious thing that while a very large majority of all the members in the House and Senate are in favor of this bill, because, as the members say to me repeatedly, there is no politics in it, it cannot be brought before the House. Your Committee is now endeavoring to get some politics in it, if possible, and hopes by this time next year to be able to report considerably more progress.

The hour being already late, the paper by Mr. Wilfred Lewis, of Philadelphia, on "Experiments on the Transmission of Power by Gearing," was read in abstract and discussed by Mr. Towne, and the paper by William Cowles, of New York, on "Improvements in Ferry-Boats," was also presented in brief. The paper

by Wm. J. Baldwin, entitled "Notes on the Comparative Values of Metal Surfaces for Warming Air," was presented by title only.

In the evening of Thursday, a reception was tendered to the Society by the Boston Art Club, in the club house of that organization. It was a brilliant and successful affair, and much appreciated and enjoyed.

Fourth Day, Friday, November 13th.

This day was devoted to excursion. A special train, tendered by the courtesy of the Boston and Lowell Railroad, left the station at 9.30 for East Cambridge. A stop was made here for an hour at the works of the Meigs Elevated Railroad Construction Company. Here a model was exhibited and run, and an inspection was given to the full size engine and car now being built, and to the experimental line of track. The party were taken thence to Lawrence, where they visited the pumping station at the Waterworks, and looked at the Leavitt engine, and also at the dam of the water-power. They adjourned to the Franklin House for lunch, preparatory to their visit to the Pacific Mills. Before separating at the close of the meal, President Holloway called the assemblage to order, and introduced Mr. Horace See, who presented the following series of resolutions. Each of them was passed by the meeting with a unanimous vote of acclamation.

His Honor The Mayor, and the City of Boston.

The American Society of Mechanical Engineers, assembled in Sixth Annual Convention, feel that they are under no ordinary obligations for courtesies extended to them by the City of Boston through its Chief Magistrate and the Joint Committee of both branches of the city government.

We appreciate the honor of the presence of the Chief Magistrate at two of our gatherings, and recognize the value of the unusual opportunities afforded us to inspect the City Sewage System, which we believe to be the most conspicuous example in the world of civil and mechanical engineering, jointly applied to the solution of a problem of such paramount domestic and public importance as the successful drainage and sanitary improvement of a great city. That such complete entertainment should have been supplemented by your most generous hospitality, and an opportunity to meet the representative men of the city government, merits an especial recognition, and the acknowledgment of our most hearty thanks.

The Trustees and Faculty of the Massachusetts Institute of Technology.

The American Society of Mechanical Engineers return thanks for courtesies extended during their Sixth Annual Convention in the City of Boston.

The Society recognizes the fitness and significance of their being permitted to convene under the roof of an institution which, while training men for many liberal professions, does not fail to accord to our chosen profession of Mechanical Engineering a prominent position.

We have realized profitable enjoyment from our examination of your apparatus, equipment, and methods of instruction, and from meeting with your president and professors.

The Society appreciates the privileges which have been extended to it, and desires to acknowledge their sense of obligation.

The Boston Art Club.

Gentlemen :

The American Society of Mechanical Engineers, having enjoyed the hospitality of the Boston Art Club, desires to record its appreciation of the privileges so generously extended to its members.

The selection of Boston as the place of our Sixth Annual Convention has been happily justified by each and every of the many and profitable and enjoyable circumstances of our entertainment here, but the reception tendered by the Boston Art Club has signalized this meeting as one of memorable interest and profit.

The members, both for themselves and on behalf of the ladies accompanying them, return most cordial thanks for privileges of such an exceptional nature, so liberally offered, and so keenly appreciated

The President and Directors of the Boston and Lowell Railroad Co.

The American Society of Mechanical Engineers feel that among the many courtesies extended to them during their Sixth Annual Convention in the City of Boston, that of your corporation in generously providing a special train for our use to and from the city of Lawrence, has made us indebted to you for a very enjoyable and profitable trip, and trust you will accept our most hearty thanks.

The Pacific Mills.

Gentlemen :

The American Society of Mechanical Engineers desires to thank the management of the Pacific Mills for the privilege of visiting their justly famous factories in the city of Lawrence. We esteem it a grand opportunity to be thus able to compass in one general survey the several successive processes of the manufacture of textile fabrics. We appreciate our opportunity and stand indebted to those who have accorded the privilege.

The American Society of Mechanical Engineers acknowledge the receipt of the kind invitation of the Trustees Boston Museum of Fine Arts; Trustees Boston Natural History Rooms; Meigs Elevated Railroad Construction Co.; Major Francis H. Parker, U. S. A., Commandant U. S. Arsenal, Watertown; Professor Ed. C. Pickering, Director Astronomical Observatory, Harvard University; Mr. B. F. Sturtevant and a certain number of gentlemen have invited us to visit their specialties while in the city, and thank them for the courtesies extended.

The American Society of Mechanical Engineers take great pleasure in thanking Mrs. Francis A. Walker for the courtesies extended to the ladies accom-

panying us to Boston, and inform her how much her untiring efforts as a hostess have contributed, more than anything else, to make their visit not only the most enjoyable, but one not soon to be forgotten.

At the close of the foregoing resolutions, Mr. See read the following:

One of the finest pieces of engineering that has come under our favored notice in the charming City of Boston, has been that of Mr. C. J. H. Woodbury, and the Local Committee.

They have solved the problem how some 400 feet of mechanical engineers can be successfully run at varying speeds in a city with bearings very much out of line, with the least possible amount of friction, say one half of one per cent.

We cannot help thinking, however, that something more than the Keeley Motor must have been used to keep it going, with the help of the finest and most costly lubricants.

We believe they should receive most honorable mention, and be recommended as those who can be found, in the judgment of the Council, as it stands in our rules, competent to take charge of work in their several departments, and therefore eligible to full membership.

These having been put and passed, Mr. Woodbury made a brief reply of recognition, and the members separated to make their visit to the Pacific Mills. This enormous establishment had conferred on the Society the very unusual privilege of a visit through their buildings, and the party was divided up and escorted by various representatives of the Company. The course of the visit was most admirably planned by itinerary, and no time was lost. More than two hours and a half were spent on the visit, which took in every part of the establishment. A tired party re-assembled on the train to return, and on the return to Boston the gathering broke up.

CLXXXVII.

PRESIDENT'S ADDRESS, 1885.

BY J. F. HOLLOWAY, CLEVELAND, OHIO.

Among the various duties devolving upon the President of this Society, is that of saying something at the time, and on the occasion of his retiring from the position of honor in which you had previously placed him. With no hope of making up in parting words for any of my previous shortcomings, I accept this duty now devolving upon me, and shall attempt briefly to say something about the Mechanical Engineer, his position and mission.

Brief as is the history of this Society, it has already been shown that there was a necessity for its organization and existence, and the rapidity with which its ranks have been filled up, and the character and standing of its members, alike show how widely felt was the want of some nucleus about which the members of our profession might cluster. Of the advantages which in the future will grow out of the formation of this Society it is yet too early to speak; but the benefits which have come to us all by reason of our connection with it, by reading its published Transactions, by attendance at its meetings, by participating in its discussions, and, above all, by the pleasant acquaintances made at its social gatherings are, I am sure, not only apparent, but are fully appreciated by all those whose good fortune it has been to join in them. The term Engineer, is a generic one, capable of being divided into a great many subdivisions; just how many it is difficult to say, as the necessities of civilization, and the advancement of science have made it necessary to add new specialties, each of which is of sufficient importance to occupy the full time and energy of a special engineer in order to master and direct its details.

While I would render honor to all branches of the profession of engineers, and would most certainly underrate none, I cannot but believe that the Mechanical Engineer stands the peer not only of any other engineer, but of any other man as well, in depth and breadth of usefulness, and for the reason that for the final consummation, and for those final practical results which best promote

the comfort, safety, and progress of humanity, as well as by reason of the fact that he, most of all, is concerned in the moulding and directing of the material forces of nature. It was well and truly said by one of the founders of this Society, that the profession of mechanical engineering underlies all engineering, and it requires but a moment's thought to make this apparent.

While the civil engineer may plan and locate his railways, canals, aqueducts, bridges and tunnels, it is the mechanical engineer who is called upon to devise the machinery and tools with which they are to be constructed. The naval or military engineers, guided by the experiences of the generals and warriors of the past, may plan defenses by land or sea, may design forts or iron-clad ships, but when their resources are exhausted, it is the mechanical engineer who is called upon to build the ships, load them with defensive armor, and not only to provide them with powerful guns, but also to provide the mechanism by which not only the guns are to be handled, but by which the ship itself shall be manœuvered and driven alike through stormy as well as calm seas.

As an example of the fact that in the prosecution of modern warfare, the mechanical engineer has become a most important factor, it is said that during the progress of the late Riel rebellion in the country adjoining our north-western boundary, one of our engineering firms who manufacture a rapid firing multiple gun, sent an agent with one of their guns to the scene of strife, with a view of extending their trade, hoping that, owing to the scarcity of troops and the difficulty of transporting them up there, they would find a market for their machine gun. The agent arrived in camp just at the time when, after a good deal of marching and counter-marching, the belligerents were at last by design or accident brought face to face. With the push natural to an American, the representative of the firm was early on the ground, and running his machine far in advance of the regular troops, he unlimbered his gun, and grasping the crank, devoted himself to the task of showing what a mechanical soldier could do; and the account said, that, unmindful of the storm of bullets that flew about him from the opposing foe, he turned his crank with such unceasing energy that he actually won the battle before the regular soldiers had a chance to fire a shot. While this incident serves to illustrate the value of the mechanical engineer, and his contrivances in modern warfare, it is only proper and just to say, that the success in this case

was largely due to the pluck and industry of the American drummer who turned the crank.

How helpless would sit the mining engineer above his earthbound treasures, be they never so rich, did not the mechanical engineer come to his aid to lift them from out their rock-ribbed home, and with fiercely fanned furnace fires, free them from their impurities and dross.

Even the giddy goddess Fashion, whose ways none can fathom, would like Alexander sigh in vain for other worlds to conquer, did not this same mechanical engineer, like gallant knight of olden time, come to her aid, and with ingenious and studied transformation of wheels and cams, and flying shuttles, bring new combinations to woof and warp, whereby the silken threads turned into new channels, reveal a hitherto hidden beauty, which for the time captivates all beholders. And so it is through all the industrial pursuits of life which require for their prosecution the handling and the transformation of crude materials into forms of usefulness and beauty.

While it is true that scientific and technical training is, and must ever be of great advantage to the mechanical engineer, there is yet another source from which, after all, he will derive by far the most benefit, and that is—experience. Not necessarily his own experience, but the experience of others, and of all ages as well. And I know of no other way in which he can be so benefited and aided all through his life.

I ever think of experience as of a vast storehouse, the cornerstone of which was laid upon that day when the unbarred gates closed behind our first parents as they left the paradise of Eden, a storehouse to which each successive day since that time up to yesterday, has added extended length and increasing stores. In this vast edifice every man of every occupation or profession has had his nook in which to pile up the experiences of his life. Of the great mass of knowledge therein stored, hidden by the accumulated dust of ages, little has come down to the present, but who does not regret that more cannot be known of the members of our own profession who in remote ages builded so well that even the ruins of what they accomplished challenge our admiration now? What an addition it would be to the literature of engineering, could we have the clearly interpreted published transactions of those engineers, who, thousands of years ago, built a Suez Canal through a country in which in our day

De Lesseps could do no better than to follow. Could they be spread before us how much of interest would cluster about the discussions of the mechanical engineers who planned the Pyramids, and contrived and executed plans for quarrying, transporting, and erecting those numerous obelisks which once dotted the plains of Egypt? Who would not, were it possible, scan with interest the drawings and plans of those engineers of ancient Carthage, who, when Europe was fringed only on its southern limit with civilization, came over to Spain, built pumps, drained, and worked mines opened ages before by other and to them unknown engineers.

The steady onward tread of those Roman legions, which once made Rome mistress of the world, was made possible by the labors of the sagacious and skillful engineers who built for them imperishable roadways, spanned rivers with bridges which are marvels to-day, and who laid so deep and so permanent their foundations in the then far off "Isles of the Sea," that the engineer of the present, as he excavates for his railways and buildings, often reaches the imbedded stone or crowning arch laid by his professional brother thousands of years ago.

But, not alone would we be interested in knowing something of the experiences of those far off engineers, who in the dim past accomplished so much with so little; we would as well be greatly interested, and greatly enlightened could we know more of those who nearer to our own time have accomplished so much for the benefit of the world at large. While it is true that we may go to that garret in Heathfield, where he whom we are pleased to call the father of the steam-engine, James Watt, lived and labored, and while we may stand beside the bench whereon he worked, may see the lathe which yet has in it the unfinished job he left there, and beside which his leathern apron lies as last he laid it down, and while all these from their associations would be of especial interest to us all, who know how much the onward progress of the world was hastened by what he accomplished for the steam-engine, there will still remain much which we shall never know of James Watt and of his experiences. None will ever look upon James Watt's scrap heap; none can do more than imagine of his studying, his planning, his model-making, his trials, and his failures, as he worked out the problems of expansion, contrived the condenser, air-pump, valve motion, governor, indicator, and, indeed, every important feature of the successful steam-engine of to-day.

That he had a scrap pile which received his failures, we may well believe, for where is the engineer who has accomplished anything worthy, who has not hidden away in some secluded nook or corner, his pet schemes that came to naught, his long-dreamed of hobbies which some inexorable law of nature defeated, his neglected but never quite relinquished models, which were to work wonders, and over which at long intervals he sits and dreams, and promises himself at some time in the future to take in hand and work out to a successful ending. That level-headed, canny Scotchman, George Stephenson, who drove the "Rocket" at Rainhill on that September day in 1829, and whose practical good common sense and industry did so much to make the railway and locomotive of his day a success, has hidden away in his lock-up, in the vast storehouse of which I have spoken, several need I say unfinished models of perpetual motion.

It is said of the late Matthew Baldwin, one of the earliest and best known builders of locomotives, one whose practical good sense, industry, perseverance, and long-accumulated experience, contributed more perhaps than those of any one else in bringing the American locomotive to its high degree of efficiency, that all through his life he was haunted with an idea that a rotary engine could and ought to be used for propelling a locomotive; and, no doubt, that in his pencil sketchings, among his models, and in his scrap-corner during his life might have been seen here and there the outcrop of a hobby which in his dreams and in his leisure moments rose up before him like Banquo's ghost, and which like it, in spite of all his failures, "would not down."

Since time was young, it has been the mechanical engineer, working often in the most obscure manner and in the most humble circumstances, who in the end pushed rapidly forward the car of progress.

While it is possible, by the aid of history or tradition, to trace back to their origin some of the innumerable inventions which have so benefited the world, how many yet remain about which nothing is now, or ever will be known? Think for a moment of that vast host lying in unknown graves, who, in the past, amid poverty and discouragements, toiled from early morn until the night was well spent in some obscure garret, or smithy, or workshop, to devise and perfect some simple machine, to contrive or manufacture some article now so common, so universally used, that its sudden removal would work a world-wide injury. While

none will question the value of the engineer in aiding the progress of the past, all will, I think, agree that at no time in the history of the world was he so important a factor as he is to-day.

Standing in a city whose enlarged culture and great wealth was made possible by the labors of the mechanical engineers, or, indeed, is their direct result, who, by their skill and industry have filled all the valleys of New England with flying spindles and busy wheels, whose ingenuity has relieved all labor of its drudgery, and whose inventions have given their country a world-wide renown, I need not, I fancy, speak at large of the mission of the mechanical engineer, or of what he has accomplished.

A stranger, standing in the midst of the glories of the Cathedral of St. Paul, asked to be shown the memorial or monument of the builder. He was told: "Would you see the monument of Sir Christopher Wren—gaze about you." So answer I: Would you know the mission of the mechanical engineer—gaze about you wherever in the civilized world you may be.

Putting aside all the wondrous history of by-gone ages, blotting out all the engineering triumphs of the Middle Ages, calling the time of Shakespeare and Milton antiquity, and coming down to within a brief century, or, if you will, to the brief span of a single generation, and what, or rather what not, has the Mechanical Engineer accomplished? So broad, so varied, and so universal has been the field of his action, and of his triumph, that I find no time to begin even an enumeration of what he has done. A thoughtful woman gazing at the ceaseless, steady action of an immense engine, driving with tireless speed the ponderous steamship through storms and calms, said half in soliloquy, but wholly in earnest, "Engineers ought to wear crowns." While it is true, that at least in this country, the position of any man or set of men will be what they themselves make it, it is nevertheless a fact that the public at large are wont to accord eminence and praise to those most in view, and oftenest heard. In the history of the olden time it was the kings and rulers who occupied much of the attention of the public: later on, and for a much longer period, it was the chieftain, or warrior, whose praises were sounded by the orator and poet, and the prowess, the bravery and chivalry of the Knight Errant, surrounded with environments of waving pennons, floating banners, and glittering armor, was the theme of poetry and song through many ages.

Hannibal and Napoleon, with their armies crossed the Alps.

Their pathway amid the clouds was strewn with thousands upon thousands of those who fell by the way, and of the proud armies who at their base began the ascent, comparatively few reached the sunny plains beyond. They had indeed crossed the Alps, but behind them their snowy peaks defiantly still touched the clouds, unharmed, unconquered. Long years after these warrior chieftains had crumbled into dust, another, mightier than Hannibal or Napoleon, came to the foot of these rock-barred barriers, which for ages past had defiantly said alike to king and peasant, "Pass me only at your peril." With no long retinue of soldiers, no vast caravan of horses and elephants and bleating herds, no waving banners, no blare of trumpets, or cry of herald, came the conqueror of our day. Seeing the weary traveler and burdened beast climbing with patient toil along the narrow pathways, and about the storm-beaten crags half hidden in the clouds above him, and beside yawning gulfs no eye could fathom, he bethought him of a better way. Seeing near by a dashing torrent, which for ages had unconfined tossed high in air its flakes of foam, he knew he had in that a giant force to do his bidding; so, curbing and guiding its wild spirit, he bade it turn his wheels, swing his cranks, move to and fro his ponderous plungers and pistons. He made swinging valves at each opening stroke, to gather in that wildest, freest, and most untamable of all elements, one of which it was said long, long ago, "It bloweth where it listeth"—the air—and driving it before him through long and tortuous passages, he made it to knock with arm of steel upon the portals of the hitherto impenetrable walls. You know the rest. With highest skill, and unceasing industry, he bade the spirits of the air and water alike to do his behest, guiding them to the right and to the left, up and down as occasion required, and from either side through months and years, until at last the hitherto defiant walls were broken down and the eager men leaped through the rent mountain, not to engage in deadly conflict or savage hate, but to clasp each other in hands of equality and fraternity. Through this open portal, to-day on roadway of steel, luxurious carriages glide swiftly and securely, filled with the inhabitants of all lands on errands of pleasure, peace, and good-will. Other vans loaded with the products of the field and vine, the spindle and the loom, alike pass from Italy's sunny plains to the north land of snow and ice, for barter or exchange.

Need I ask who conquered the Alps; the soldier or the engi-

neer? Need I ask whose triumph has contributed most to the welfare of all the world, the generals who went over the Alps, or the engineer who went through them? Let Mont Cenis and St. Gothard answer.

The day of the professional warrior, the swash-buckler of camps, whose sword was on hire for any cause, has passed forever, and he is fast dropping out of sight. That there are, and it is to be hoped ever will be, men who for their country's defense, and for the cause of right and justice, will peril their lives on the field of battle, we fully believe. It was such a one who, but a short time since, was borne to his last resting-place, followed by a nation in mourning, and for whose requiem, tolling bells beginning with the early dawn upon our eastern coast, awakened new echoes in every town and hamlet across the broad continent, until with the setting sun they died out amid the murmuring waves of the Pacific. As the comrades in arms of this great man came together to devise some memorial that should perpetuate his worth and keep his memory green, with one accord they said, let not his monument be of broken swords and captured guns, with all their attendant memories of conflict and strife, but rather let the sculptor with his highest art and best skill, transfer to the enduring marble, the semblance of the great citizen sitting in the shadow of his home, and under the shadow of an impending fate as well, facing that death calmly which, baffled on a hundred battle-fields, now held him within easy reach, tortured with a pain we can but at best imagine, using his last hours and waning strength in writing a memoir; not to place himself higher on the rolls of fame, or in the hearts of his countrymen, but to leave behind him comfortable sustenance for the family he so much loved, and from whom he was so soon to part. This sentiment of his comrades, the officers and soldiers of the Grand Army, and which so accords with the feelings of all who admire the kindness of heart, the inflexibility of purpose, and the absence of all vindictiveness in that great warrior, and greater citizen, General Grant, illustrates most fully the change in public sentiment of which I have spoken, and illustrates, as perhaps it never so well has been illustrated, the truth of the saying that, "The pen is mightier than the sword." *And let me add,* that among the pens that have accomplished mighty results for the progress of civilization, liberty, and equality, is the drawing pen of the mechanical engineer.

There remains but one more thought to which I beg your in-

terest and indulgence, and that is the position of the mechanical engineer with regard to his fellows. It is not many years since the mechanical engineer knew far more of the men of all other professions than he did of his own. It was not many years ago that the leading members of our profession knew little of each other except by reputation, and I disclose no secret when I say that there was little endeavor made to do away with the exclusiveness and distrust, to use no harsher term, which to a certain extent was prevalent then. Happily all this is changed now. The formation of this and similar societies, the frequent meetings and conferences held, the comparison of methods, the recitals of our successes, and the avowals of our failures, together with the knowledge that has come to us all by reason of this interchange of views and experience, makes us all to feel that we live in a wiser and better age.

But not alone in a professional way have we been benefited by these gatherings. The harmonious union of men standing high in the attainments of their profession, high in the regard of the public; representative men in communities spread all over the land, by such meetings as this, held in the important industrial cities of our country, in each of which their best citizens have come forward to greet us with most cordial welcome, there has been shed a new luster upon the profession of mechanical engineering, and it has been raised to a still higher plane of usefulness and observation. But better than all, by these meetings we have come to know each other, as in no other way would it have been possible. In this acquaintance we have been taught a broader charity for each other's failings, we have come to know something of each other's trials and misfortunes, and to find on every side warm hearts, filled with generous impulses ready as well to give as to receive.

Let it be our ambition so to conduct ourselves, that we shall ever honor our calling, and shall assist in raising it in the estimation of all, to that high position of honor and usefulness to which, by reason of what it has done and is doing for the world and humanity, it is so justly entitled.

CLXXXVIII.

THE BASIC BESSEMER PROCESS.

BY T. EGLESTON, PH.D.

In the year 1878 a paper on dephosphorizing iron was presented to the Iron and Steel Institute, of England, which was considered to be of so little importance that it was withdrawn by the authorities of the Institute before it was read to that body. This paper was written by Messrs. Thomas and Gilchrist, and was published as part of the proceedings of the Institute, by mistake, in *Engineering*. Notwithstanding this reception of it, it was not long before it began to attract the attention of the iron and steel manufacturers. This is the history of the introduction of what has proved to be the most important improvement in the treatment of iron ores and pigs which had before that time been considered too impure for use. The amount of output of steel made from material previously considered unfit for use, in 1882, the fourth year after the introduction of this improvement, was 400,000 tons, and during the year 1885 will probably amount to over a million tons.

Having had occasion to study this process in Europe in the years 1882 and 1884, and again in 1885, and as it has been adopted to a very slight extent only in this country, I have thought it worth while to present to this Society the results of my examination of the process abroad. My study was confined to three works on the continent, and four of the largest and most recently constructed works in England. I shall not attempt to describe in detail any one establishment, but shall simply give the outline of what seems to me the best practice in each. I have already described at length the various processes of making the basic refractory materials,* and shall discuss in this paper only the manufacture of the steel, which is of the greatest possible interest to this Society, as it shows conclusively how necessary mechanical engineering has been and still is to the success of this process, and what a wide field there is in metallurgy for the employment of mechanical engineering talent.

* Trans. Am. Inst. Min. Eng., May, 1885.

There are several types of these works. Those on the continent have generally been adapted from acid works. In Great Britain, the basic part of the works of Bolckow, Vaughan & Co., at Eston, which were the experimental ones for all England, and the school for all the others, were built first. The works of the North-Eastern Company and Glengarnock were constructed especially for the basic process, having the benefit of the experience gained at Eston; while those of the South Staffordshire Steel & Ingot Iron Co., though recently constructed, are composed for the most part of the different parts of a dismantled acid works. Most of the works are planned with a view to taking the iron direct from the blast furnace. They are all constructed so as to use cupola metal if necessary; some of them use a mixture of the two. The object of all is to produce ingot iron, or the very soft varieties of steel, as well as rail steel, from phosphoric pig. The works of Bolckow, Vaughan & Co., at Eston, are those in which the process may almost be said to have originated. Its success has been largely owing to their wise foresight in recognizing the importance of the process, and their large-minded liberality in allowing the public to have the benefit of their experience. Theirs were the first works built, and they should not therefore be criticised from the point of view of our knowledge of to-day. They use the iron direct; have six basic 15-ton and four acid 8-ton converters; and employ both processes, making about 5,000 tons per week when running full.

The works of the South Staffordshire Steel & Ingot Iron Company are situated about two miles from Wolverhampton, and are to be connected with the Spring Vale blast furnace, so as to use the iron directly from the furnaces. They were completed in the spring of 1884, and are yet in the trial stage. The plant was purchased for the most part from the Mersey Iron Works, of Liverpool, when that establishment was dismantled. It is an acid plant adapted to the basic work. The plant is composed of three converters, with one in repair, three cupolas for melting the iron, two cupolas for burning the dolomite, and trains with reversible engines for making heavy plate.

The works of the North-Eastern Steel Company, at Middlesboro', were completed in June, 1883, to do general work. They are situated on the bank of the River Tees. Ocean vessels discharge at their wharves and carry away their products. The works use locomotive and overhead cranes, and steam capstans; very little of the carrying is done by hand. They are surrounded in every direction by

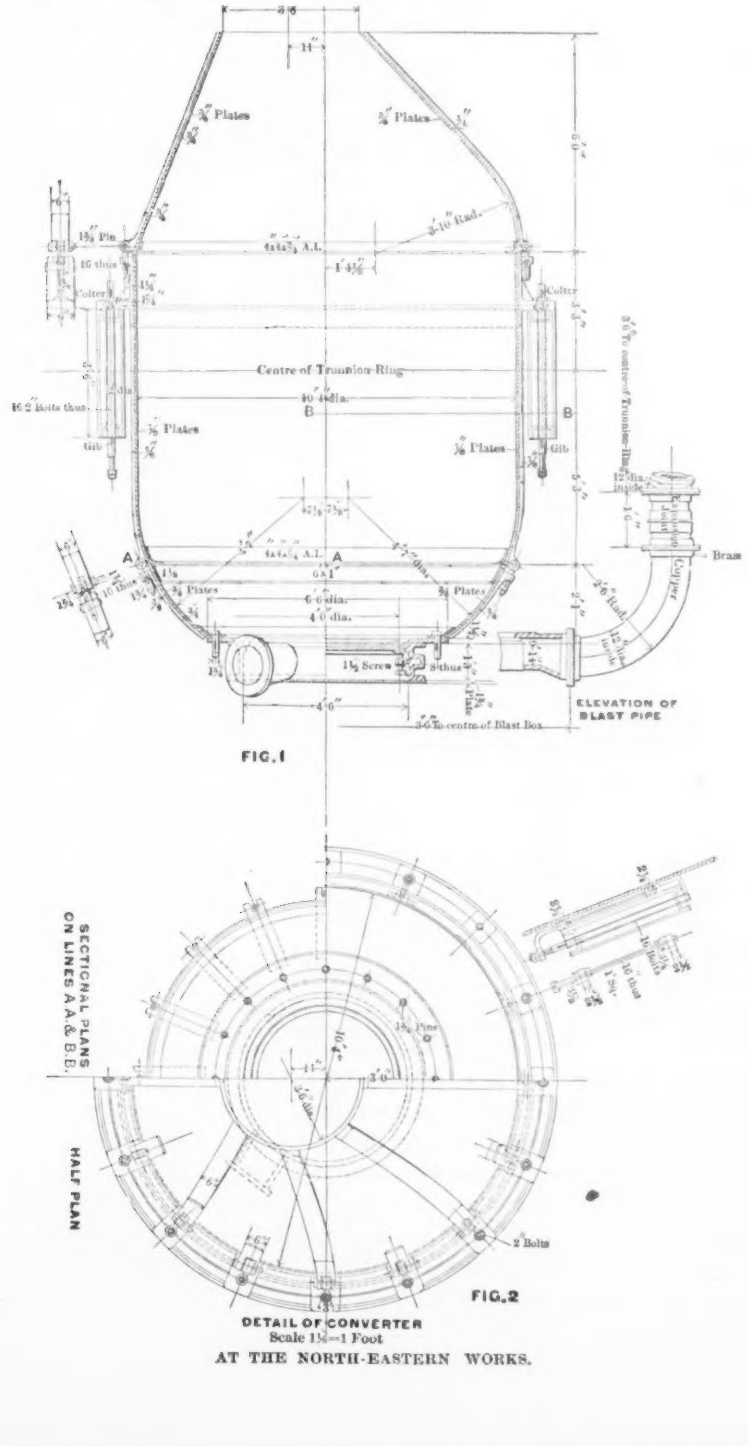
railroads and in their immediate vicinity there are many blast furnaces of large capacity, which furnish direct iron. They have also cupola furnaces. They have four 12-ton converters.

The works at Glengarnock are situated a few miles from Glasgow. They were completed in July, 1885. They have three cupolas for burning the lime, and three for melting the pig, four 10-ton converters, soaking pits, cogging and plate mills, and a basic shop with overhead cranes.

The works at Angleur, near Liège, have six 5-ton converters. Four are built after the old plan with a deep pit, and two are built with the casting pit entirely above ground, which is much more convenient. These works, with those of the Rhine Steel Co. and Hörde, were large producers of acid steel, but have for the most part given up its manufacture to take up that of the basic. In these works the two processes are either running together, and the products mixed so as not to be distinguished, or the basic has entirely replaced the acid works. When both plants are running, the material is delivered together, and these products cannot afterward be separated. The iron ties so extensively used in the lines of most of the continental railways made in these works are almost exclusively made of Thomas iron. At Hörde they weigh about twenty to the ton. They have been used on very many of the European railroads for a number of years, and are found much better and cheaper than wooden ones.

The process differs in many of its essential details from the ordinary acid Bessemer process. Acid refractory materials cannot be used. The disposition of the works is also quite different, the change in the process necessitating important changes in its arrangement. The works are much less complicated, occupy less room, and are more light and convenient. There is nothing, however, to prevent the acid Bessemer from adopting most of the mechanical details of the basic process, and it is not probable that any more acid Bessemer works will be constructed in the old style. Each works requires to have its own shop for the manufacture of the basic material. This must be a very large space, and is placed behind the converters for greater convenience, both of manufacture and of repairs to the converters.

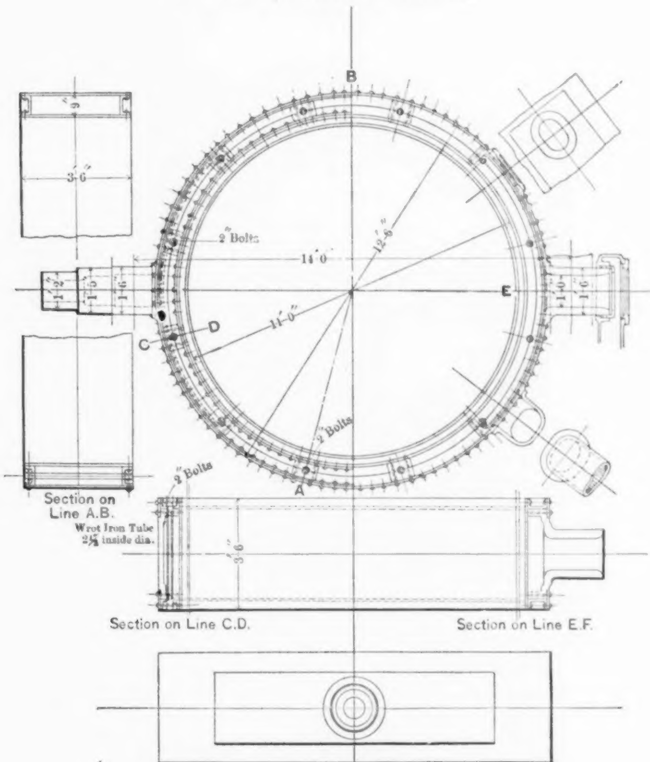
The converter itself is made in four parts, each one of which can be treated independently. These parts are the nose, the body, the bottom, and the plug. It will be seen by reference to the draw-



DETAIL OF CONVERTER
Scale 1½=1 Foot
AT THE NORTH-EASTERN WORKS.

ing, that the general shape of the basic converter, Figs. 1, 2 and 3, is quite different from that of the acid one, Fig. 4, where there is a projecting nose. In this process, experience has shown that while it is necessary to have a nose, it is more desirable to have it a little to one side of the axis of the cone than to have it on the side. In some works it is placed directly in the axis of the cone.

Fig. 3. (Section of Fig. 1.)



DETAIL OF TRUNION RING FOR CONVERTER
Scale 5¼ feet per inch

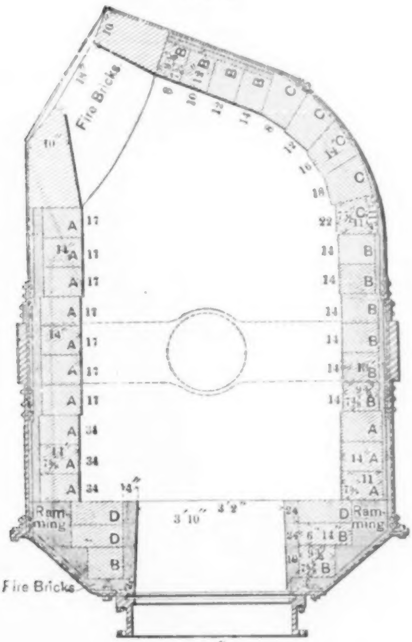
AT THE NORTH-EASTERN WORKS.

The body is usually cylindrical, arranged with lugs, so that it may be raised from its trunnions and carried on the crane to whatever place it is necessary, in order to reline it. There are two types of these carriages which differ essentially in their details—one, the carriage used at the South Staffordshire works, is shown in the elevation in Fig. 10 (p. 48), where the hydraulic lift is placed in the pit underneath the converter, and the body simply lifted off on to the car-

riage. These lifts are shown in Figs. 6 and 7. This method has all the disadvantages of the acid basic Bessemer, where the hydraulic lift is fixed, and in the pit beneath the converter. In the other, that of the North-Eastern and Glengarnock works, the hydraulic lift is on the carriage itself, leaving the pit entirely free, Fig. 5.

In the acid Bessemer works, the converter rotates only through

FIG. 4.

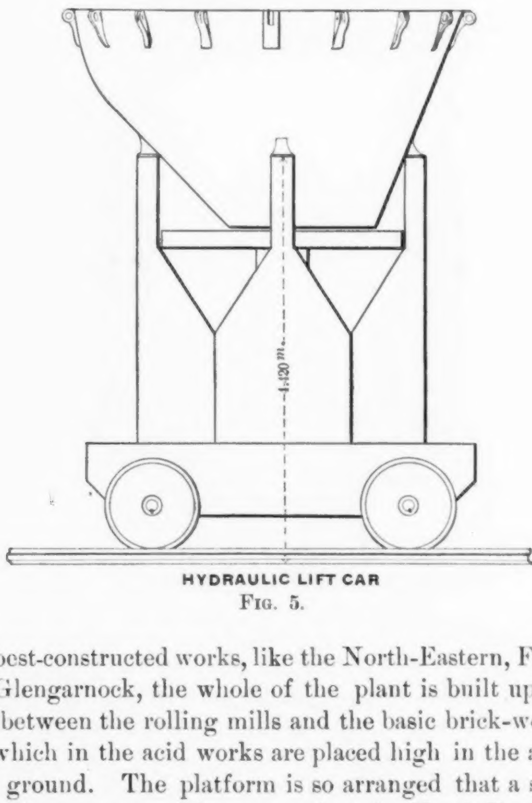


Scale ¼"=1 Ft.

CONVERTER OF SOUTH STAFFORDSHIRE WORKS.

a half circle. The least that it can rotate in the basic works is three-quarters of a circle, and in many of the works it rotates either way through 360°. This is necessary, because it has been found that when it rotates only through a small arc the wear on the sides is very unequal. This is quite natural, for when the steel is turned down for casting, a considerable quantity of the slag, which is very infusible, accumulates on that side, and remains there protecting the walls until the heat in the converter rises sufficiently to melt it off. The other side is, however, not protected at all. As it is necessary to remove the slag from time to time, if the converter rotates in such a way that the slag can be emptied away from the casting pit on the

opposite side, not only will the slag be out of the way, and can be immediately removed, but some of it will adhere to this side, and the two sides will then be protected evenly. This simple change in the rotation of the converter has made it possible not only to work faster, but better, as it relieves the ground under the converter entirely, so that the men can have easy access to it at all times. The lining of the converter is made either by ramming, with very hot iron rammers, shrunk dolomite, burned at a very high temperature and ground to about the size of a bean, and mixed with coal tar, or of bricks made of the same mixture and building them in.*



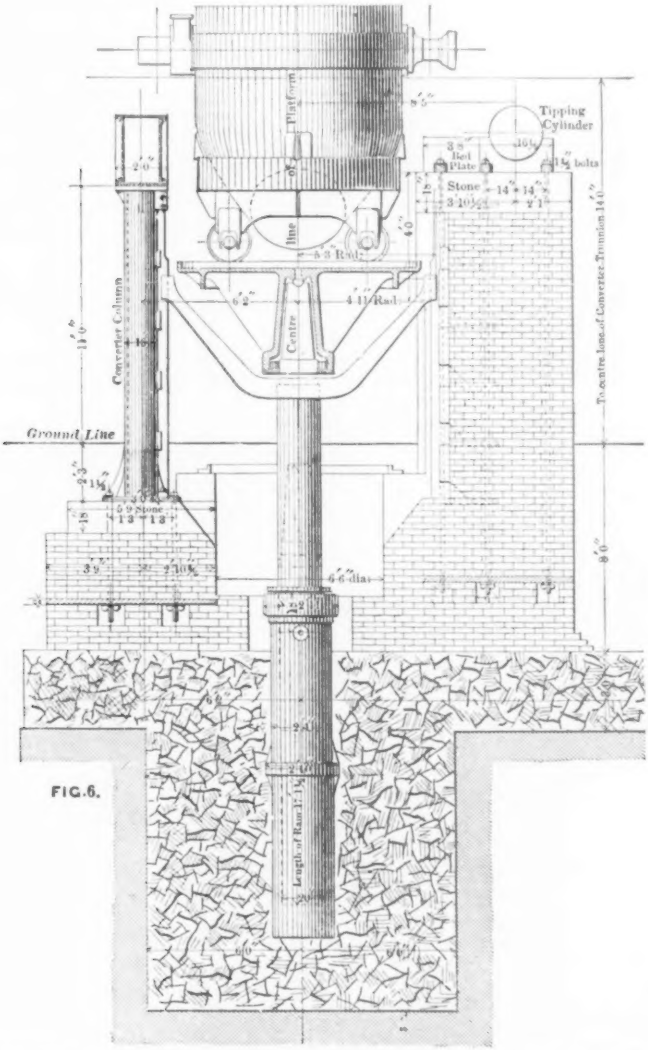
HYDRAULIC LIFT CAR

FIG. 5.

In the best-constructed works, like the North-Eastern, Fig. 9, p. 48, and the Glengarnock, the whole of the plant is built upon an iron platform between the rolling mills and the basic brick-works. The cupolas, which in the acid works are placed high in the air, are put upon the ground. The platform is so arranged that a small locomotive engine runs upon the top, and at the North-Eastern all the freight cars, without discharging, are brought by hydraulic lifts and moved about on the platform. This being supported upon pillars, the ground floor beneath it is entirely free. At Glengarnock, and at the

* Basic Refractory Materials, Trans. Am. Soc. Min. Engs., Vol. XIV.

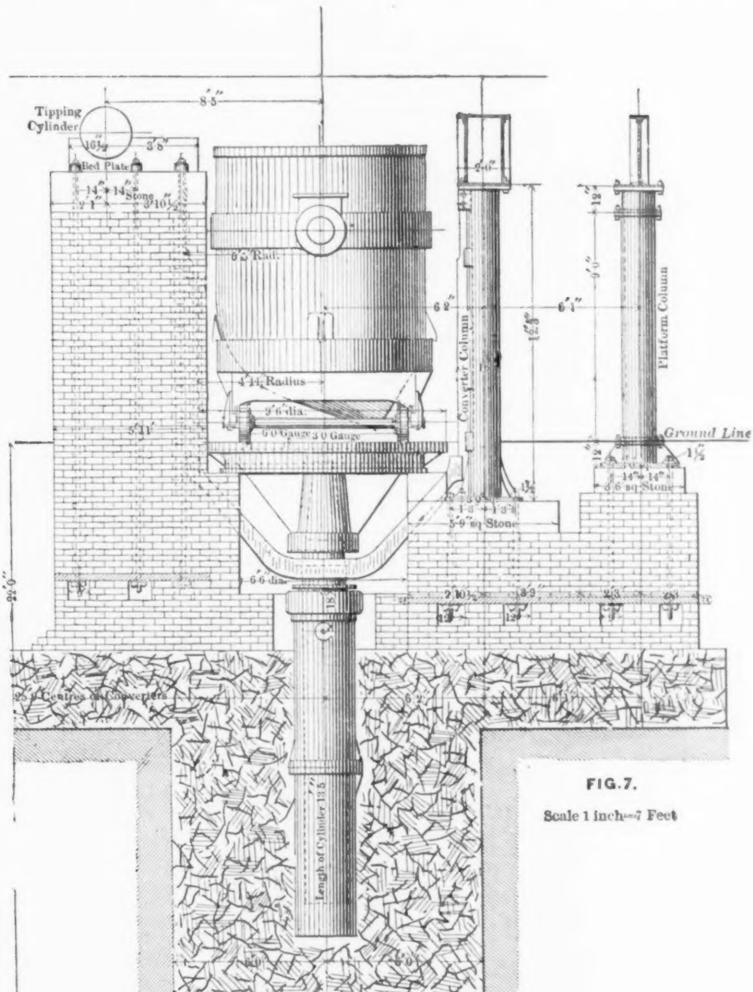
proposed works at Palmer's ship-yard in Newcastle, the locomotive reaches the top of the iron platform by a surface railroad of very



PIT-LIFT OF THE SOUTH STAFFORDSHIRE WORKS, AT RIGHT ANGLES TO FIG. 7.

heavy grade. The limekilns are also on the ground level, the lime being stored in bins in the story above the converter in such a way as to be connected with the mouth of the converter by a spout, or

charged, as at the South Staffordshire works, in a conical-spout wagon, Fig. 8, and brought to the level of the converter like the rest of the

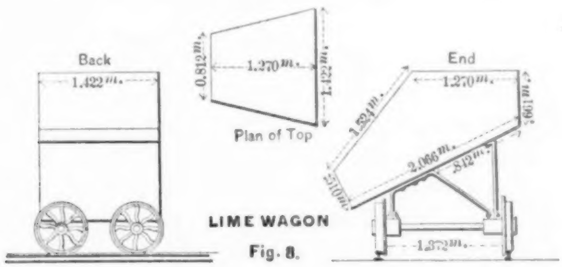


PIT-LIFT AT THE SOUTH STAFFORDSHIRE WORKS, SHOWN AT RIGHT ANGLES TO FIG. 6.

charge. The pig iron is introduced from the cars into the top of the cupolas, which are on the converter level. They are raised high enough above the ground below so that their contents when melted

can be discharged into a ladle on a bogie, which is raised upon a lift, or brought up the incline, and poured directly into the nozzle of the converter. The spiegel furnaces are on the platform, so that their contents are brought from the converter level directly to the mouth of the converter. The parts of the converter wear very differently, and require very different repairs; and as there is great variation in the length of time which they last, they must be arranged in such a way that they can be acted upon separately.

In all the most recently constructed works, every part of the establishment is controlled by overhead cranes, the work of changing a converter being done by the ladle bogies and by the overhead cranes. When the converter is to be taken to pieces, it is turned horizontally. The cast-iron bottom plate of the wind chest is then removed and left upon the upper level. The converter is then turned to a vertical position, the nose being up-



LIME WAGON
Fig. 8.

permost and the bottom with its plug removed. If no other repair is required, a new plug is put on in the same way. If further repairs are necessary, it is turned with the nose down, and the nose removed. It is then turned back into its normal position and the body repaired in place as at Eston and Glengarnock, or, as at the North-Eastern, the body removed by a seventy-ton overhead crane, and carried to the place where it is to be repaired. It is put together in inverse order.

The moving of the converter on the wagon, Fig. 5, is done by steel ropes and steam winches, or wheels on hydraulic pistons, turn-tables and tracks being provided in every direction, so that it can be removed to any convenient position. The parts of the converter are set down by the overhead crane where they are wanted. There are always such a number of the parts in duplicate ready to be used in case of need, that no delay arises from the want of them. Any part taken off is immediately replaced, while that needing repairs or worn

out, goes at once to the repair shop. The necessary delays in the process are such that it is only by the best mechanical devices that time can be saved and the output increased. The weak points of the process are the deficiency of output as compared with the acid process, which can only be cured by the most careful mechanical engineering, and the high cost of refractories per ton of steel, which is, however, being rapidly reduced.

When the process was first introduced, it was supposed that any impure irons could be used, provided the amount of silicon was kept within certain limits, and that the more phosphorus it contained the better. This has been found not to be the case. A certain amount of phosphorus, which is over one per cent., is necessary to raise the temperature of the metal to the point required for the proper conduct of the operation; more phosphorus increases this temperature, and makes it possible to make a purer product, but it increases the loss very rapidly. The silicon should not exceed one per cent. — the less there is of it the better; it would be well if there were none at all. When there is more, there must be less phosphorus, and there is a greater probability of the steel being of inferior quality. The usual limits required for phosphorus are 1.8 to 3 per cent. On the continent a larger amount is admitted. So far as the quality of the steel is concerned, the amount of phosphorus makes but little difference, provided there is enough of it to produce the heat, but the greater the amount of it beyond a fixed limit, the greater the loss, the amount of lime used, and the amount of repairs to be made. The limits of sulphur, silicon, and manganese differ a little in the various works. Those generally adopted are given below:

Silicon	0.5	to	1.8	per cent.
Sulphur	0.08	"	0.3	"
Manganese	0.5	"	2.5	"
Carbon	2.5	"	3.5	"
Phosphorus	1.2	"	3.0	"

In considering the amount of foreign materials that may be contained in the pig, it is found that those which contain less than one per cent. of phosphorus cannot profitably be treated. It should contain as a minimum from 1.2 to 1.5 per cent. What the maximum may be will vary in different places and different circumstances. It is usually thought desirable to have about 2.5 per cent., and in many of the continental works the quantity is carried up to 3 per cent. when the silicon is 0.5. When the amount of

phosphorus is increased beyond 3 per cent., the after-blow is very much lengthened, which causes a very great increase of the wear and tear of the lining.

A certain amount of manganese is also necessary. It should never be less than one per cent. It is better to have it above two per cent. The best quantity is considered to be between two and three per cent. Its principal rôle is to remove oxide of iron, and at the same time serve for the elimination of the sulphur. Beyond a certain limit it retards it. Up to this point it is highly beneficial, all the more so, as its presence implies the absence of silicon.

Silicon should be present in as small quantities as possible; 0.5 per cent. is considered as harmless, but in no case should the quantity be beyond two per cent. It would be better if there were none at all. Its presence delays, or even prevents, the separation of the phosphorus by making the slags acid. To insure the separation of the phosphorus, the slags should be basic from the start. There is no difficulty in doing this with irons of less than one per cent. of silicon. When more than that is present, the after-blow will be considerably prolonged, without the certainty of making a low phosphorus ingot. Experiments are now being made in Wales to remove the silicon.*

Sulphur is the worst of all the ingredients likely to be found. It should be under 0.1 per cent. With high manganese there may be as much as 0.2 per cent. to 0.3 per cent. It is estimated that it takes from 1 per cent. to 1.5 per cent. of manganese to eliminate 0.15 per cent. of sulphur.

These amounts have all been found by experience. They are not closely adhered to in any works, as local circumstances will cause the economical conditions to vary a little, but they are accepted as those which, on the whole, give the best results.

The pig used at the North-Eastern works contains 2.75 per cent. of phosphorus, and from three-fourths to one per cent. of silicon. The contract specifies 2½ per cent. of phosphorus. More can certainly be used, but the more phosphorus the greater the waste in iron.

The whole object of the South Staffordshire mill is to make plate—*i. e.*, soft ingot iron—in order to compete with the plates coming from other sections.

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* Basic Open-hearth Process, School of Mines Quarterly, Vol. VII., p. 49.

The highest limits allowed for plate are:

Silicon	trace.	
Phosphorus	.08	per cent.
Manganese	.3 to .5	"

If the iron is taken directly from the furnace, the manganese is allowed to run down to 2, or even 1.25 per cent. If there is no manganese, as much as 1 per cent. of silicon may be present, but no more. If manganese is present in considerable quantity there will be no silicon.

The cast iron which is used at the South Staffordshire works contains before melting:

Silicon	1.25 per cent.
Phosphorus	3. "
Manganese	1 to 2 "

After melting in the cupola it contains:

Silicon	0.8 per cent.
Phosphorus	3. "
Manganese	1.25 to 2.50 "

When the silicon is 1 per cent. they want 1.5 per cent. of manganese in the pig; when the silicon is 1.5 per cent. they want 2.5 per cent. of manganese. They do not want silicon, but as much manganese as possible. They will take down to 1 per cent. of manganese if only 0.5 per cent. of silicon is present. Six per cent. of phosphorus can be worked, but it will give more than double the loss of 3 per cent.

In most of these works the object is to get the iron direct from the blast furnace, in which case the manganese will be saved. This is equally true of both acid and basic works. When there are a number of furnaces, as in the very large European works, there is little difficulty in doing this. At Eston there are nineteen blast furnaces heated by Cowper stoves, three of which are running on basic pig. At Barrow, which is an acid works, they use a mixture of blast furnace and cupola metal. Here there are twelve blast furnaces in one row and two others at a little distance, each producing 80 to 100 tons in twenty-four hours with four castings. The engine houses are behind the line of furnaces, the hot-air stoves of iron behind them, and the regenerative stoves between them. The casting pit is in front, and unprotected. There is an opening in it about twelve by six feet, which communicates with a gallery beneath, running the whole length of the line of furnaces. Each opening corresponds to a place to cast into two ladles on bogies.

The runners are made in the sand, and communicate with the pig bed. The first ladle has its proportion of blast-furnace metal run into it, and then the second. The rest of the iron is turned into the pig bed. The slag runs off one side into cars. There are special locomotives both for the iron and slag. The line of cupolas is at right angles to that of the furnaces. The ladles are filled half full at the blast furnace, and receive the other half of their charge from the cupola. When full they are carried by a locomotive more than a mile to the converter. The distance in a straight line is not more than three hundred feet, but the levels are so different, and there are so many lines of track to cross, that it has not been thought best to make any change. An iron foot-bridge connects the blast furnace and the converters. The bogie ladle which is used in most of these works, has been fully described.* The molten iron is turned directly into the converter from it.

At Hörde they formerly made ferro-phosphorus in the blast furnace, with from 11 to 20 per cent. of phosphorus, out of a mixture of Thomas slag, puddle slag, and phosphate of lime. This was used in considerable quantities to increase the heat in the converter. As a substitute, they now use at least fifty per cent. of puddle slag in the blast-furnace charge, but have given up making the ferro-phosphorus.

They use at these works *tuyères* in the blast furnace, composed of two pieces of round plate, the one more flaring than the other, upon which water plays. They cost very little, and give entire satisfaction. All the old water *tuyères* are given up.

The details of arrangement of the plant differ in most of the works. The general arrangement is, however, the same. The departure from the construction of the acid plant is wide, and grows wider every day as the price of steel falls and the necessity for a greater output becomes more urgent.

The plant of the North-Eastern works is arranged in four stories, the two upper ones being reached by lifts, and the two lower ones being served entirely by locomotives. On the ground floor there are three cupolas, each capable of melting from 1,600 to 1,700 tons of cast iron per week. They are built on a platform so that their tap-holes are six feet above the ground. They discharge their contents into a ladle moved by a vertical worm, which is carried by a lift to the upper platform. These cupolas are lined

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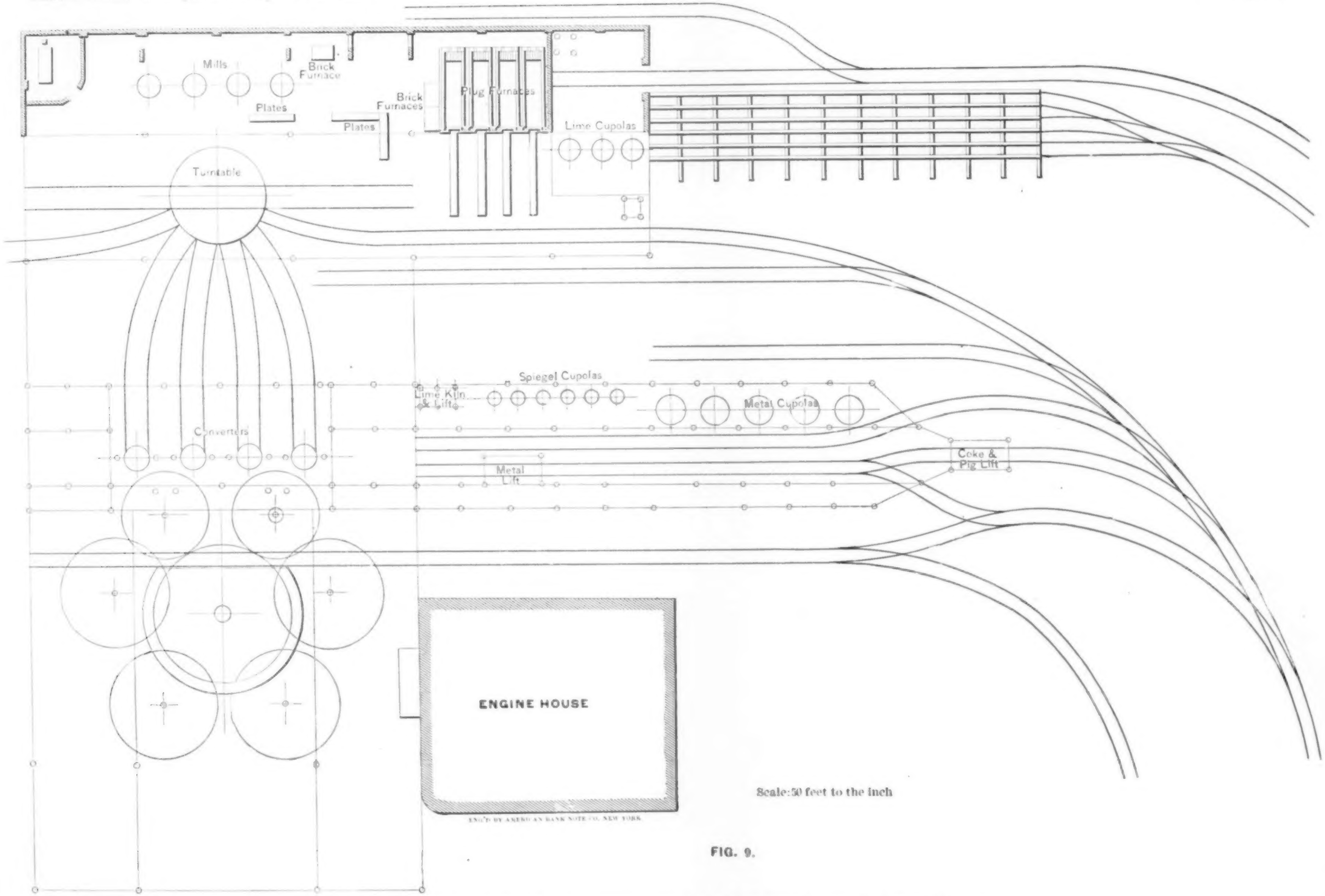
* *Journal of the Iron and Steel Institute*, 1876.

with a refractory sandstone. On this level there are three hydraulic lifts, one at each end and one in the middle, Fig. 9, which are each of twenty tons capacity, both of which are capable of raising the ordinary freight cars loaded with either coke or pig iron, and, without unloading, discharging them directly into the cupolas. The one at the end is generally used for the pig and coke, and the one next it for the charge of molten iron, but they can be used indifferently for one or the other. The second story is a staging, supported on pillars, and gives access to the mouths of the cupolas and converters. It is built of iron, and is 22 feet above the lower floor. On this level there are four spiegel furnaces. These furnaces are lighted on the cast iron used for making the moulds, and are then changed to spiegel. The spiegel is not, however, always melted, but is frequently charged hot, warm, or entirely cold into the converter. The mouth of these furnaces is on a level 12 feet higher than the converter level. The third lift is the ordinary hydraulic chain lift of the district, of two tons capacity. It carries the spiegel for the spiegel furnaces, and the lime, in iron bogies to the third floor. The lime is stored in bins. It is charged in the converter through a long flexible pipe. The converters are shown in Figs. 1, 2 and 3. The converter level is served entirely by a small locomotive, which does the whole of the work of shunting and carrying the cars and the ladle of molten iron where it is wanted. These cars are all brought to the level below by an ordinary locomotive. There are four 10-ton converters, with a bridge sufficiently wide in front and behind to do all the work required with ease. The peculiarity of these works is, that there is plenty of room in every direction for doing all the work. The whole of the converter room was originally covered with an iron roof, which became so weakened by the heat, that a heavy storm did so much damage to it, that it was removed, and the converters now blow in the open air.

At the South Staffordshire, Fig. 10, works the steel plant consists of three cupolas with drop-bottoms, at one end of the line in front for melting the iron, two of which are in use, and two cupolas for burning the dolomite and the old bricks, which are here always burned over, and which make the best bricks. Most of the dolomite comes to the works already burned.

In front of the cast-iron cupolas is a platform about eight feet from the ground, on which the runners from the cupolas for the cast iron for the ladle are placed. A hole in the plat-



Scale: 30 feet to the inch

FIG. 9.

PLAN OF THE BRICK-SHOPS AND CONVERTING PLANT OF THE NORTH-EASTERN WORKS.

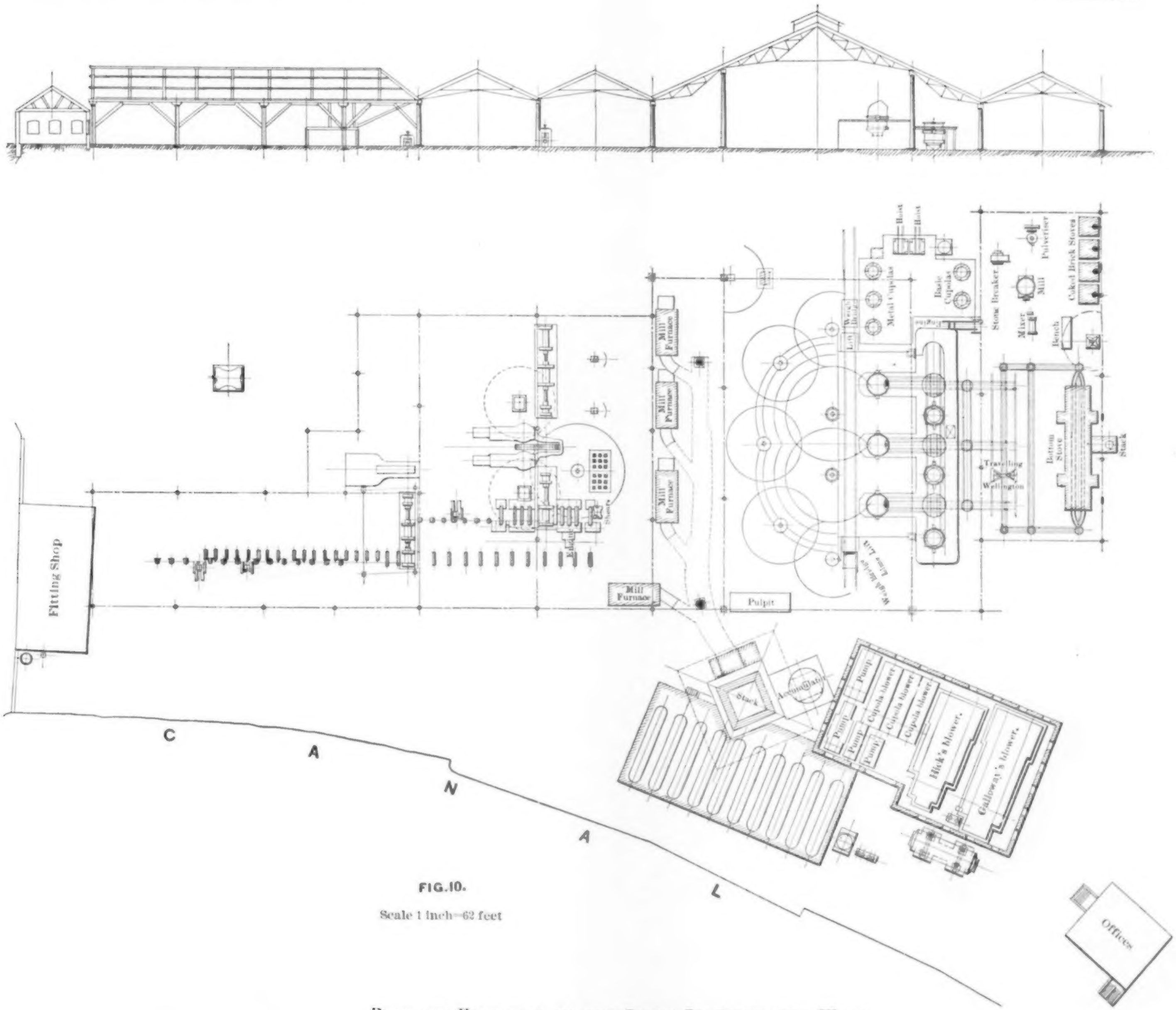


FIG.10.

Scale 1 Inch=62 feet

PLAN AND ELEVATION OF THE SOUTH STAFFORDSHIRE WORKS.

form allows of the iron flowing into the ladle below. These cupolas are placed close to each other. Next to the cupolas is the lift for the cast-iron ladle. Next this, arranged behind a heavy bridge, on which is a very wide gauged railroad, are three converters, Fig. 4, 8 feet in diameter and 20 feet apart between the center of the trunnions. At the other end of the platform is another hoist for bringing up the lime and the spiegel. On the side, at right angles to the line of cupolas and converters is the pulpit, which is raised to the level of the converter platform, and has the pressure gauges and a bell connecting with the engine room.

The converters, which were made for an acid plant, are for five tons. They are made to blow against a water screen. There are three of these, two in use and one in repair. The pit is made of two arcs of a circle, Figs. 10 and 11, commanded by two ladle cranes, so that each crane commands two co.:verters. The ladle is on a four-wheeled truck, and is movable by hand or by hydraulic machinery. Around the pit there are five cranes, some of them supported from above. The converters are made of three parts, the body, the ring or lower part, and the plug. Each of these is treated separately. All the parts can be detached and lifted on the hydraulic lift underneath the converter, wagons of different sizes being provided for each of the parts, and turn-tables in the basic shop, by which they can be carried to their respective places. The body is turned down and held on the ring on the wagon by lugs, and is lined on these wagons. When it is necessary to repair the iron-work of the shell, there is a jib crane for lifting them. The bottoms and plugs are rammed on blocks of wood, and then moved by the same crane whenever it is necessary. For these three converters there is always one converter being lined. There are two converter and three bottom trucks. There are always two bottoms ready for use, and four bottom shells ready to be lined. For the cupolas there are two ladles; each ladle bogie has two reserve ladles, or three in all. There are three blowing engines for the converters, two for the cupolas, and two duplex hydraulic pressure pumps.

The works at Glengarnock have nine blast furnaces for furnishing direct metal, which are situated about a quarter of a mile from the steel works. The molten metal is brought to the converter level by a locomotive running up a steep grade to the top of the converter platform. The blast furnaces are tapped into a bogie ladle running in a tunnel under the level of the ordinary pig bed.

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Only half of a charge may be taken direct; the other half may be taken from one of three cupolas, situated at a short distance from the converter, and high enough above the ground for the bogie ladle to run under their spouts. The cupolas are calculated to melt 2,000 tons of cast iron per week, so that the converters may be run on direct metal alone, cupola metal alone, or half of each. By the side of the converters are three cupolas for calcining the dolomite. Behind the converter is the basic shop, which is 160 x 75 feet, provided with a 20-ton overhead crane. There are four 12-ton converters, or 10-ton basic, which weigh, when lined and in place, 60 tons each. They are placed on an iron platform 15½ feet high. The nose and bottom of the converters only are movable. The body is fixed, and must be relined in place. The converter is made to rotate through 360° by a hydraulic piston attached below the level of the platform. The spiegel furnaces are on the same level as the converters. The lime is added from a chute which is attached to a low platform on the basic shop side. It is brought to this level in wheelbarrows, which empty directly into the chute. The converter is turned back at an angle of 45° to receive it. The lime is raised to this level by a hydraulic lift. The metal is brought in a bogie, tipped by a worm attached to a hand wheel which acts upon a gear wheel fitted to the axis of the ladle. The axis is about four feet long, and is attached to the ladle by a clutch, so that it can be easily removed. By this disposition no slag or iron can slop on to the gearing. The full ladle is easily turned over by two men. All the work of the platform is done by a locomotive. The bottom and nose are removed as at the North-Eastern works, but as the bridge is lower, the hydraulic wagons are much less cumbersome. The converter platform has no roof, but is entirely open to the air. The molten iron is poured into a spout on wheels, about three feet long, the end being turned into the converter.

The converters for the basic plant at Eston are six in number, divided into two sections of three each. Each set of three, Fig. 12, has two ladle cranes and two pits, and four ingot cranes. The ladle cranes have the ladle on wheels, controlled by a man in a house at the other end of the crane, so that it can be brought in and out on the crane to suit the place of the ingot in the pit. The pit is very shallow, being about two feet six inches in front, and not more than twelve inches next the crane. The converter holds twelve tons. The iron is brought direct from the blast furnace, melted, and carried by a hydraulic lift up to the platform. It is poured into a

runner, whose position is regulated by a bar passing under it, the bar being supported by two chains. All the six converters have but one pulpit. The place about the pit is very contracted.

At Palmer's ship-yard, at Newcastle, they are about to erect a large basic plant. Two converters are to be put up first, and the others afterward. The plant is to be arranged like that of the North-Eastern, so far as the pit and the ladle cranes are concerned. There are three hydraulic cranes, one on each side and one in front of the pit. They expect to make five-ton ingots for plates. To move these ingots there is to be a large crane on a carriage, which will not only lift the ingots out of the pit, but carry them at once to the soaking pits, of which there will be but few, as the ingots are so large. The arrangement of the brick shop and of the Bessemer plant in general, with this exception, is to be like the North-Eastern plant, Fig. 9, but as they have plenty of room, the locomotives are to go to the top by an inclined way, as at Glengarnock, taking the iron direct from the furnace.

In the establishment at Hörde, the acid plant which was just constructed is now lying idle, as they have given up making acid steel. The basic plant which was among the first built, has the cupolas set high up at the proper distance, and lifts at convenient places. In front of them are the long runners, like those in acid works, which are fixed. The end runner rotates so as to come under the fixed ones, and has so much motion that the spout goes a short distance into the converter. For the two converters there are two spiegel cupolas and three iron cupolas, with eight *tuyères*, supplied by an air-chamber running round the whole furnace. The blast engine for the cupolas is a fan of fully thirteen feet in diameter, made by C. Brinkmann, of Witten, Westphalia. They have found it necessary to abandon the Root's blowers, and use these entirely. There was a very great loss of wind at considerable pressure, but more on one side than the other. A piston engine would have been better, even if it had to run at an exceptionally high velocity. Two converters are placed together, Fig. 16, and, at a convenient distance, two others in a straight line. They are set on stands made of iron, so that each one is independent. These stands are 27 feet high, and 18 wide at the base, and 4 at the top. The trunnions of the converters are set in fittings on the top of this stand. A locomotive ladle crane, on a carriage 8 feet by 12 feet, runs through a pit about 30 feet wide. The ingot moulds are put anywhere that it is convenient to have them; the locomotive

moves the whole charge anywhere. As there is always in hydraulic cranes a loss of water, the ladle is moved up to its position by a large pump on the crane; a small one on the opposite side keeps up the supply. The pit is not more than three feet deep, and is so wide that the heat cannot collect, so that, with the four converters going, there was no very sensible elevation of temperature except just around the moulds. The stays for the ladle are made of rails bound together by screws passing through a loop. This makes a very stiff and a very strong stay. Formerly the slag was poured into the pit, as is usual in acid works. The ladle crane then came to take away the steel. The slag was cooled with water and immediately removed. Now, as soon as the slag is ready to be cast the locomotive crane comes to receive it in a small ladle, and carries it away to the most convenient place to dump it. This slag is all sold to a company, which treats it. When the steel is ready for casting, the crane returns with the steel ladle to receive it. During the castings the steel boils tumultuously, causing the contents of the ladle to jump as much as 10 to 15 inches, and to slop over the sides. Most of what goes over is slag, but some scrap steel is made. All of this which is not recovered in the works is recovered in the treatment of the slags.

Of all the plants that I have visited, the plans of the North-Eastern and Glengarnock works, including the basic shop, the converter plant, and the mills, are the best designed and arranged for the most economical carrying out of the process. Those designed for Palmer's ship-yards are in some respects an improvement on them, but the main features are the same.

When a converter has been relined, just set up, and is cold, 1,400 lbs. of coke and coal are put in and lighted with hot slag. When the coal is well fired the blast is turned on, but is used only during the time that the other converters do not need it. If the blast could be used all the time, it would take only two hours to heat it up; but as it can only be blown in the intervals, it takes five hours. In this interval the converter is rolled—that is, turned very slowly backward and forward with the blast on, so that all parts of it will become equally heated. If a casting has been made, and no other repairs are required except the removal of the material which has become attached to the mouth, this is removed with a bar. What clay is needed for repairs around the mouth is put in and pressed to its place, and the converter is ready for a new charge. If there is any delay, the lime is added with some coal, and the converter

"rolled" until the charge is ready. In this way the converter is kept quite hot. Rolling is, however, never done when it can be helped. As soon as the converter is ready, the quantity of freshly burned lime necessary for the charge is added. In some of the works this lime is purchased and kept on hand, but as it is liable to slack, it is better to have the limekilns arranged, as at the North-Eastern and Glengarnock works, near the converter, so that lime just drawn from the kiln may be used. In most of the works the lime is added from a bin which is kept full by the use of a special lift. It is placed in front of and above the converter, with a movable spout which fits into the mouth of the converter. This is the most convenient and in every way the best arrangement, but it cannot always be made. In the South Staffordshire works a lime wagon with a spout is used, Fig. 8. This is filled on the ground, and is then raised on a special lift and brought to the front of the converter and the change made. In most works all the lime required for each part of the operation is put in at once; in a few it is thrown in during the operation. The amount of lime to be added is determined by the amount of phosphorus and silicon in the pig. The usual rule is to add 16 to 20 per cent. The best way to add it is to calculate for a very basic scoria, which will not contain more than 10 to 15 per cent. of silica, less even than that, if possible. The lime should be as free from silica as possible, and should not contain any large quantity of iron oxide. When the iron is direct metal, a sample is taken and the amount of lime to be used determined from its appearance. If there are long intervals between the blows, so that the converter is not very hot, the lime is sometimes heated, but is generally added cold. In some works it is considered essential, in order to have hot metal at the end of the operation, that the lime should always be charged hot. In order to avoid the inconvenience of having to draw it from the kiln, the lime in some works is kept in hot stoves until wanted, but it usually gets cold before it is added to the converter. At Creusot 1.5 per cent. of fluor spar is added with the lime in order to increase the fluidity of the slag. As soon as the lime is in, the converter is ready for the charge of iron. Where the works are acid plants transformed, this is added from long runners, as is usual there; but in the newly constructed works, either iron direct from the blast furnace, which is apt to give high phosphorus, as at most of the works, is used, or the cupolas are placed on the ground and the melted iron raised by a hydraulic lift. In the South Staffordshire works the iron is brought in a ladle from the cupolas, which are

situated to the left of the converters. They are raised high enough above the ground so that the tap-hole is just over the top of the ladle. The ladle, with the iron in it, is then raised vertically to the bridge in front of the converters. By means of a capstan worked by two men it is brought opposite to the converter and turned over with a hand wheel and worm. This worm should always be on the outside. When it is in the interior, as it is impossible to prevent slopping, it very often gets clogged, which sometimes makes serious delay, and sometimes requires that it should be taken apart before it can be used again, and usually requires the use of a jack-screw to raise it again after it has been turned down to empty the iron.

At the North-Eastern works the iron is brought by a locomotive from a furnace over a mile distant. To prevent it from cooling while under way, the ladle is first filled with iron and then covered with blast furnace slag, which is left to cool and forms a thick solid crust on the top of the melted iron. A space in front is cut out to pour from.

At Eston the furnaces are set so high that the bogie with the ladle on it can be run under the spout which delivers the liquid pig from the furnace. The iron in the ladle is brought up on the lift, and carried to the converter by the locomotive. In front of the converter is a short spout, which for convenience is arranged on wheels, so that it can be turned into the mouth of the converter and turned back again out of the way when not required. This is put into position and the iron turned into it from the ladle, which is carried away by the engine. The spout is then turned away and the converter turned up.

When the ladle of the North-Eastern or South Staffordshire works is carried away, as it is difficult to turn it up by hand, it is let down on the lift inclined as it came from the converter, a piece of iron being placed one end on the platform and the other against its side, the lift is made to descend and the ladle is turned up by the pressure of the iron bar against its side. At Glengarnock there is no difficulty of this kind. Whatever arrangement is made for bringing the iron, there should be the least possible delay between the furnace and the converter. The iron should be both hot and liquid, all the more so if for any reason there has been any delay between the two blows. The operation of blowing is called *teeming*. A good teemer should be an active man, with a quick, accurate judgment. He turns the converter down to receive the charge, and

should be at his post watching every indication until the charge is finished. He alone gives directions what to do, and makes all the signals.

In the first three to four minutes after the converter is turned up the silicon burns with a reddish flame, and some of the phosphorus is oxidized. The whole of the silica and part of the phosphorus passes into the slag. In the next ten minutes the carbon burns with a clear yellow flame, and often a great deal of slopping. So much carbonic oxide is produced that some of the phosphorus which has passed into the slag is reduced and re-enters the iron. The exact time when all the carbon is burned out is determined by a spectroscope. The flame then sinks. During this time the heat of the charge is such that the pressure of the blast is made to vary considerably, until the phosphorus, which up to this time has been only slightly acted on, commences to oxidize. It now passes rapidly into the slag, producing great heat. This period of "after-blow," as it is called, lasts until the blower judges that the phosphorus is gone, the signal to the engineer being given by an electric bell on the pulpit. While the carbon is burning some manganese passes into the slag. After the carbon line has gone the charge is blown a few seconds only. The converter is then turned down, and the slag poured out. A sample is then taken. Lime is added, the amount being from 1 to 6 per cent. At South Staffordshire, it is 1 to 1¾ per cent.; at the North-Eastern, it is 2 per cent. The sample is hammered flat in its own heat, and cooled with water, and broken. According to the fracture the blower judges of the phosphorus present and blows again, repeating the operation until the charge is right. From long experience, the men are able to judge by the color of the flame and the noise of the blast how to operate, and what they lack in judgment of the eye and ear is given by the test. The number of times a converter is to be turned down to empty the slag, and take a sample will depend on the quality of the iron, and how much experience the men have had with the same iron. It will generally not be more than three times, but I have seen the samples taken as many as four, and sometimes more, with an inexperienced teemer.

It is found that repeated additions of lime greatly facilitate the separation of the phosphorus, and that they are absolutely necessary when the phosphorus "hangs," *i. e.*, refuses to come out. When the charge is right the ferro-manganese is added. This is heated

red-hot for the purpose, but is often black before it is put in. The ferro-manganese contains 77 per cent. of manganese and 5 per cent. of carbon. Just as little as will give the right quantity of carbon is used. The ferro-manganese should have less than one-tenth per cent. of silicon. If the charge should work very hot from the presence of an unusually large amount of phosphorus or silicon, the amount of lime is increased, and cold scrap sufficient to keep the temperature down, is added.

As the ferro-manganese contains phosphorus, and as the bath immediately after the after-blow contains considerable oxide of iron, there is danger that its reduction by the carbon of the ferro-manganese will form carbonic oxide sufficient to reduce the phosphoric acid in the slag and cause the phosphorus to pass into the metal. It is necessary, therefore, to pour off the slag before putting in the spiegel, to prevent the reduction of the phosphoric acid in the slag. As all the slag cannot be separated, a certain amount will remain in or go back into the iron. In order to diminish the quantity in some works, as at Creusot, only about one-third of the spiegel is added in the converter and the rest is put in, in the ladle into which but very little of the slag comes. If the whole of the spiegel was added in the converter it would cause too great ebullition. In this way the phosphorus in the ingot is kept down to 0.02 per cent. To prevent the reduction of phosphoric acid, sufficient pig containing 2 per cent. of silicon is added in some of the works, to take up the oxygen in the iron and make silicic acid of the silicon, and the slag so formed poured off. In other works, in order to prevent the formation of carbonic oxide from the carbon of the spiegel acting on the oxide of iron in the bath, and consequent danger of the reduction of the phosphorus already in the slag, from 8 to 10 per cent. of a highly silicious pig is added before the addition of the spiegel or ferro, so as to be sure of the reduction of all the iron oxide. The slag thus formed is removed and the ferro added. As the addition of anything containing carbon is not desirable if it can be avoided, in order to add as little as possible, a ferro-silicon containing 10 per cent. silicon was for some time used with advantage. For rails they use 7 per cent. of spiegel; for ordinary work 3 to 4. This spiegel contains 12 per cent. of manganese.

I give below three tables of the chemical changes which take place during the operation, the first two made by Mr. Massenez, of Hörde, and published in 1880, the last recently made by Mr. Meier,

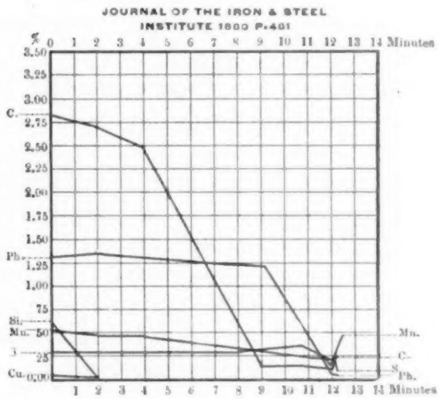


FIG. 16*a*.

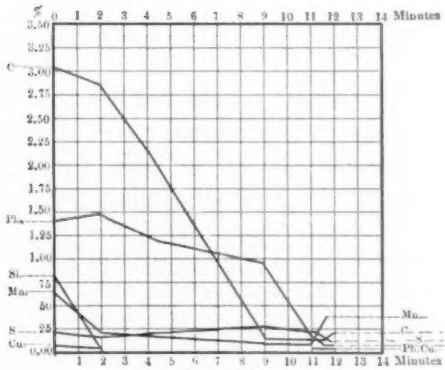


FIG. 16*b*.

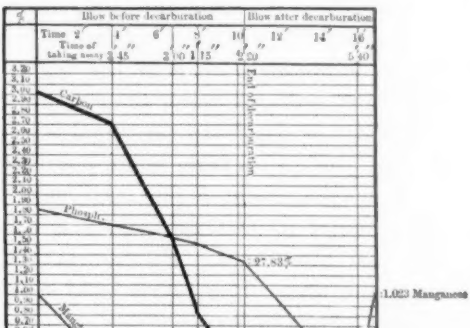


FIG. 16*c*.

of Dudelange, Germany, and communicated to me by Mr. P. Gilchrist.

These three diagrams do not differ essentially in their results. They were made at intervals of nearly five years, the last one having just been made.

All the slag is not turned out of the converter, so that when the contents are turned into the ladle the slag runs over the sides, making a disagreeable mess which attaches itself to the outside of the ladle. Sometimes when the iron has been cold it boils furiously both in the ladle and in the moulds, rising at times as much as 18 inches. It boils also when the charge is overblown or does not have enough lime. The cinder contains:

Iron	5	per cent.
Manganese	3	"
Silica	5 to 6	"
Phosphoric acid	18 to 29	"

The disposition to be made of the slag has been one of the most annoying of the questions to be considered. In most of the works it is turned on to the floor, toward the basic shop, and is always in the way. At Glengarnock, however, it is poured direct from the converter into a slag bogie, placed beneath it. The slag is received in a cast-iron receptacle, made of 10 staves which are plain on the inside, but reinforced at the top, bottom, sides, and middle on the outside. These are bound together by three round hoops, places for which are cast in the outside. These hoops have eyes at each end through which screw bolts pass. This cylindrical vessel is placed on an iron bogie. It is 60 inches in diameter at the bottom, 45 inches at the top, and 42 inches high. It has three handles on the outside with which to catch the crane hooks, to lift it off the bogie when the slag is cold. This slag bogie is placed under the converter and receives each successive discharge of slag, until the metal is ready to be cast. Fearing that the slag would slop from being poured from so great a height, a spout on wheels, about 5 feet high, was prepared, but no trouble has been found in pouring directly into the receptacle, and the spout has been abandoned. By this method there is no inconvenience from the slag. It is taken away when the blow is over, and is used in the blast furnace.

The following is an exact diary of one operation at Hörde, two at the North-Eastern works and two at the South Staffordshire works, which I took, watch in hand.

TIME OF BESSEMER OPERATION AT HÖRDE.

11.35 Converter turned up, lime put in.
11.39 Charge made.
11.44 Turned up, yellow and purple flame.
11.48 Flame quite yellow.
11.51 Commences to slop heavily.
11.52 Smoke becomes whitish, with a deep yellow border.
11.56 Flame small and little of it, carbon line gone.
11.57 Flame very yellow and murky.
11.58 Crops thrown in.
12. Turned down. Flame very red. Smoke almost black; *sample taken.*
12.04 Turned up.
12.05 Turned down; *sample taken.*
12.06 Slag run out. Flame almost purple.
12.08¼ Turned up.
12.08¾ Turned down; *sample taken.*
12.12 Crops thrown in, and spiegel charged.
12.15 Turned down for casting.

TIME OF TWO BLOWS AT THE NORTH-EASTERN WORKS.

3.20 Ladle covered with slag brought by locomotive.
3.22½ Pouring finished.
3.24½ Turned up. The flame clear and transparent, tipped with red at sides. Flame long, slightly purple.
3.30 Flame growing yellow.
3.31 Flame tipped with yellow. Silicon out.
3.33 Carbon lines in spectroscope.
3.38 Flame thick, yellow above, still transparent below; carbon lines; slag thrown out frequently; carbon in red and green points.
3.42 Carbon line gone. Flame short, transparent, reddish yellow, with large sparks of slag.
3.45½ Large amounts of iron oxide smoke. Flame short, thick, very full, and strong yellow.
3.47 Brown smoke, very thick.
3.48 Turned down; slag tapped; *sample taken.*
3.52½ Turned up; short yellow flame and much brown smoke.
3.54 Turned down. Length of after blow 1 minute 10 seconds; *another sample.*
3.57 One barrow of lime.
3.59 Turned up 35 seconds after blow; slag tapped.
4.03 Two per cent. of ferro-manganese, 80 per cent. manganese, 7 per cent. carbon; slag run off.
4.08 Turned down for casting.

4.03 Charge in. Converter turned nearly vertical; clear bluish flame, transparent, with reddish streaks.
4.13 Carbon line clear in spectroscope; flame blue and transparent at the mouth, yellowish at the tip; gets longer and thickens gradually; slag sparks carried very high.

4.19 Flame thick, yellow and transparent only a foot or two from the mouth; the converter turned up vertical; slag sparks very high and abundant.

4.20¼ Flame falling rapidly and becoming bushy; carbon out.

4.20 Flame short, reddish, brownish yellow, orange, transparent, and high shower of sparks; no slopping in either blow.

4.23 Short flame, transparent yellow flame; strong brown smoke.

4.26½ Turned down; slag tapped; *sample taken.*

4.34 One barrow of lime added.

4.34½ Turned up.

4.41 Sample taken.

4.52 Turned down for casting.

TIME OF TWO BLOWS AT THE SOUTH STAFFORDSHIRE WORKS.

10.33 Charge in. Converter turned up; reddish yellow flame; many sparks.

10.35 Slopping a little.

10.36 Flame has yellow borders.

10.37 Flame yellow with reddish streaks.

10.38 Flame yellow, almost entirely, only reddish at converter.

10.39 Flame yellow, tipped with blue.

10.40 Flame light yellow and bluish; slopping.

10.41 Flame light yellow, slopping a great deal.

10.43 Flame bluish yellow, tipped with yellow; slopping.

10.44 Brown smoke commences.

10.46 Flame edged with purple.

10.48 Blast increased.

10.50 Flame whitish yellow with purple edge.

10.52 Flame short, tipped with purple.

10.54 Flame very short, tipped with brown; much black smoke.

10.54½ Turned down; run off slag; cleaned mouth of converter.

10.56 Two buckets of lime thrown in.

10.58 Turned up; long yellow flame; reddish smoke; very smoky on sides.

10.59 Flame shortening up; very smoky.

10.60 Turned down; *sample taken;* spiegel brought up.

11.1 Slag tapped.

11.4 Another bucket of lime added.

11.5 Turned up; blast—short yellow flame, reddish tip and edge; much smoke.

11.6 Turned down; *sample taken.*

11.8 Slag run off, and cleaned from mouth of converter.

11.10 Another bucket of lime added.

11.11½ Turned up; full yellow short flame, much smoke, red edge.

12. Turned down; *sample taken;* slag tapped; lime goes out in lumps.

12.15½ Turned up for ten to fifteen seconds; spiegel added; rather cold.

12.18 Turned down for casting.

11.32 Lime charged; converter rolled.

11.30 Throw in lump coal.

11.40 Blow a little.

11.41 Turned down for casting.

12.5 Iron weighed; pulled to hoist; commence to raise it.
12.15 Begin to run in metal.
12.23 Iron in ladle turned into converter.
12.23½ Converter turned up; long yellow flame, changing to reddish.
12.28 Flame short, deep orange; sparks numerous; blast 22 lbs.
12.29 Blast 19 lbs.; iron slops; but little slag.
12.30 Great slops of slag.
12.31 Blast let down to 14 lbs. to stop slopping.
12.36 Blast 19 lbs.; flame thins, bluish; thick on sides, thin in the middle.
12.40 Flame short, with purple edge and tips.
12.41 Carbon line goes; smoke black.
12.42 Turned down; slag poured off.
12.44 Two buckets of lime added.
12.44½ Turned up; short full yellow flame; purple tip; much smoke; blast 18 lbs.
12.47 Turned down; *sample taken;* slag run off.
12.50½ Turned up; blast 15 lbs.; short thick yellow flame; reddish smoke.
12.51½ Turned down; *sample taken.*
12.53 Spiegel added.
12.55 Turned down for casting.

As soon as the converter is empty a careful inspection must be made of the bottom to ascertain its condition. This is done through the nose. When after five or six castings it looks worn, the iron plate which forms the bottom of the wind chest is removed and the thickness measured with the instrument made for the purpose. This is an iron rod, three feet long, with a stop near the handle to prevent its passing too far into the *tuyère* holes. The end is bent over a little, but so that the rod will pass into the *tuyère* hole. The distance between the bend at the end and the stop is the exact thickness of the bottom. The bottom on the inside is caught with the hook at the end, and the distance between the stop and the bottom of the plug on the outside shows how much the plug is worn away. In some cases the bottom of the converter, on being turned down, was found to be reduced to about seven inches in thickness. Many of the holes were stopped. The bottom was so thin that it was not safe to use it any longer. Preparations to take the bottom off were made just as soon as the converter was turned down and before it was tapped

They usually make six blows at the South Staffordshire works in twelve hours with each of the three converters. They should make about thirteen. These works are new and are hardly in good working order. The shortest time that it has yet been possible under ordinary circumstances to make a single blow has been forty minutes. This would give thirty-eight blows in twenty-four hours. There are so many unforeseen delays, however, that this limit has

not often been reached. At Bochum they have reached as high as thirty, but most of the works do not make more than ten or twelve blows per shift when everything is working well. It is expected that each converter will make 500 to 600 tons of ingot steel or iron before relining.

At the North-Eastern works they expect with the four converters to make forty-eight turns every twenty-four hours. They are actually making from thirty-six to forty-two. The plant was built to make 2,500 tons of ingots in a week. The converters turn either way. All the chains of the overhead cranes and of the chain pulleys are sent to the makers to be reheated every three months and tested to three times their capacity.

At Hörde the men are paid by the number of charges they make. Before commencing, 20 per cent. of partly-burned lime is put into the converter and the charge put on this. No slacked lime must be used, as it gives bad results. The experiment of blowing powdered lime through the *tuyères* was made, as it was thought that a more intimate mixture of the steel and lime would result, but it had to be abandoned. The converters are of ten tons capacity. They make nine to ten charges in a day of 12 hours.

When a basic converter has been relined they always use the spectroscope, but when it is going they turn down a number of times, thus ascertaining the state of the metal by trial. To find out the condition of things a sample is taken and the charge blown, if necessary, again. As an example, a charge was blown 3 minutes and 15 seconds after the carbon line disappeared and the spiegel added. It was then blown about five seconds to make the charge right. The rule which they adopt in working on a new charge is to blow until all the carbon is out, and then, if from experience it is known, or from the quality of the blow it is judged, that it will take five minutes to get rid of the phosphorus, to blow four minutes and then turn down toward the basic shop and run off the slag and take a test. It is always best to get rid of the slag whenever it is possible, and to add fresh lime. The men judge by the more or less granular or fibrous condition of the sample how long to blow. They have every interest in making good material, when they are paid by the ton. The average at the North-Eastern works was between .05 and .09 per cent. of phosphorus, out of a week's run; only one or two were as high as .10, and this from the direct iron. When the iron has over .12 per cent. phosphorus the men get no pay, when 0.10, half pay. It is quite necessary that the pig to

be treated should be uniform in phosphorus and silicon, for in this way the men acquire experience in treating it which they could not do if they varied. The iron billets contain .04 to .05 per cent. of carbon. Rails for India .35 carbon, for the North-Western Railroad .40+. The contract for the rails made with them requires that they shall not break by cold; the rails were made of very low carbon, so as not to break at −20° F.

At the North-Eastern works the twelve-ton converter turns down into its ladle, there being two transfer ladle cranes to command the four converters. This ladle is then transferred to the ladle crane, which is supported at the top as are all the others. The castings are made successively to the right and left of the pit, which is 60 feet in diameter. The positions of the transfer and the four ingot cranes are shown in the diagrams, Figs. 7 and 13. At 3 P.M. the ladle was full; at 3.01 it was transferred to the ladle crane and hooked in to prevent its turning over. The transfer is made by the ladle having two trunnions on the same piece. The transfer crane takes the outside. The ladle crane takes the inside and simply lifts the whole up. It is so balanced that it never tips, but to prevent any accident that may come from jar it is always hooked in. At 3.05 the first mould was filled. It took about 30 seconds to fill an ingot mould of about 1,000 lbs., and only a little longer one of 1,200 lbs. At 3.05 the sample was taken above the center mould. Only a few of the moulds boiled; scrap was put in, then a cover, then sand, and finally water, which effectually stopped the boiling in almost every case. At 3.10 all the moulds were filled. At 3.15 they tried to take the moulds off, but they did not come. After trying six it was given up. In the mean time the ladle had been carried around to the other side of the pit and the slag emptied. Such a ladle will last as many as thirty heats. If the skulls, as is usually the case, can be successfully extracted, they can be patched up. If not they will last only five or six heats. They are lined with acid linings, and there are four extra ones in repair. It has not been found that the acid lining affects the metal, or is in any special way affected by it.

At the Rhine Steel works the ends of the ladle stoppers are made of graphite. They are first made in a press to the proper shape. Then after a little drying they are put into a holder, the top of which has four holes to hold them securely, and the internal screw bored in. This takes considerable pressure, so that the clay comes through the holes. They are then burned. One man can

make 150 to 200 of them complete in 12 hours. They cost 18 pfennigs, and are sold for 25. The stoppers are screwed upon iron rods covered with clay, and then heated in an oven.

The arrangements of the various pits are shown upon the plate, Figs. 11 to 16. The old English plant, Fig. 14, had two converters turning down toward each other, with a ladle crane serving the converters and the supplementary cranes around the pit. The Holley plant, Fig. 15, made the converters turn down toward the pit, with a shallower pit and the ladle crane serving the converters, with supplementary cranes arranged like the others. The Bolckow, Fig. 12, has three converters in a straight line turning toward the pit, with two ladle cranes serving quarter circle pits, which are really on top of the ground, each pit served by two supplementary cranes, so that three converters have two ladle cranes and four supplementary cranes. The South Staffordshire works, Fig. 11, have three converters in a straight line turning toward the pit with two ladle cranes, a pit composed of two arcs of circles, and five supplementary cranes for three converters around the pit. The North-Eastern works, Fig. 13, has four converters in a straight line, each set of two converters being served by a transfer ladle crane which carries the ladle to the central crane, which is in a shallow pit, and is served by four supplementary cranes. At Hörde, Fig. 16, there are three converters in a straight line, turning down toward the pit, served by a loco-mobile crane, which first takes the slag and then the steel, and carries them where they are wanted.

At the works of the North-Western Railroad Co. at Crewe, the old English acid plant has been so modified as to be very compact. This has been done by bridging between the two converters and placing a turn-table for the bogie ladle between them. The cupolas are placed a short distance back. The floor level of the works is such that the converter bottom is put on from there, while the converter is nearly horizontal, the pit is very deep and has two levels, the ladle crane can be raised to either height. There are but two ingot cranes to each pair of converters.

At Glengarnock there are two nearly circular pits for the four converters, with a 15-ton ladle crane to each pit. There are five 8-ton cranes to the two pits, so arranged that three of them can be used in each pit. Both the ladle and the pit cranes are supported above, and are tied to each other and to the framework of the roof by girders. They run above on friction rollers, and are so

Fig. 12.

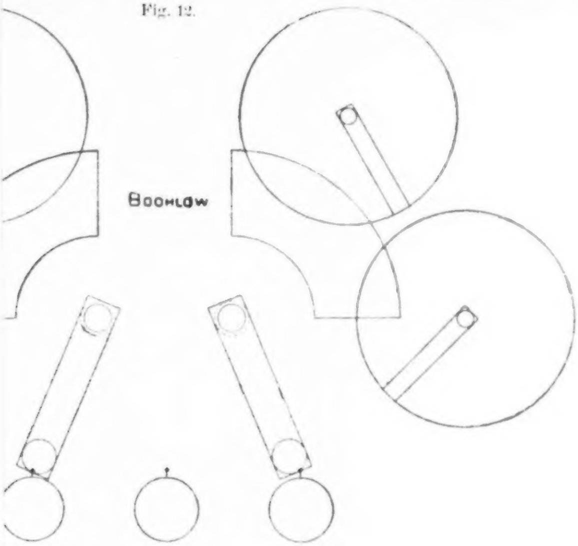
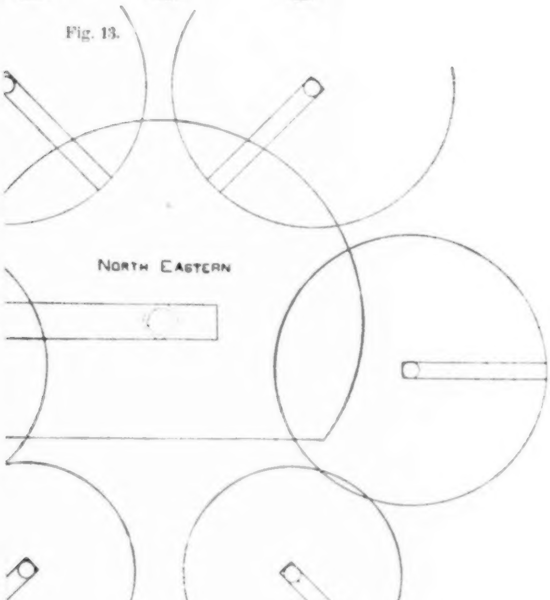


Fig. 13.



make 150 to 200 of them complete in 12 hours. They cost 18 pfennigs, and are sold for 25. The stoppers are screwed upon iron rods covered with clay, and then heated in an oven.

The arrangements of the various pits are shown upon the plate, Figs. 11 to 16. The old English plant, Fig. 14, had two converters turning down toward each other, with a ladle crane serving the converters and the supplementary cranes around the pit. The Holley plant, Fig. 15, made the converters turn down toward the pit, with a shallower pit and the ladle crane serving the converters, with supplementary cranes arranged like the others. The Bolckow, Fig. 12, has three converters in a straight line turning toward the pit, with two ladle cranes serving quarter circle pits, which are really on top of the ground, each pit served by two supplementary cranes, so that three converters have two ladle cranes and four supplementary cranes. The South Staffordshire works, Fig. 11, have three converters in a straight line turning toward the pit with two ladle cranes, a pit composed of two arcs of circles, and five supplementary cranes for three converters around the pit. The North-Eastern works, Fig. 13, has four converters in a straight line, each set of two converters being served by a transfer ladle crane which carries the ladle to the central crane, which is in a shallow pit, and is served by four supplementary cranes. At Hörde, Fig. 16, there are three converters in a straight line, turning down toward the pit, served by a loco-mobile crane, which first takes the slag and then the steel, and carries them where they are wanted.

At the works of the North-Western Railroad Co. at Crewe, the old English acid plant has been so modified as to be very compact. This has been done by bridging between the two converters and placing a turn-table for the bogie ladle between them. The cupolas are placed a short distance back. The floor level of the works is such that the converter bottom is put on from there, while the converter is nearly horizontal, the pit is very deep and has two levels, the ladle crane can be raised to either height. There are but two ingot cranes to each pair of converters.

At Glengarnock there are two nearly circular pits for the four converters, with a 15-ton ladle crane to each pit. There are five 8-ton cranes to the two pits, so arranged that three of them can be used in each pit. Both the ladle and the pit cranes are supported above, and are tied to each other and to the framework of the roof by girders. They run above on friction rollers, and are so

Fig. 14.

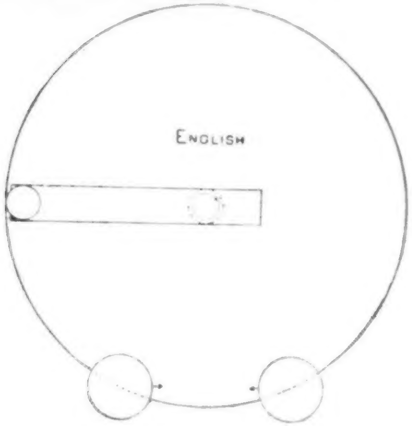


Fig. 15.

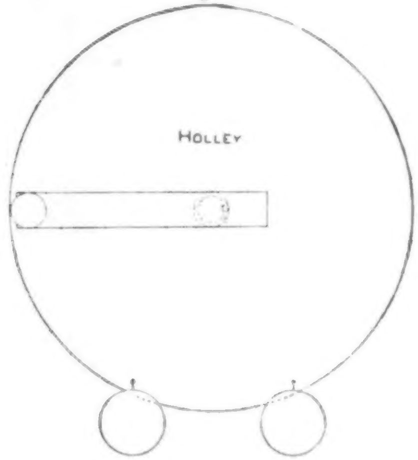


Fig. 12.

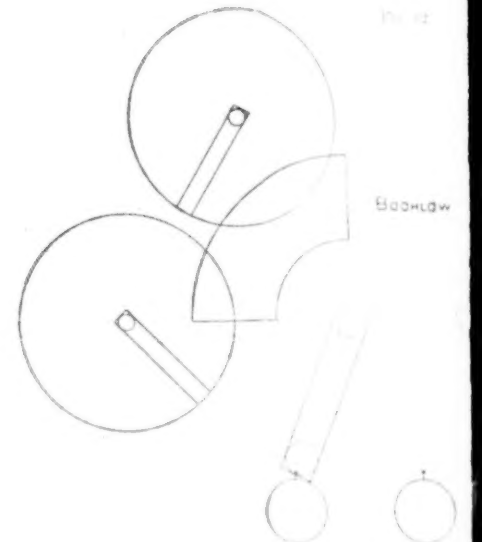


Fig. 16.

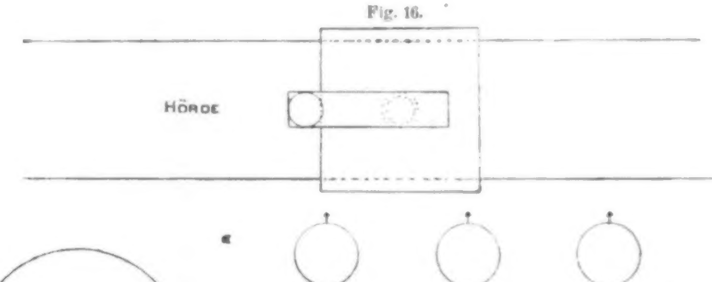


Fig. 13.

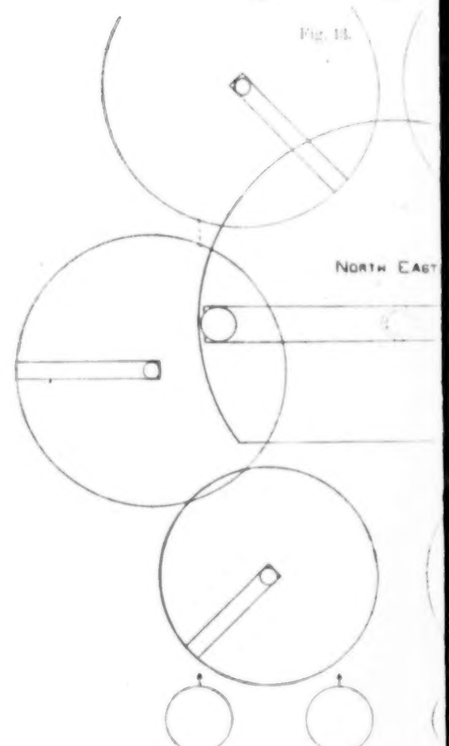
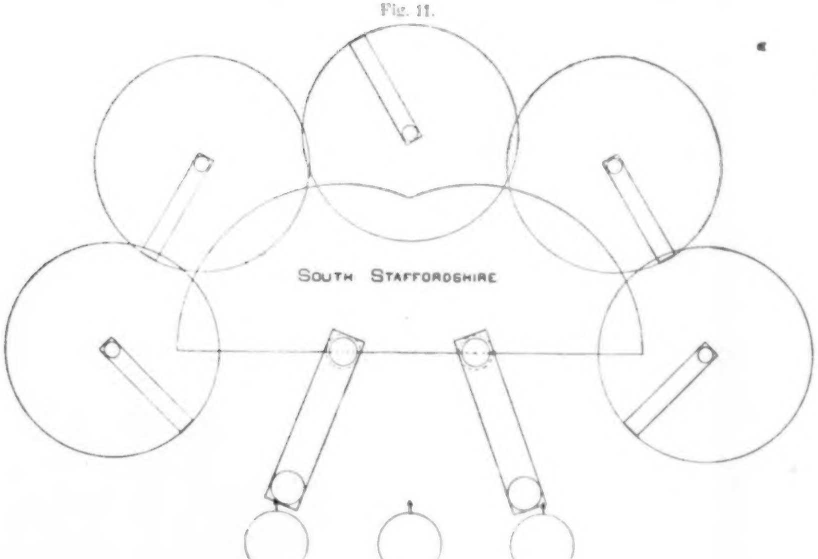


Fig. 11.



ARRANGEMENT
OF
BESSEMER PITS

Scale 10" = 1" = No 20

Fig. 15.

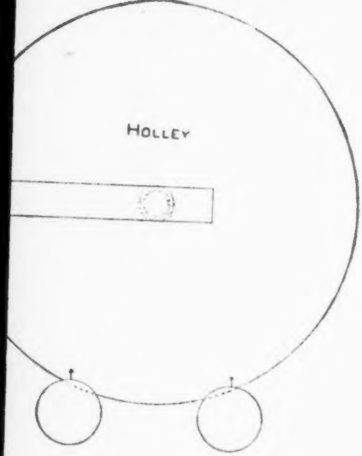
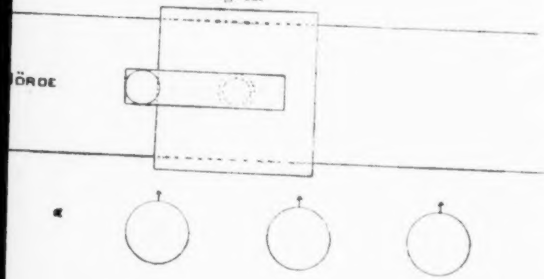
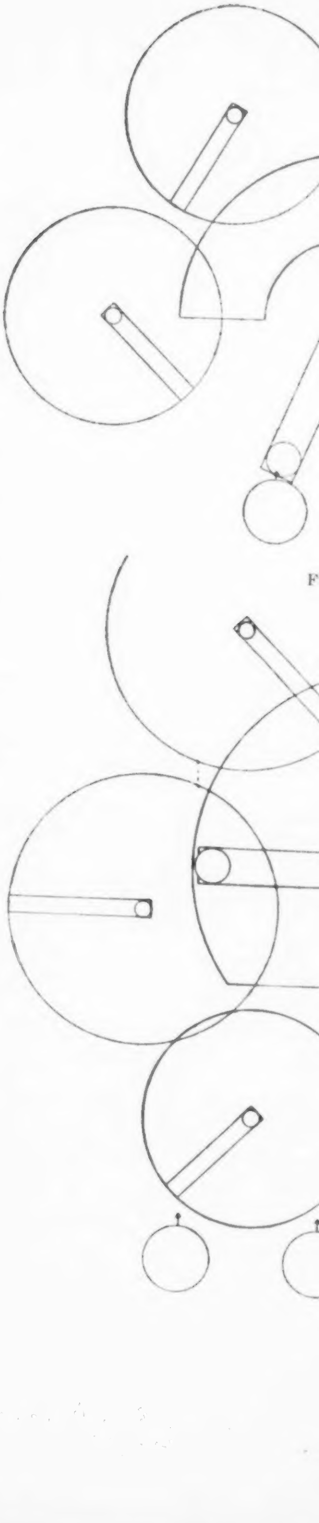


Fig. 16.





ARRANGEMENT

OF

BESSEMER PITS

SCALE 10'' = 1'' = 1/120

Fig. 12.

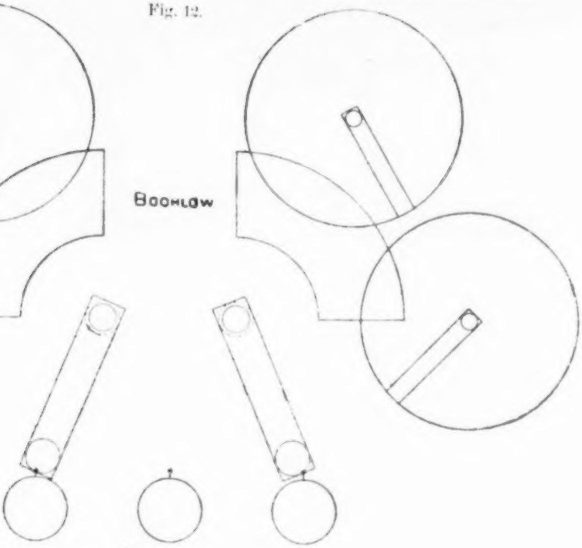
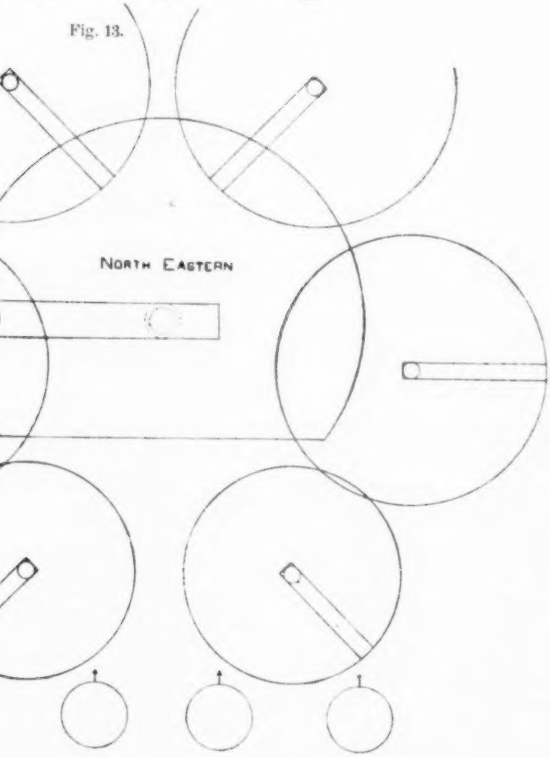


Fig. 13.



light that they are easily moved. Two of these cranes on each side are so situated that they can transfer the hot ingots to the soaking pit, and one has a jib so long that it can take the ingots out of both casting pits, and at the same time sweep both soaking pits, and land the soaking pit ingots on the actuated rolls leading to the cogging mill. One crane in each pit commands the sticker press. To the jib of one of these cranes a Duckham scale is attached, capable of ten tons, for weighing the ingots which have only partially filled the mould.

Of the pit arrangements the North-Eastern seems to be the best. The one at Glengarnock is very compact and convenient. The Bolckow one is very cramped, the South Staffordshire one less so; but of all the methods the Hörde seems to be on all accounts the best adapted for doing good work, for the reason that the converter and the pit are entirely independent of each other. I see no reason why there should be any connection between the two. The pit is a constant source of embarrassment, and a reason for delays, and for doing work which eventually has to be done over again. The casting house should be entirely independent of the converter, and so arranged that the casting may be done wherever it is convenient, and the stuck ingots arranged for, in such a way that they can be pressed out of their moulds at once, so that the mould can be used over again without delay. The skulls are all broken up to be used again.

When the ingots do not draw easily they are hammered on the sides with a sledge, and when they still do not fall they are struck with a mass suspended to the crane hook holding the mould, by two men. The moulds are swung on the crane to a spot behind it, where water is allowed to play on them until cool.

In some of the works the bottom of the ingot rests on a cast-iron plate with a depression in it, so that the ingot has a protuberance on the bottom. This makes the ingot roll out so that the crop ends are smaller. The plate is flanged so as to make the depression come in the center of the ingot. The edges are covered with sand, and, if the steel flows out, it is covered with wet sand. The moulds are generally easily removed, but sometimes they stick a little on the bottom. A blow or two will usually be sufficient to loosen them. If not, they are struck on the sides with the head of a crowbar. If they do not then deliver, they are struck on the sides with a sledge, and have either not cooled enough to contract sufficiently, or are held by some mechanical obstruction. They are

generally put back into the pit to give them time to cool, and are raised again and hit several sharp blows with a sledge. If this is not sufficient to loosen the ingot, the mould with the ingot in it is left suspended in the air. A large mass suspended from the hook which is attached to the chain of the ingot, by its chain is then rammed against the side with as heavy a blow as two men can give. If this does not loosen it, it is taken out of the pit and is either left to get cold or is sent still hot to the "sticker" press.

In a single cast, eight of the thirteen moulds at the North-Eastern works drew without pounding. The others had to be pounded. The thirteen moulds were all drawn in thirty-five minutes.

All the ingot moulds from which the ingots cannot be drawn are taken out of the pit and generally left to cool. The number of the blow is painted on them, and they are then treated differently in different works. In some, as at Bolckow, Vaughan & Co.'s, they are placed in a horizontal hydraulic press, and the ingot pressed out. At Glengarnock, the "sticker press" is made of very heavy casting, with brackets on each side, inclined so as to be 18 inches apart on the bottom, and 36 inches apart on the upper sides. They are thus large enough to receive the largest ingots. On the bottom a heavy step is cast the width of the thickness of the mould, which is just above a shallow pit. The mould support has a very wide base. The front is inclined at 80°. On the top a hydraulic cylinder, with a differential ram, capable of 50 tons pressure is placed. In this press the moulds are stripped so hot that they can go to the soaking pits. In the ordinary presses the ingot is cold before it can be put into the press. In these works one press is placed beside each pit and directly back of the soaking pits, and is commanded by a pit crane. In others, the mould is placed in a frame in an upright position, the bottom being 12 inches from the ground. A falling weight is made to strike the center of the ingot. In both these methods the object is to save both the ingot and the mould, but the latter is frequently broken by the miscarriage of the weight. It is the general experience with these "sticker presses" that at least 75 per cent. of the stuck moulds can be saved. In most works no attempt is made to save the mould. If it will not deliver it is placed under a drop, and the mould broken off from it. The number of the charge is then painted on the ingot, and it goes back to the works. The same falling weight is used to break up all the skulls and scrap made about the works.

In most of the works the moulds are poured from the top and

large ingots are made, but at Hörde the ingot moulds are very small, being hexagonal in shape, about 6 inches across and 32 inches high, and the system of bottom casting is used. It takes three-quarters of an hour to cast seven tons. By such a system the amount of loss is at a maximum. There are crop ends from the ingots and runners from the castings. These are all used again either in the Siemen's Martin's process or in a converter, but a diminution of output from the plant is the result. When the bottom runners are weak they sometimes burst, and I have seen a whole casting run into the pit from that cause.

Generally the ingot moulds are either on the sand of the pit or on an iron plate covered with sand, which is sometimes flanged so that it can be banked with sand. At the North-Eastern works the red-hot ingots are carried on small bogies to the reheating furnaces.

When the steel is very soft it sometimes boils furiously, running over the sides of the moulds. Such ingots are never full, and when the boiling subsides a little they are often refilled. All the ingots which boil have hollow ends. The red-hot ingots at the North-Eastern works and in most others are lifted from the pit by the crane and placed on bogies which it takes two men to handle. At Bolckow, Vaughan & Co.'s four are lifted on to an iron car, and a train of such cars is moved by a locomotive, and they are carried to the scales to be weighed. At the North-Eastern works these are about 100 feet distant. From here they are carried either to the soaking pits or to the reheating furnaces which are about 150 feet from the casting pit.

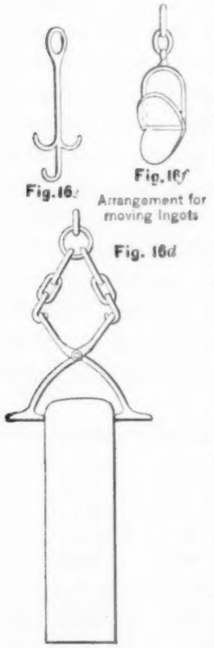


Fig. 16e

Fig. 16f

Arrangement for moving Ingots

Fig. 16d

At the West Cumberland works when the ingot is uncovered it is caught with the shears whose sides project, Fig. 16*d*. This is caught by one of the two hooks, one a ring, Fig. 16*f*, bent at right angles through the center, which turns on a swivel, the other, Fig. 16*e*, three hooks. The ingot in the shears is lifted out of the pit by the crane hung on an overhead railroad and carried to the soaking pits by two men. It is transferred to the soaking-pit crane from the overhead railway, and just before it reaches the bottom of the pit the chain is let go, and the force of the blow on the sides of the

shears forces the ingot out. All the transfers by crane are easily made with either of the two hooks.

The soaking pits are made in two ways, one with bricks and the other in steel castings like an ingot mould, but four inches thick. Both of them have double covers. The brick ones are built as they usually are and need constant repairs. The others need almost none. The mould is put on a foundation of fire-brick packed two feet thick. The casting is placed on this and rammed on the outside with fine broken brick. When cold it takes five to six heats to bring them up, when hot it takes half an hour to get them ready for the rolls. When once hot they are kept so from Saturday to Monday without difficulty. When not in use coke is put in to keep up the heat and to keep out the air. They intend to go from ingot to cogging mill without heating. These pits are made of any size to suit the ingots they are to receive. At Glengarnock there are twelve 18 × 18 inches, and 5 feet 6 inches deep, and six 30 × 30 inches, and the same depth. From the soaking pits the ingots are carried either by an overhead railroad, or by actuated rollers as at Glengarnock, the object being to get them to the mill as quickly as possible. The ingots for tin plates are cogged, taken to the hammer, cut up into small pieces, re-heated and rolled to plates.

Where there is an acid and a basic plant together, as at Angleur, the Rhine Steel works, Hörde, and other places on the Continent, no difference is made between the basic and acid products. The ingots all go to the rolls at the same time and cannot be distinguished. No attempt is made to mark them. When the work has been done properly, they claim a little less phosphorus for the basic than for the acid steel. The basic steel contains only a trace of silicon.

The slags which form during the operation contain from 16 to 26 per cent. of phosphoric acid. The average composition is given below.

	Creusot.	Hörde.
Lime and magnesia	54.	52.
Alumina, etc.	5.	2.58
Oxide of iron and manganese	11.	19.2
Phosphoric acid	16.	19.33
Silica	12.	6.23
	98.	99.94

They are treated differently, according as the works are acid works transformed or built for the purpose. In the former case they are dumped into the pit, and are always in the way. When

the works are new the rotation of the converter allows the slags to be tipped away from the pit toward the house where the refractory materials are made. It falls in a heap out of the way. It can be cooled with water at once and taken away, and this is the method at the North-Eastern works. At Eston and Glengarnock the slag is not dumped on the floor but in an iron car, and is carried away at once. At Hörde it is tipped into a special ladle on the ladle-crane and carried to some designated place. At most of the works in England the slag is thrown away. At Eston and Glengarnock it is used in small quantities in the blast-furnace charges for Thomas pig. At Hörde they are all sold to a company who have purchased the patent right to treat them for all Germany.

This treatment is very simple. The slags are first crushed, in doing which a considerable amount of scrap steel is recovered. They are then ground fine and treated in large tanks with weak hydrochloric acid, in order to dissolve out all the phosphates without attacking the silica, which is easily done cold. The slag is kept constantly in motion. When the acid has done its work and before the silica is attacked, the liquid is run off, and strained milk of lime is added until the material is very nearly neutral, tests being made at very short intervals. This precipitates the phosphates as an impalpable powder, which is at once pumped through a filter-press. The water containing the lime chloride is allowed to run off. After washing, the phosphates are dried and sold for manure. It is not only valuable from the quantity of phosphoric acid which it contains, but because its very fine state of division makes it readily attacked in the earth, and easily absorbed by the roots of the plants. The residue in the tanks still contains six per cent. of phosphoric acid, but in order to recover that there would be danger of attacking the silica. It also contains 45 per cent. of iron. It is sold after washing to the iron manufacturers, and is an excellent material for making Thomas pig. The process costs but little and yields a large profit.

The arrangement of the mills is different in all the works. In all the newly constructed ones the chief object has been to do as much of the work by mechanical means as possible. The plan of the South Staffordshire mill is shown in Fig. 10. The North-Eastern, which is one of the later ones and the best-planned mill that I have visited, is built on a long strip of land, one end of which is the bank of the River Tees, where everything arriving by water is landed. The mill is situated about the middle of the plot. The

reheating furnaces are situated about 100 feet from the converters to the left. There are four of them, with a wood-paved space in front of them. Each set of two furnaces has a chain actuated by raising the hydraulic press vertically. Opposite each of the four doors of the furnace is a hydraulic piston with a wheel and chain for drawing out the ingots. The chain is fastened below, passes over the wheel at the top of the press down the side to a wheel which changes the motion to a horizontal one, and then in and out over four pulleys. The object of this is to draw the hot ingots out of the furnace. The lift has four of these pulleys on each side of it to be used for separate furnaces. The furnaces have four doors. To put an ingot in, the bogie is run up against the open door. When the ingot goes in part of the way a bar bent at right angles with a roller on the angle and crowbars are used to get the ingots to their place. Coal is piled up in front of the door to burn any air that enters there. The furnace is fired with coal and blown. The waste heat is used to make steam. There is 40 feet clear between the furnace and the lift, so as to give plenty of room to manipulate the ingots. The ingots are brought just as hot as they will bear transportation from the ingot pit. At Hörde these furnaces have eleven doors and hold 26 of the small-sized ingots which they make there at a time. They were trying the experiment of using water-gas for heating, making it with superheated steam. It was not very successful, although it works well at Essen. At the North-Eastern works the ingots are all large, and the furnaces hold only 12 of them. On the right and just in front of the cogging mill are two sets of four soaking pits. Each pit is seven feet deep, and seventeen inches square. It has an iron top and a double iron cover. The pits are commanded by a light hydraulic crane, with the top supported, which swings to the mill. When these pits are used, the ingots are soaked in their own heat for about half an hour, and then swung to the cogging mill. This is a very heavy one, moved by a heavy reversing engine. The ingot is passed through each pass of the rolls twice. They are 36 inches with a lift of two inches. They are eight feet wide and have seven passes. The housings of the rolls are very heavy. On both sides of the mill are large actuated rolls to move the ingots. The cogging rolls are only two-high, but work up to 400 tons in twelve hours. The rolls are raised and lowered by hydraulic pressure, a quarter section toothed wheel acting on a full one. The tables in front and behind have five

rollers, beyond these, a set of actuated rollers which carry the slab or bloom to the bank, if it is to be sent away, or to the roughing and finishing rolls about 100 feet away, if it is to be worked up. If it is to be sent away as ingot, the two ends are cut by the saw, when it drops on to a car in a pit holding an old-fashioned scale-rack on a bogie. This is actuated by a wire rope about fifty feet distant, caught by a steam crane, the end caught by a hook. It is carried where it is to be deposited and dumped by letting out the suspension chain, the hook holding the bottom part. Two of these cranes run in and about the works. One man handles the crane, another attends to the adjustment of the pieces to be lifted, and dumps them when they arrive at their destination. These locomotive cranes do all the work of the mill. The charging of the red-hot ingots is automatic, and also their transportation to the point where the cranes take them. As these cranes are obliged to be near certain points all the time, they get their water from pits in the center of the track, on the outside of the building in which there are water plugs. The tanks for the water supply are, as is quite usual in England, on the top of the engine buildings. To the left and beyond the cogging mill are two furnaces like the others with a double-wheeled hydraulic press, so that one press can serve several furnaces now used for heating ingots, but probably to be used for heating blooms. Over the cogging mill and engine there is a heavy overhead crane commanding both and the full width of the mill. The engine is here to the right. The roughing and finishing rolls have a reversing engine to the left. They both have actuated rolls on the floor in front of them. The roll engines in almost all of the new works are reversing, the reversing being done by hydraulic machinery. On the left of the engine is the roll-turning shop, also commanded by the overhead crane. The engine is entirely surrounded by a brick wall about four feet six inches high. The engineer's house is in the middle over the engine, and commands a view of all that part of the mill. The roughing rolls are in a direct line from the cogging mill, and the blooms come directly to it. After passing these rolls it is transferred by four automatic pushers across to the finishing rolls through which it is made to pass. The finishing mill is there composed of two pairs of 28-inch rolls.

At Glengarnock, the cogging mill has a pair of rolls 7 feet long and 36 inches in diameter, worked by a reversing engine. The rolls are raised and lowered by a small steam engine placed on top

of the housings, and within easy reach of the platform from which all the motions of the mill are controlled. The rolls take 15-inch ingots and reduce them to 3 × 2½ inches. The ingot, while being rolled, is pushed forward and tilted by four hydraulic cylinders, so arranged that the ingot can be turned over, made to miss one, two, or even three passes, if desired, or the ingot may be raised and made to travel horizontally with or without being turned over. The mechanical devices about these rolls are so very ingenious and simple that with two men and a boy their capacity is stated to be 200 to 250 tons per shift of 12 hours.

At the South Staffordshire works, where they make plate exclusively, the rolls are very large and heavy. In all the new works the engine-house commands a full view of the works. At the North-Eastern works, and also at Eston, the policy is to have as few crop ends as possible. Whether short or long iron billets or rails are made, the ingots are rolled out to pieces which are often over 125 feet long. To provide for this length there are two inclined planes, one on each side of the rolls. Behind it is very steep, the pieces being rolled passing at times over the overhead crane; in front the inclined plane is rather lower. At Eston it is flat. The piece is received on rolls which go only a certain distance and are then stopped, the next roll taking the motion, and so on. On passing the last pass it goes to the side of the inclined planes and is carried on actuated rolls to the saw. Here it is cut to any length. A single ingot generally makes five rails of 24 feet. If the pieces are short, as wire billets, they are dropped upon a wagon below the surface of the mill and carried out as the blooms. If the pieces are long they are caught by the locomotive cranes and carried to their place either in or out of the works. At Eston the work is done by overhead cranes which, as here, command the whole of the rail plants. The rails are cut up and are pushed to their place by a wagon carrying a pusher, which pushes the rails or bars to right or left, but allows the wagon to pass under in one direction so as to shove on the journey back.

At Eston the rails are moved to one side or the other of the mill, as required, by a truck actuated by a wire rope attached to the drum of a reversible engine. None of the carrying is done by men. It is all done by small locomotives and overhead or locomotive cranes. The rails are not curved, but straightened cold with ordinary gags.

Messrs. Thomas and Gilchrist gave the cost of making one ton of basic steel in 1882, in England, as follows :*

	Cost per Ton. s.	d.	Weight. cwt.	Cost of Ton of Ingot. s.	d.
Labor				3	6
Coal	5	6	4.5	1	3
Coke	12	6	0.75		5.75
Ingot moulds					10¼
Lime			3.33	1	10
Ferro-manganese	30	11	.11 lbs.	1	6
Refractory material				3	4
Repairs				2	0
Interest and sinking fund at 10 per cent.				1	0
General expenses and royalty				4	6
Total cost				20	3
Waste			3.5	7	4¼
Cost, including waste				27	7¼
Cost of pig				42	0
				69	7¼

These figures are given for direct work. Remelting would add 3s. 6d. to 4s. 6d. per ton more to the amount.

The waste in the process will vary from 14 to 16 per cent. The amount of the basic additions will be from 15 to 20 per cent. of the pig ; the spiegel 4 to 6 per cent. The lining used will amount to 150 lbs. per ton of steel produced ; extra labor, 4 to 6 pence. The difference between the cost of Bessemer and Thomas pig will depend on the locality.

With regard to the quality of steel there seems to be no doubt now that not only low carbon steel and ingot iron can be made, but also all the varieties which can be produced by the acid process with any percentage of carbon and with great uniformity in their composition with both low phosphorus and silicon. The silicon, sulphur and phosphorus may always be made lower, while the manganese will generally be about the same as in the acid process under the same conditions. These amounts may be regulated so as to produce any quality of steel, and the only reason why the basic has not taken the place altogether of the acid process is, first, the extra cost of the refractory materials, which is every day diminishing, and also the inferiority of output occasioned by the time which is required for the after-blow, and also by the cost

* Journal of the Society of Arts, London, April, 1882.

of repairs. The result of twelve tests made in September, 1884, at the North-Eastern works showed the lowest tensile strain to have been 23.53 tons, and the highest 28.34; the lowest elongation, 23 per cent., the highest 30; the lowest reduction of area 45.9 per cent., and the highest 59.3. There is no doubt that higher qualities could be made if the demand for them was sufficiently remunerative.

With regard to the advantages of the construction of such works, it may be said that they are equally well adapted, with changes of lining, for the production of either basic or acid steel. The peculiarities which are necessary for the basic are great conveniences in the manufacture of the acid. The method of transferring the ladle gives every advantage in the pit, and does not in any way increase the difficulties of the manipulation of the plant. In fact, there does not seem to be any reason why there should be any pit near the converter. Experience has shown that the liquid steel as well as the iron can be transported considerable distances. The pit is a great disadvantage to the work of the converter, and the converter to the pit, and there seems no reason why they should be kept together. With the casting shop separate from the converter there would be plenty of room and little concentration of heat, so that the men could work more freely and with more effect. The experience gained at Hörde shows that there is no necessary connection between them. They might better be some distance apart than to be connected. The control gained by discharging the slag into a ladle instead of on the ground is also a great gain. Even if the slag has no commercial value, getting it into compact form makes it easier to handle and leaves the space under the converter free at all times. The method of giving complete rotation to the converter and arranging it to move either way, so that the slag is tipped toward the basic shop instead of toward the pit, is an advantage, but does not equal the complete removal of the slag in one mass, which is perfectly manageable, as at Glengarnock, and therefore necessitates the use of fewer men. The method of removing the different parts of the converter on hydraulic wagons, leaving the pit always free, and the use of overhead cranes for manipulation and adjusting all parts of the apparatus, needs to be seen to be appreciated. The rapidity with which all parts can be moved from place to place and put into position again removes the difficulties of much of the manipulation. With a sufficiently large basic shop, and steam capstans, the transfer takes but a few moments. The objection to the

basic process is the deficiency in output, which in many places must nearly counterbalance the profit which comes from using an inferior iron. When a few engineering difficulties are overcome there seems no doubt but that the basic will successfully compete with the acid process. With regard to the great cost of refractory materials per ton of steel, the future lies in reducing the cost by either cheapening the present process or finding a more enduring material. There does not seem to be much prospect of cheapening the process so long as stamping the slurry into moulds for the bricks or for relining the converter, which must be done by hand, is considered essential. When all the parts can be compressed by hydraulic machinery, it would seem not only that the work would be more uniformly done, but that the strength of the bricks to be burned in moulds and of the parts to be burned in place would be very much greater and the work more rapidly done. There seems to be no reason why the manufacture of a brick from the mould to the oven should take more than five minutes. The manufacture of the magnesia bricks by hydraulic machinery at Hörde and elsewhere has demonstrated that a very small brick about a quarter of the size of the ordinary tar brick can be rapidly and cheaply made on a machine but poorly adapted to it. With well-designed machinery the bricks could be well and rapidly made, and with larger bricks and machinery adapted to the purpose, the same number of large bricks could be made as of small ones, and they would be much more uniform. Even the rammed portions might be better done by machinery than by hand alone. There might, I think, be a considerable economy effected in the burning of all the materials used. The system of burning them in non-continuous furnaces is almost universal, but with a large production there is no doubt in my mind that a continuous furnace of the Mendheim type * would be better and cheaper. It is not an uncommon thing to find the plugs, which are very expensive to make, split entirely through when taken out of the furnace, from too rapid firing or cooling. Not unfrequently they are taken out of the furnace red-hot, and are damaged by the sudden exposure. A uniform refractory material can only be made by uniform treatment. This means equal compression of the material of which it is made and proper time for cooling and the same method of treatment. By using a furnace like the Mendheim, where the temperature can be applied only in

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* Basic Refractory Materials, Trans. Am. Inst. Min. Eng., Vol. XIII.

a definite way, and where the heat absorbed by the material in burning is given up to the heating of the gases which produce the combustion, this is done with great regularity and uniformity. There are none of those sudden increases or decreases of temperature which are so fatal to the manufacture of good refractory materials. Not only would the bricks be better, but the shrunk dolomite would be better and much more evenly calcined. The reduction of cost seems to be much more likely to be effected in this direction than any other.

The probability of finding some other and better refractory material for this purpose is not great. The most promising material as yet tried is magnesia, but that has failed because it has been found to be too expensive. If its manufacture could be cheapened, as it seems likely from the manufacture of magnesia from sea water,* or if the bricks could be made to last longer, the effect would be produced. The action of wear, which is both chemical and mechanical, is more the latter than the former. The way to improve seems, therefore, in the direction of making something more solid than has yet been made. On the supposition that all the reactions have been properly performed and the magnesia properly worked, compression seems to be the only way to effect this. That it has once failed, and has been abandoned, is no conclusive argument, when the defect seems to have been a want of proper mechanical appliances. The same may be said of the artificially prepared dolomite where no proper dolomite can be had, but this extra cost must be counterbalanced by such processes of manufacture as to make the brick last longer.

The basic plant can always be used on acid materials. It therefore has a very great advantage over the acid process, and with the cheapening of the refractory material, which of necessity must come with a demand for it, the probability is that in the near future there will be a greater difference in profit in favor of the basic process. There are, however, a number of conditions which must also be fulfilled. It is not easy to find an iron containing the requisite amount of phosphorus, neither too high nor too low, that fulfills all the other conditions. It must therefore be said that the basic pig at the present time is not easy to obtain. It will, however, become easier as the blast-furnace practice which is required for its manufacture from ordinary ores becomes better understood. The experiments now being made in Wales, to reduce the amount of silicon in the

* Basic Refractory Materials, Trans. Am. Inst. Min. Eng., Vol. XIV.

pig, are of the greatest possible interest. The difficulty lies between too high and too low phosphorus, and this is one which will undoubtedly be overcome, and in many districts could be overcome by melting irons of different grades in the cupola. There is a saving, too, in using direct iron. This, however, can only be done with safety where there are a number of furnaces, so that accidents in the quality and quantity of the supply could be guarded against. It would, however, not be safe to do without the cupolas, which must be ready to do duty where for any reason the direct iron fails.

The quality of the metal which is produced is altogether exceptional, and can be made quite as suitable for making ingot iron as steel. It can be produced with great uniformity and regularity when the same kinds of pig iron are treated in the converter. When different kinds of pig iron are treated it requires a considerable amount of testing, which is also true of any other process. It has been proved by experiment on a very large scale that material suitable * for plates, for chains, for wire, and for rails can be made of such a quality as would compete with the products of any other manufacture.

The length of the time between the pig-iron from the blast furnace and the manufactured steel, whether rail or plate, or whatever it may be, need not be, at the outside, more than one hour, and is usually about half that time. It might, therefore, be expected that this process would be immediately introduced on a large scale, but the greatest impediment to it is that the world has already all the steel it requires, and a further production, for competition, must be made under conditions which will presuppose a very decided lowering in the price of the material. Up to this point the basic process has not fulfilled these conditions. Whatever may be the future of the basic process, it has stimulated the manufacture of refractory materials to such an extent that we shall undoubtedly get them of a much higher quality, and much lower cost, than we have ever been able to obtain, and if the process does nothing more than to cheapen refractory materials it will have conferred a great benefit upon metallurgical processes in general.

In conclusion, I wish to express my thanks to Mr. Finlay Finlayson, of Glengarnock, for information, and my especial indebtedness to Mr. A. Cooper, of the North-Eastern works, and to Mr. P. Gilchrist for special facilities for studying this very interesting process in the works.

* Jour. Iron and Steel Inst., 1884, p. 412.

DISCUSSION.

The President.—I am very glad that Prof. Egleston, who, as you know, is a prominent member of a sister Society, acknowledges here before you all, that it is the mechanical engineer who must take charge of the production of this newer metal, that it is to the mechanical engineer you must look for its use and adoption, and that it is the mechanical engineer of the present and of the future, to whom these various problems of its manufacture must be turned over.

Mr. Durfee.—I have no hesitation in saying that this is one of the most valuable papers that has ever been brought to the attention of the Society. But it is a matter of great surprise and regret to many men who are familiar with metallurgical progress abroad, that the American manufacturers of steel have so far ignored entirely the advantages of the basic process. I think there has not been an attempt made in this country to carry it out commercially. I believe that at Steelton they did make a trial of it; just exactly what their methods were I do not know. But, notwithstanding the reputation which the American people have acquired for enterprise in mechanical matters, we have in many cases hastened exceeding slowly. In the matter of the introduction of the acid process, parties who were pecuniarily interested in the patents hung fire and neglected to take any special interest in it for eight or ten years. Of course, in that time they were getting the advantage of the blunders of other people. But at the same time it was a notable instance of want of enterprise on the part of some of the more prominent of our iron and steel manufacturers.

There is one matter in connection with this paper in which I have considerable personal interest, and that is the soaking pits. I believe in no works in the United States are they in practical use yet, notwithstanding the fact that they have been successfully employed in a large number of places abroad. There is a large saving in their employment there. There is no reason why there should not be a corresponding saving here. But I have exhausted my eloquence and persuasive powers with some of our leading manufacturers of steel in the endeavor to induce them to take the thing up. They are full of good intentions. In fact, they suggest the statement which was made in regard to the place that (following the new version) we must call Sheol, which is said to be "paved with good intentions." In regard to the soaking pits there has

been more of that kind of paving used for their foundation in America than is creditable to the country.

The President.—I simply want to say one word, as I do not think that many gentlemen here are familiar with the fact. There has recently been constructed in Joliet a new plant for the handling of ingots and the rolling of rails. Very little has been said about this yet, and I do not know just how much the proprietors care to have said about it; but I had the pleasure of witnessing its operations recently, and it certainly works very nicely, and it is surprising to see how very few men are there employed in producing steel rails from the ingots.

Prof. Egleston.—With regard to the use of soaking pits, very much of the success now being achieved in increasing both the output and the quality of the steel in the Basic-Bessemer is owing to their use. They are making them in the new works three times the size they formerly made them. As one of the results of their use they are now rolling ship plates of more than double the length and width formerly made with about the same labor and fuel, and were it not for the soaking pits and the mechanical appliances connected with them, I do not think it would be possible for them to make them of such size for about the same cost per ton. The method merely turns now on getting the ingots out of the moulds so hot and into the soaking pits so quick that they can go from the soaking pits without any other reheating to the finished product. Where these plants are now being used the object is to make everything as long as possible in order to do away with the crop ends. Every rail that is rolled in many of the new works is rolled from 130 to 150 feet in length, so that in five or six rails there are only two crop ends. It is the avoiding of these crop ends that is diminishing the cost of manufacture. It does not make any difference whether the product is wire billets, or rails, or angle iron, or whatever it may be, the pieces are cut from the long bar to the sizes wanted. The mechanical ingenuity which is required to do this is of the very highest order that the profession can furnish. I do not think that any of the young mechanical engineers can do better than to study such a plant as would be required to make say seven rails in one length. That would require that the one length should be about 220 feet long. Seven rails with two crop ends means that all the labor and money which were formerly spent in making one thing now go into the finished product of one with only a slight increase in the time required to make the one, and that the crop ends

are reduced to a minimum of time, labor and material absorbed in them. I only know of one works where the crop ends are wanted, and that is in the acid open-hearth process, and in those works Whitworth told me that as he needed scrap in order to treat his pig and could not purchase any pure enough for his purpose, he had to make it; but this is a very exceptional case. Generally people make scrap by accident, because they cannot help it, not because they want it. I have seen scrap made by having the whole charge go into the pit; but the capital represented by the steel, went to the pit at the same time. It is the work of the soaking pit, it is the avoidance of crop ends, it is the mechanical appliances of and attached to the rolls, it is the shears that cut up the big plates—and I wish I had time to go on and tell you about some more things that, depending on the ability of the mechanical engineer, make the profit of the modern steel works.

The President.—What the Professor says about scrap, reminds me of the foundryman who said "he thought it was a very poor foundry that did not make all the scrap they ought to use, themselves."

The treatment of ingots, as now observed in many steel works, seems to me (and I do not profess to be a steel-maker) simply barbarous. That a metal so subject to strains, internal and otherwise, by the slightest change of temperature, should be subjected to the great differences of temperature that ingots now are on being taken out of the ingot mould, being cooled on the ground, hauled off and reheated again, is simply barbarous. I hope our friend, Mr. Durfee, will be able to teach our steel-makers that when they have a hot ingot, they should never let it get cool until it is put on board the cars in the shape of rails.

CLXXXIX.

RAPID TRANSIT AND ELEVATED RAILROADS,

WITH A DESCRIPTION OF THE MEIGS ELEVATED RAILWAY SYSTEM.

BY FRANCIS E. GALLOUPE, BOSTON, MASS.

It would be difficult in a paper of reasonable length to treat a subject having so wide a bearing as that of rapid transit, with the exactitude and thoroughness of detail which might be expected in a technical article. The following notes are therefore with hesitation submitted, in the hope that the incomplete form and in some cases the mere suggestions which only can be presented within the limits of this paper, may be accepted in place of a more extended treatise.

The modern demand for increased facilities of transit is two-fold. First, there exists an imperative need of better means for the conveyance of passengers within all our large cities, making the problem an universal one, although its attempted solution has thus far been local. Second, there is the more general demand for more rapid means of communication between cities and important centers of population or business.

It is the present purpose to show what the existing requirements are, as indicated by their gradual development, for obtaining with safety a higher speed of transit, and how the problem may be met as it arises, first, locally, and then for transportation through longer distances.

The endeavors made to supply these wants are seen on every hand. Probably most would agree that the time of increased facilities for transit is coming. The world will never go back to slower speed. The tendency is indeed precisely opposite; that is, to save time, shorten working hours, and to concentrate the volume of transactions in centers of business or of trade. Whatever this progress of business and of life demands will be developed and put in practical use.

Fifty years ago there existed only the very beginning of the present great development of the surface railway system, which has cost

in the United States alone nearly seven thousand millions of dollars, and employing three hundred thousand men, with an extent at the end of the year 1884, of 125,379 miles.* They transported last year 334,814,529 persons, and earned in gross $770,684,908, with interest and dividends paid to the amount of $269,939,137.

Previous to this the most rapid methods of transit, still within the memory of older men now living, were only post-riding and the now primitive stage coach. Not even the horse car had been invented. Later on, the horse railroad system took its place in the streets of our principal cities, and although not developing much increase of speed, its great convenience, as well as the economy shown by the introduction of the principle of carrying passengers by rail, as compared with any other method of land transportation, has caused the growth of this system to the extent of many miles of track and great perfection of detail. In Massachusetts, the present extent of the street railways is 310 miles, as compared with 2,851 miles of steam railways in the State; their value $12,410,-631, carrying 94,894,259 passengers in 1884, and employing 3,846 men and 8,996 horses, as compared with about 16,000 employees on street railways in the whole country.

The horse railroad has had so important an influence in the building up of suburbs and extension of the growth of cities as seemingly to have become an absolute necessity; yet so great are the present objections in blocking the streets, failing to supply sufficient accommodations to the public, and loss of time by the delays incurred by passengers, that in the East, at least, it is becoming the general opinion that its limit of capacity and usefulness has been reached nearly if not fully.

While this system has been growing and other methods of obtaining relief from the crowded state of the streets and the consequent retarding of transit have become established, such as the London underground railway and the Vienna depressed railways, a system of elevated railways has been developed in New York City of which the results attained in the short period of time since 1872 have been extraordinary. Not only have these demonstrated the fact, not before proved or deemed hardly practicable, that a complete steam railroad system could be run upon the tops of a line of posts set in the streets, as in the Bowery line, with entire safety,

* From Poor's Manual of R. R.'s, 1885. Cost of Roads and Equipment, $6,924,554,444. First Railroad completed in Mass., 1827; first locomotive run Aug. 8, 1829.

speed and convenience, but the permanent success of the principle has been, I think, fully demonstrated. A short statement of their progress is inserted, from a recent paper. "During the first year the roads carried 170,000 persons, and during the past year nearly 100,000,000." "The first year's earnings were $17,000; last year nearly $7,000,000." "There was a steady progress each year." "The aggregate earnings since the road was first built have been $32,000,000; the aggregate passengers carried 444,000,000."

Such being the facts, let a moment's glance be given at the local conditions existing in cities. Experience has shown that ease of communication in the transaction of business requires its concentration into the least possible space. A street too wide for business purposes is more detrimental than one too narrow. The result has been the erection of five, seven and even nine story business blocks, which, with the general introduction of fast running elevators, supply the demand for offices and warerooms, and are more valuable for business purposes than lower floors farther removed from the business center. Now, with this great concentration and consequent increase in the volume of business through the streets, the capacity of the streets themselves has not been increased proportionately.

The result has been a blocking of the streets to a large extent, and the obvious remedy, if the height of buildings is doubled, is to have two-story streets, so to speak, *i. e.*, to relieve their crowded condition and divide the travel by some form of elevated railroad which shall take from the surface that portion of it which desires merely transit as quickly as possible, and thus relieve the one portion from its blocks and convenience the other.

We must either have rapid transit *upon the surface, under it, or above* the surface of the ground. The first is impracticable, for reasons to be shown later on, while the second is open to the same objection, on account of the limited field available caused by its excessive cost.* For general usefulness, the only feasible method is the third.

Objections to this remedy have been of two kinds: first, the alleged damage to property adjacent to an elevated line of railway; and second, the sentimental one of injury to architectural features of the buildings. The first should be at once recognized where

* The proposed underground railway for Broadway, New York, is estimated to cost from $800,000 to $1,400,000 per mile, for single and double tracks, respectively.

real damage exists, and met so far as practicable by the road. A new element has been introduced with the elevated railway, disturbing the existing business relations and property interests, which latter cannot defeat the railroad, but which must be readjusted after the introduction of this new element. The injury done in places, whether the abuttor's land is held to end in the edge of the sidewalk, as in New York law, or extends to the center of the street, should be compromised between the railroad and property interests, in equity, by the payment of damages, in case either of direct damages by land taken, or of consequential damages, if proved that the rent or income from the property is thereby diminished; but these cases are only incidental in comparison with the great and lasting benefits to the public at large. As to the second objection, it may be said that the demands of transit should be first met, with as little loss in other respects as possible. The primary use and purpose of the streets is for transit, and not for the display of the architectural features of the buildings lining them.

Opposition to these necessary facilities for transit, while somewhat surprising when the great benefits to be derived from them are considered, is yet to be expected when we review the history of the introduction of railroads to supersede the turnpike and the stage coach, the introduction of the horse railways even, or that of any of the great improvements, such as many in the progress of manufactures, which have destroyed the value of some class of property which they supersede. All such must, in the end, give way to the public need.

While it has been found that the elevated system is best adapted for long-distance travel, *i. e.*, for distances exceeding a mile, the reverse is true of the horse railway, which will still be found better fitted for the accommodation of some portion of the short distance passengers than even the elevated railway. Where the time required for conveyance is short and speed therefore not an object, considerations of convenience will still lead the short-distance passenger often to prefer to step upon the cars of the horse railway, which goes directly where he wants to go, instead of climbing up a flight of steps into an elevated railway car which may not leave him so nearly at his destination; and especially will this continue to be the case if, as now seems likely, its service becomes improved by the use of electric motors, in the near future.

For the increase of transit facilities, certain definite requirements should be met in any successful system. These may be re-

garded as those of, first, *safety*; second, *speed*; and, third, *convenience and economy*.

The leading features of the surface railway system, viz.: first, *the rail and car*, for the reduction of required motive power and dead weight carried per passenger to their least amounts; and second, *the truck system*, having independent moving trucks, coupled, supporting upon them the platform and body of the car, should be retained.

Under the requirements for *safety*, should be noted, 1st, safety from derailment, since next to railroad accidents occurring to persons crossing or walking upon the tracks, which are not reported, more than one-half of all reported railroad accidents are from this cause; 2d, safety from obstructions upon the track. These consist of passing teams, trespassers and cattle, rocks and timber falling upon it, wash-outs, which are of the nature of obstructions; in winter, the blocking of the tracks by snow, drifting of the same, and many other causes resulting from railways built at grade, or upon the surface of the ground; 3d, an efficient brake system should be provided that will act automatically should the cars break apart or other derangement occur; 4th, appliances to give the engine-driver positive and absolute control not only of the engine, but over the movement of the entire train.

Among the requirements for any material increase of *speed*, are, 1st, those insuring at least equal safety to that now existing under the increase proposed, such as holding the truck upon the rails by flanges or their equivalent, so that no derailment can possibly occur by the trucks lifting or jumping away from contact with the rails; 2d, the center of gravity of the engine and cars should be lowered and the stability of rolling stock increased, to prevent strains which would overturn them; 3d, more secure attachments between the truck and car body should be provided, to prevent the momentum of the car body from breaking away from the former; 4th, there should be provided an improvement in the design of motive power, especially by the use of independent means for producing adhesion of the driving wheels to the rails; or a controllable and variable adhesion, not dependent upon the weight of the engine for the pressure of the driving wheels upon the rails; 5th, a consequent saving of weight both in engine and cars, with the same power of engine, and reduction of the dead weight carried per passenger, should be reached; 6th, for speed, a clear line to be provided, with no crossings at grade, and the use of an efficient block system.

For the attainment of *convenience and economy* the system used should be adapted:

1st. For curves of shorter radius than have been heretofore practicable, especially in cities, where streets are narrow; and for through lines, a better alignment as to grades and curves, made possible frequently only where the track is raised above the surface. 2d. For economy in repairs, by possessing freedom from wash-outs or settling of the ground, and from the decaying of cross-ties.

It will be seen that most of the above requirements can be met and the result in view reached only by the employment of an elevated system. It is the belief of the writer that all steam railroads, excepting perhaps those only for freight having speeds of less than ten miles per hour, should be elevated from the surface of the ground because of the many advantages of such a construction, as will be shown more fully in the further discussion of this subject. In Massachusetts, a resolve of the Legislature,* referred to the Railroad Commissioners, looking to the feasibility of a gradual abolishment of all grade crossings in the State has already been passed, and this may be regarded as a first step in the direction indicated.

To show that many or all of these conditions may be fulfilled in concrete form and may exist practically, the problem will be illustrated by the selection and brief description of one of the several distinct systems, each containing some excellent features, that have been proposed, namely, that of the Meigs elevated railway system, now under construction in the city of Cambridge, Mass.

This plan, invented and developed by Captain J. V. Meigs, of Lowell, Mass., as the result of over ten † years' careful study of the surface roads, their advantages and defects, is unique in that it is a complete system, one part absolutely depending upon the others, and having little or no analogies in the surface roads.

It may be regarded as a development from the New York elevated system, taking for its starting point the fact only that a railroad can be built and successfully run upon a single line of posts.

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* "*Resolved,* That the Railroad Commissioners examine and report to the next Legislature upon the subject of providing for the gradual abolition of grade crossings in cities and the populous parts of towns." [Approved, April 19, 1884.]—*Report of R. R. Coms.*, 1885.

† Application for patent filed May 16, 1873; issued May 11, 1875. Earliest notes made in 1867 or 1868.

Objects.—The Meigs elevated railway system is designed to meet the modern demand and all the ordinary requirements of a railroad for the safe, quick and convenient transportation of passengers in cities, or to and from the suburbs of cities to such central points as passengers desire to go; while on longer lines, connecting cities and towns, to carry both passengers and freight not only more economically and safely, but more speedily than has heretofore been done.

The New York structure is essentially an ordinary railroad, elevated, and is open to the criticisms of liability to derailment and consequent want of safety; a too great height of the position of the center of gravity, and want of stability, by reason of the large leverage upon the posts when sustaining wind pressure upon the sides of the cars; and the well-known obstruction to light and air produced by the size of the structure in the street.

The fundamental principle of construction in the Meigs system is to *concentrate the strains due to the load upon the track directly upon the central line of the way*, avoiding all disadvantageous leverage. It is, in effect, to turn the ordinary track up edgewise, or vertically, with one rail lying directly over the other, instead of side by side in a horizontal plane, as in all other railroads. Or, it is as though the Y of the posts in the New York system as well as all the cross-ties, nine feet in length, were abandoned, and the double girder beneath the track condensed into a single central truss, removing four-fifths of the material causing obstruction to light and sight from the street.

Its general appearance is shown by Fig. 17.

In the execution of the design based upon these peculiar conditions, the roadway consists of a single lattice iron girder or truss, four feet in depth, and resting upon iron posts or columns placed 44.4 feet apart.

Distinguishing Features.—Its peculiar features and differences from the ordinary railroad exist in—

1. The Way;
2. The Switch;
3. The Trucks;
4. The Passenger Cars;
5. The Engine;
6. The Draw Bar, and
7. The Brakes;

as in the following description.

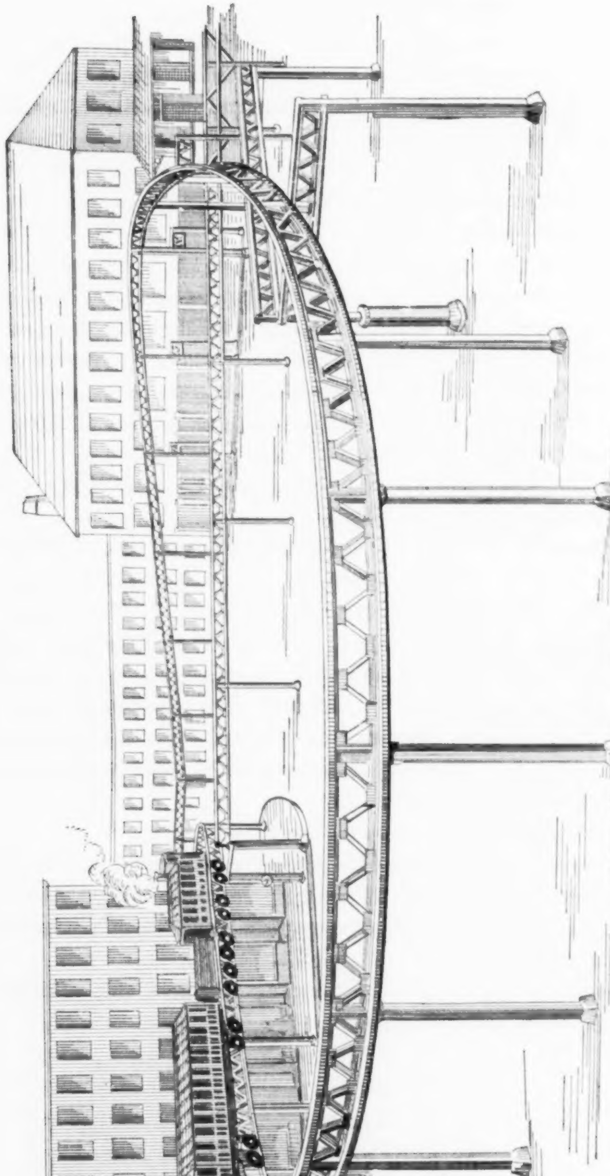


Fig. 17
GENERAL VIEW

I. The Posts and Foundations.

The posts may be of wood, rough, hewed or sawn square, for the wooden track system; or of iron when the iron way is used. In the latter case they will be of rectangular form, about 11 inches by 10 inches in section, and 24 feet in length. They are composed usually of two medium 10-inch channel bars, *f f*, Fig. 22, and two

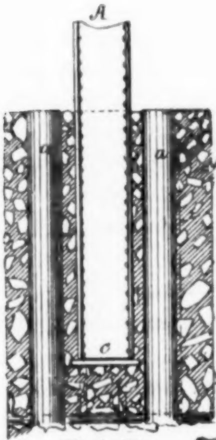


Fig. 18

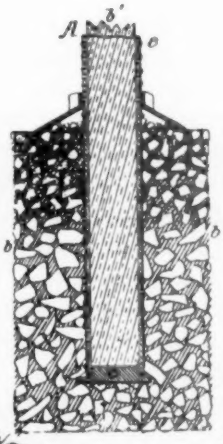


Fig 19

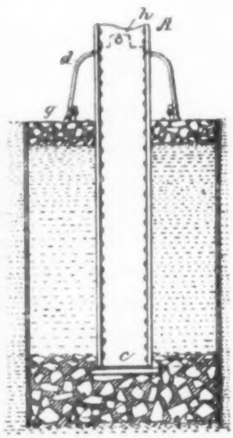


Fig. 20

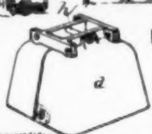


Fig 21

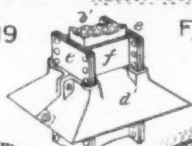


Fig 22

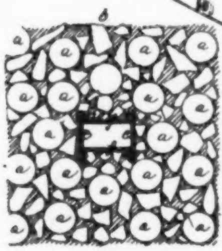


Fig. 23

Fig. 24

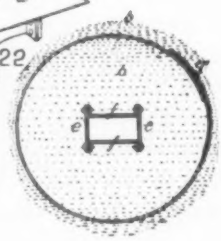


Fig 25

plates, *e e*, all about ½ inch in thickness, riveted upon and along the flanges of the channel bars and the edges of the plates. They thus form a hollow box-like structure, which may be varied in cross-section or thickness in special places, or may have the solid plates replaced by diagonal bars riveted in lattice form upon the channel bars. The weight of each post, having a sectional area of

23 8 square inches, is 1,919 lbs., the crushing load 235 tons, and safe load 39 tons, while the greatest load that will be imposed on a post in any position of a passing train will not exceed 35 tons.

The foundations, as shown in Fig. 18, this and all subsequent figures being in scale $\frac{1}{32}$ of full size, or $\frac{3}{8}$ inch to the foot, being a vertical section, and Fig. 23 a horizontal section of one form, consists of a plate *c*, upon which the post rests, of somewhat larger area than the post, as shown in Figs. 18 and 20, or of a similar plate *c'*, as shown in Fig. 19, which has an upwardly presenting boss entering into its interior, set in and on a concrete foundation about 3 feet in diameter and 6 feet in depth. The lower part of the posts may remain hollow, or they may be filled with concrete, *b'*, or with sand or other non-compressible filling, as shown in Fig. 19.

If the foundation is upon soft earth, the earth is packed, as shown in Figs. 18 and 23, by driving piles, marked *a*, all around the place where the post is to be set, and filling between, and over them, if necessary, with the concrete.

Where this is not necessary, another plan, shown in Figs. 19 and 24, is more usually followed, in which the post hole is simply filled with concrete and broken stone below.

Where the ground foundation is good, but the surface soil is mobile, as in the Mississippi valley and elsewhere, in which case side thrusts are not well resisted, still another method, shown in Figs. 20 and 25, may be employed, consisting of a stout resistant boxing or lining to the post hole, of iron or wood, as shown at *g*, and filled with concrete or sand inclosed between concrete ends, thus forming a strong side support to the post, practically increasing its section below the ground many times. At the surface of the ground the posts may, if advisable, be additionally braced or guarded from abrasion and injury by passing vehicles by means of caps or collars, *d* or *d'*, shown in Figs. 21 and 22. These are either of cast or wrought iron, made in two parts, and bolted together and to the post by means of ears or bolts passing through the post.

These posts, so set, at a distance of 44.4 feet apart, will be amply sufficient in strength to carry safely a girder capable of supporting substantially such trains as are now in general use, at a height of at least 14 feet from the ground to the bottom of the girder.

II. The Way.

The way upon which the train runs, Figs. 26 and 28, consists, as before stated, of a single iron girder or truss for each span, 4 feet

in depth, placed centrally over the line of posts, and comprises an upper track-beam, a lower track-beam, upon the sides of each of which are carried the rails, and between these track-beams the bracing or trussing. At the proper height for resting the lower track-beam the bracket angle-irons, *i*, are riveted upon each post,

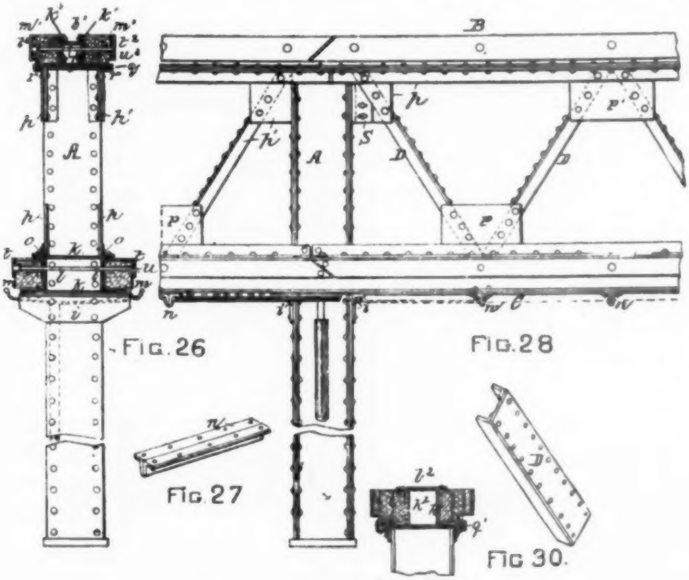


FIG. 26 FIG. 28 FIG. 27 FIG. 29 FIG. 30.

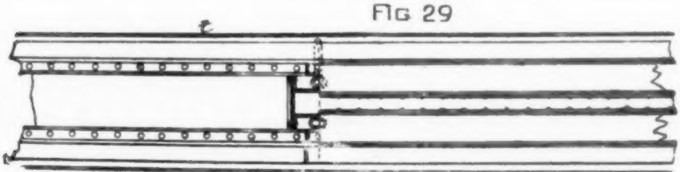


FIG. 31.

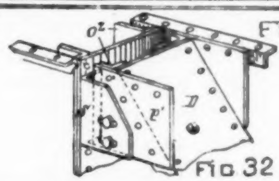


FIG. 32

and on these irons the lower portion of the girder rests. The upper track-beam rests upon the extreme top of the posts, and through the diagonal braces supports the trucks and cars, whose weight is carried upon the lower track-beam.

The lower track-beam of the girder, *C*, is a box-beam composed of two channel bars, *l l*, Fig. **26**, to which are riveted flat plates,

k k, which in turn are securely riveted to the post sides by angle-irons. In the exterior recesses formed by the channel bars are imbedded wooden beams, *m m*, one on either side of the line of posts, which may be single sticks or composite beams made up of several pieces, and which act as rail stringers. Across the girder at intervals of about 2 feet are riveted **T** or double angle-irons, as shown at *n*, Fig. 28, in order to bring the entire strength of the iron into action.

In the angle formed by the upper plate, *k*, of the box-beam, *C*, with the channel bars, *l*, on its outside, the angle-irons, *O*, are also riveted. These may be made much deeper than shown, so as considerably to stiffen the girder and serve as attachments for the braces, *D*; or, as shown, attachment plates, *p*, are inserted at the proper intervals and riveted to the angle-irons, *O*, and diagonal braces, *D*, composed, as shown in Fig. 30, of two angle-irons and a plate, or of channel bars, or **I** beams, attached by similar plates to the upper track-beam.

The upper track-beam, *B*, of the girder, is also a box-beam composed of two channel bars, *l′ l′*, and two exterior angle-irons, *k′ k′*, re-inforced by angle-irons, *r r*, all well riveted together and carrying in the recess formed by the exterior angle-irons the stringer beams, *m′ m′*, of solid or compound sticks of timber, for supporting the rails, as described.

An expansion joint, Fig. 32, is formed at the end of these upper track-beams, by means of a bracket, *S*, firmly riveted to the post, to which the brace portion, *D*, and the attachment plates, *p′*, are fastened by bolts passing through slots of sufficient length to allow for the ordinary expansion or movement of each section of the way, due to changes of temperature.

The lower surface of the upper box-beam rests upon a terminal plate at the top of the post, which thus takes its weight, so that these brackets in the slip-joint serve only as guides. The upper beam may also be made up as shown in Fig. 29, in which *q′* is the lower plate connecting the two lateral channel bars of unequal flanges, k^2 k^2, and l^2 a top plate to form the box-beam. The purpose of using wood for the track stringers is not only to form a continuous support for the rails and a convenient substance for the attachment of the rail fastenings, but to assist in the preservation of the alignment of the way due to its freedom from expansion by heat, and also to aid the iron work in resisting longitudinal stresses upon the girder, as in braking a train. It also supplies required

elasticity for rails and girder, saving the iron from unnecessary wear from knocking and pounding, in several important respects.

The rails of this structure are four in number; the two bearing rails, which carry the load of the car, being angle-irons placed upon the outer upper edge of the stringers, *m*, upon the lower track-beam. They are marked *t* in the figures, and are fastened to each other, to the stringer-beams, and to the lower track-beam of the girder by through-bolts, *u*, as shown. The upper track-beam also carries two vertically placed rails for the balancing or friction wheels, lettered t^2 t^2, and are similarly held to the stringers, which project over the line of posts and braces and have a small recess beneath them under which flanges upon the horizontal wheels run to securely lock the truck upon the track.

The horizontal distance from outside to outside between the lower rails which is found to be sufficient for transverse stiffness is 22½ inches, which thus constitutes the gauge of the road and the total width of the way with its rails in the street; the corresponding gauge of the upper rails being 17½ inches.

It is anticipated that the form of rail section in common use will be eventually adopted in permanent constructions, in which case the lower wooden stringers may be chamfered off at their upper outward corners to take the rails, whose axes would incline at an angle of about 45 degrees with the vertical. The length of the posts, 24 feet being usually sufficient, occupying 4 feet for the truss and 6 below the surface of the ground, giving 14 feet clear way beneath the truss, will be varied to follow the grades and contour of the ground; and at freight houses the girder may be sunk below the surface 2 feet, to facilitate unloading upon low platforms or into teams, or remain at grade, the platforms and road being raised the same amount.

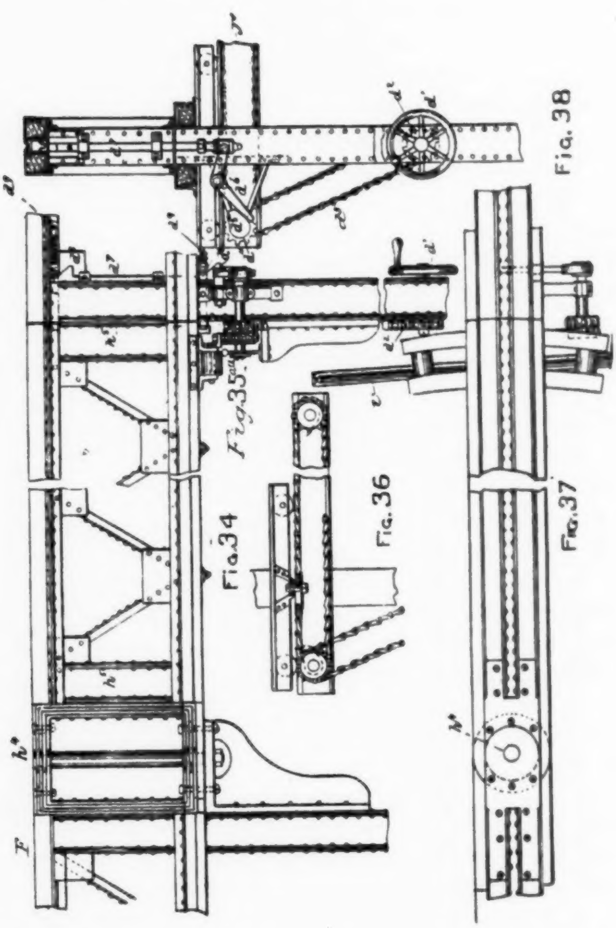
The present cost of building the permanent way is about as follows:

Iron	Way, with	high posts	$70,000 to $75,000	per	mile.
"	"	" low posts	50,000 to 55,000	"	"
Wooden	"	" angle-iron rails	20,000 to 25,000	"	"
"	"	" round posts and sawed wooden rails,*	6,500	"	"
"	"	" hewed track-stringers and hard wood rails,*	4,500	"	"

* Estimate of H. Haupt, C. E. In a timber country and with the cheapest possible wooden construction throughout.

III. The Switch.

The switch, Figs. 34–38, consists in simply swinging a single section of the way upon a peculiarly constructed and very strong hinge attached to one of the posts.



In Figs. 34 and 37, F, and the continuance beyond the other post, d^9, are portions of the main line; h^5 h^5, the section composing the switch; h^4, its hinge, the whole being shown in plan in Fig. 37.

The movement of the switch must be sufficient to enable the cars and trucks on one track to clear the end of the rail on the

other track, or some 4 or 5 feet. When operated, the free end is swung over upon the supporting carriage provided with rollers traveling upon the supporting rail *v*, being operated by suitable chains and rag wheels by means of a winch or hand wheel, d^1, Fig. 38, and locked in position by a locking device shown in Figs.

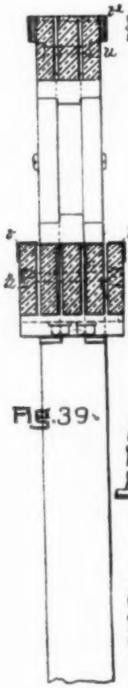


Fig. 39.

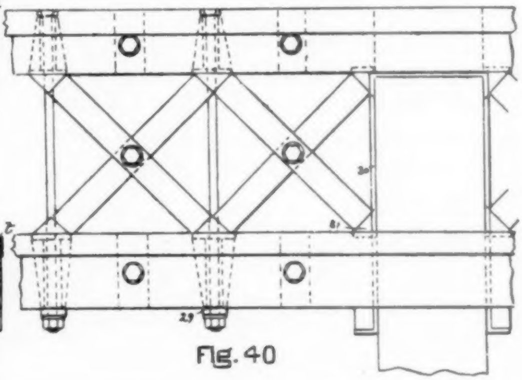


Fig. 40

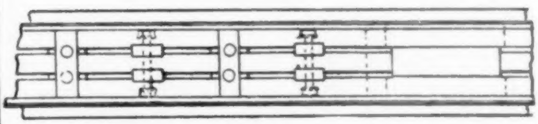
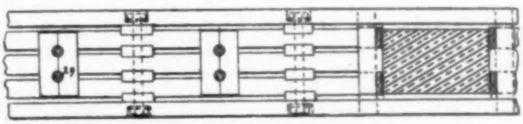


Fig. 41



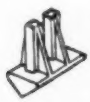


Fig. 42.

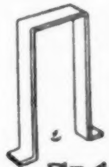


Fig. 43

Fig. 44

35 and 38. Fig. 36 shows the connection of the rag-chain with the free end of the switch and its carriage.

The hinge is strongly bracketed out from the post at the end of the main way, *F*, as shown in Fig. 34, and consists of a series of curved iron plates arranged concentrically around the pivot-pin or pintle, h^4. One-half of these plates are attached to

the swinging section and one-half to the post, the series on each being composed of alternate layers of concave and convex ended plates which shut into each other so as to retain the strength of the track at this point and yet form a hinge. In operating the switch, the first movement of the hand wheel withdraws the locking spring-latch, d^9, from engagement with the girder, by means of the wedge-cam, d^8, and this device is also designed to be operated automatically by a moving train.

A singular effect of the inclined position or re-entrant angle of the truck wheels, described later on, has been found to be such that in running upon a switch left open even to the extent of 15 inches, the switch will be closed by the train, thus increasing the safety of this feature.

The angle formed at the pivot or hinge end need never exceed 5°, for by dividing the throw of the switch equally to right and left of the center line of main way, the length of switch truss required would not exceed 35 feet, which is a very moderate length.

IV. The Wooden Way.

A form of construction for the permanent way of much less cost than that built entirely of iron, is illustrated in Figs. 39–44. It consists of a wooden Howe truss, Figs. 39, 40 and 41, which may be set on posts of any shape, *t t*, showing the rails as before. Fig. 43 shows straps of wrought iron passing over the tops of the posts, upon which the lower beams of the compound girder are hung, being in this manner supported by the posts, thus avoiding the cutting and loss of strength in the posts. Figs. 42 and 44 are cast-iron chairs for the end bearings of the diagonal wooden braces of the truss. The trussing may be altogether dispensed with by placing the posts near enough together, as it may be a more economical construction in some cases to adopt.

V. The Truck.

The truck which has been found best adapted to the peculiar form of way has been designed and constructed as a development of the considerations governing the adoption of the permanent way Fig. 45 shows an end view, Fig. 46 a side elevation, and Fig. 47 the plan view. It consists of a horizontal rectangular wrought-iron frame, 1, Fig. 47, stiffened by cast-iron pieces, 2, Figs. 45 and 47, and provided with stiff cast pedestals, 4, 5, 6, bolted to its

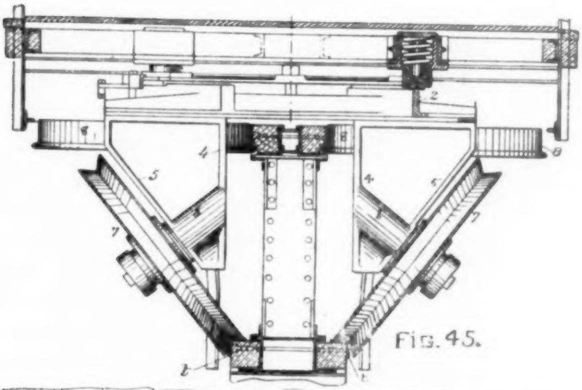


FIG. 45.

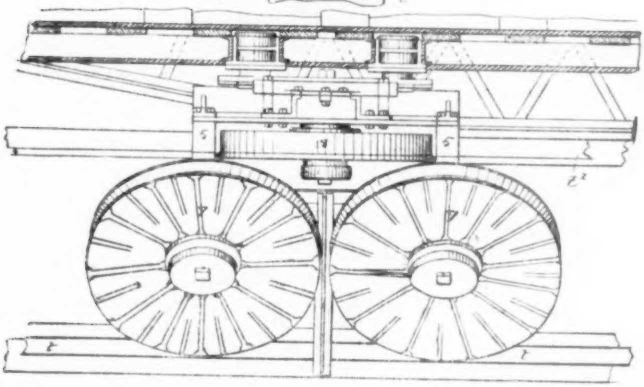


FIG. 46

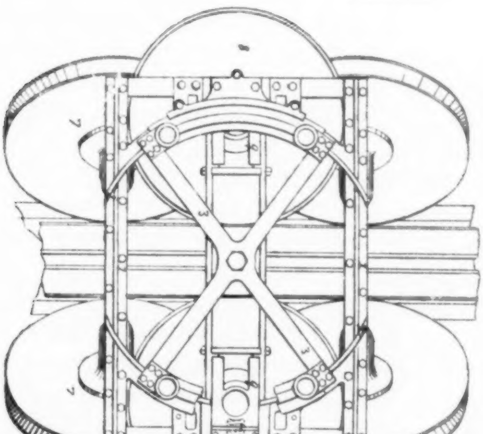


FIG. 47

under side, in which are fixed short axles for the wheels. The supporting wheels, 7, of each truck are four in number, and have a notched rim or right-angled groove which fits the angle-iron rail upon the upper corners of the lower track-stringers, being placed at an angular position of about 44° 50′ with the vertical, so as to run upon it, the axles being inclined.

Between the supporting wheels are two horizontal wheels, 8, one on each side of the girder, upon vertical axles attached to the truck frame, and bearing upon the vertical rails on the upper track-beam of the girder. These move to a limited extent in sliding boxes, *q*, Fig. 47, to which their axles are affixed, are kept in yielding contact with the rails by springs outside the boxes and serve the purpose of balancing wheels to take side oscillations of the cars. They have flanges which lip under the lower edge of the rail-plates and thus tie the truck to the rails so that no lifting or jumping can take place, or the possibility occur of the trucks running off the track.

The truck wheels, which are large, being 42 inches in diameter yet light and strong, have a broad tread of 3½ inches upon each bearing face, and rotate independently of each other upon large fixed axles surrounded by a loose sleeve which divides the friction. They are lubricated by oil carried within the axles, which are hollow, so that the journals constantly run in a bath of oil, none of which can drop out by reason of caps tightly screwed over the hubs of the wheels upon the under side.

Between the supporting wheels on either side of the girder are strong safety braces of T iron, extending from the pedestals, 5, to points opposite yet so as to clear the rails by a small amount; and since the wrought-iron frame of the truck comes immediately above the girder, should any or all of the truck wheels break and even fall off, the frame would fall but about an inch before resting upon the girder, forming a strong shoe which would slide upon but could not leave the way, or allow the cars to overturn. That is, even in case of the breakage and absence of all the truck wheels, the framing alone could not leave the way, without lifting it through a space of over four feet, the entire depth of the girder, and this shoe is made sufficiently strong to maintain the cars, even without the wheels, in position upon the way.

Upon the top of the truck frame is a wrought-iron movable frame, 3, Fig. 47, of segmental shape, carrying four spring posts containing heavy spiral springs, the posts interlocking beneath

their upper flanges with similar spring boxes or sockets securely bolted into the floor framing of the car, which comes directly above the truck, within 18 inches of the top of the girder. A center-pin serves to guide the turning of the truck beneath this upper frame, and horizontal flanges of the truck frame castings overlap the periphery of the upper turn-table, thus as effectually tying the car body to the truck as the latter is tied to the rails.

The distance between the supporting wheels is 4 feet, which thus forms the rigid wheel-base of the truck, the trucks turning at curves and switch angles upon the balancing wheels placed centrally between them. Appliances for the transverse movement of the latter upon curves are also provided, which it will not be necessary to detail.

The theory of this truck is very simple, but yet has been found liable to constant misconceptions. To explain its principal features let it be conceived, in the diagram Fig. 51, that A is a platform, assumed to be one foot in length perpendicular to the plane of the paper, loaded with an uniformly distributed weight of 4,000 pounds. Let a a be vertical posts supporting this platform, in the first instance, each of which posts would then sustain 2,000 pounds of load. Let B be a central post, and removing the posts a a substitute the diagonal braces or supports, b b. These diagonals now support the same load as before, but the stresses in the braces b b will be greater than those in the former posts, a a, in proportion to the cosine of the angle that b makes with the vertical. For, the resultant resistance of the brace, b, acting opposite and equal to the stress due to the weight of the load, taking the direction and magnitude of the latter on any scale to be A R, Fig. 52, may be resolved into the two forces, A a and A c at right angles, A c producing an outward strain in the platform A, and A a being the component, in magnitude and direction due to the weight acting directly downward, or equal to 2,000 pounds. Hence it follows that the load in the direction A R is greater than that due to the weight, or A a; or

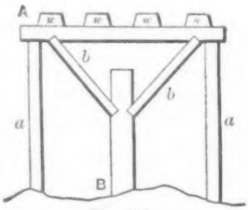


Fig. 51

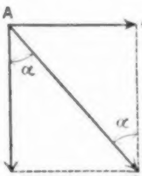


Fig. 52

$$A\,R = \frac{A\,a}{\sin\alpha} \text{ and } R = \frac{2000}{.705047} = 2{,}837 \text{ pounds.}$$

Now, to support the weight upon a post and girder in this way, *B*, Fig. 53, being an end view of the way, a shouldered stick having a bearing against the upper rail at *a* may be used to carry the load, bearing upon a notched stick *b* at the other end, made to fit upon the lower rail at *c*, which would thus support the load. The resultant of the weight *W*, acting downward upon one side of the post, will be to produce a downward pressure along the center line of *b* upon the lower rail *c*, and a horizontal pressure upon the upper at *a*. Now the supporting and balancing wheels of the truck are placed in precisely the positions these braces would occupy to support the load in the best manner upon this way. Fig. 53 represents one-half, or one side of the truck. Of course the same thing would be true of the other side, and the leverage against the overturning of the car by any oscillations, unbalanced loads, or wind-pressure upon its side is represented by the depth of the girder, or distance between the rails *a* and *c* vertically, which is about 4 feet.

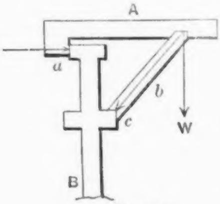


Fig.53

The only difference is that in place of the fixed brace, with the wheels instead of the props a rolling brace is obtained, supporting the load wherever it exists, and removing so much heretofore necessary obstruction from the street by transferring it to the truck instead of allowing it to remain in the permanent way.

From what has been said it will be seen that the angle of the truck wheels will not necessarily be 45°, but dependent upon the proportions given to the permanent way. They are actually laid out as follows: Let *B*, Fig. 54, be an end view of the way, *a* and *c* the rails. From *a* as a center with a radius *a c*, strike an arc *k c l*. Upon the same scale cut off a portion of the arc from *c*, with the chord *c d* equal to the middle diameter of the wheel. This chord will form with the vertical the proper angle to use for the wheel; and bisecting this, *b*, the position of the axle is found, which is maintained, together with the remaining bracing needed, in the construction of the truck frame itself.

Fig.54

For all loads, whether due to wind-pressure or centrifugal action upon the side *a*, or balanced or unbalanced loads acting downward upon the side *c*, press the wheels against the upper rail at the point

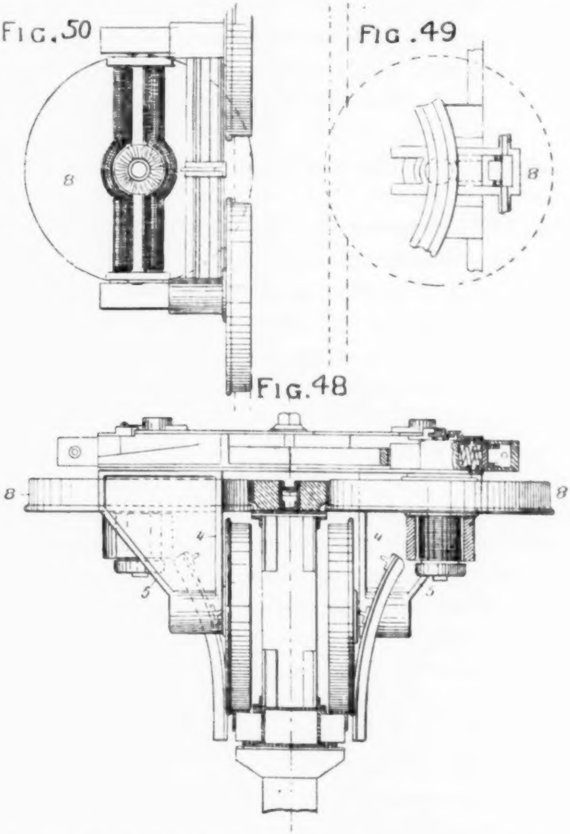
a, about which the truck tends to turn, and the wheel, *b*, will transmit the force at right angles to the line *a b*, or directly through the center line of the wheel *b c*.

With this construction there are no end strains upon the hubs of the wheels, since they are left free to move to a limited extent along the axles, being guided in position wholly by the lower rails. This allows the wheels to separate by sliding out upon the axles, should an obstruction, such as the protruding of a bolt-head, exist upon the track, while there can be no twisting strain brought upon the wheel or axle. For, from the fact that the wheel is guided and its position upon the axle determined entirely by the rail, there being no hub bearings, it follows that the bearing is always square upon the bottoms of the axles, and the effect of the load is thus communicated at all times directly down through the center of the wheel to the rail. From this it results that all loads, balanced or unbalanced, aside from the weight of the wheel itself which of course bears upon the upper flange, are, when the truck is once properly proportioned, equally distributed over the vertical and horizontal surfaces of the lower rails. For, since the force of the load acting vertically downward may be resolved into two forces at right angles, one acting through the center line of the wheel and the other along the axle, the wheel with no end bearings to the hubs would move until the latter component force becomes balanced and neutralized, or, in other words, it would slide upon the axle until the pressure of the wheel upon the vertical and horizontal surfaces of the rail is equalized.

Besides the increase of load due to the inclination of the wheels, there appears to be but one other mechanical objection to the construction. Because of the varying diameters of the wheels at different distances from the center line there will be a slip of the rims in every revolution equal to the distance they will travel in a revolution beyond that of the central element. This will probably not prove more objectionable than the present flange friction existing upon railroads, and may be improved by slightly flaring the grooves upon the sides so as to bring the bearing principally near the central portion of the wheel rim. It may be entirely avoided by the use of ordinary rails placed at the proper angle, and providing the wheels with a flat tread at right angles to the center line, with double flanges upon the edges.

By reason of the independent motion of all the truck wheels, which is rendered practicable only because the design of the truck

prevents the possibility of derailment from any cause short of the destruction of the way, curves are followed so closely that, practically, the increase of friction of the cars upon curves even as small as 50 feet radius, is too slight to be noticed or measured by weighing in a model one-eighth of full size. The construction also



admits of a car 50 feet in length turning by means of these trucks from a street but 28 feet wide into another of the same width.

In Figs. 48–50 is shown an alternative plan of construction for the truck. It is provided with vertical instead of inclined supporting wheels having flanges upon their outer rims, and is designed especially with a view to the use of electricity as a motive power. Fig. 50 shows a dynamo-motor for the truck and car, the electric current in this case being derived from the rails, which are insulated from the girder for the purpose, expansion being provided for by

cables at the slip joints of the posts. The motive power is conveyed to the truck and cars through the horizontal wheels, which act as the driving wheels.

VI. The Passenger Car.

The car designed by Captain Meigs is novel in many of its features. An outside view, end view, and cross-section are illustrated in Figs. 55, 56, and 57. It consists of a strong well-framed platform built of 5 inch channel beams, 7 feet 6 inches in width by 51 feet 2 inches in length over all, trussed upon each side for additional stiffness and attached to the trucks by four interlocking spring posts at either end. The body framing is composed of light **T** iron ribs, bent in circular form, filled in by panels covered with upholstering, which covers the entire interior, and sheathed with paper and copper upon the exterior. The car is perfectly cylindrical above the floor, 10 feet $8\frac{1}{2}$ inches in diameter, inclosing the same cross-sectional area as the standard car in use. The construction is made as light as possible, and strength of form carefully studied. The cylindrical shape is expected to diminish wind resistances and stresses fully one-third as compared with the ordinary car. The seats, or chairs, 52 in number, are arranged as in parlor cars, *i. e.*, independent, re-

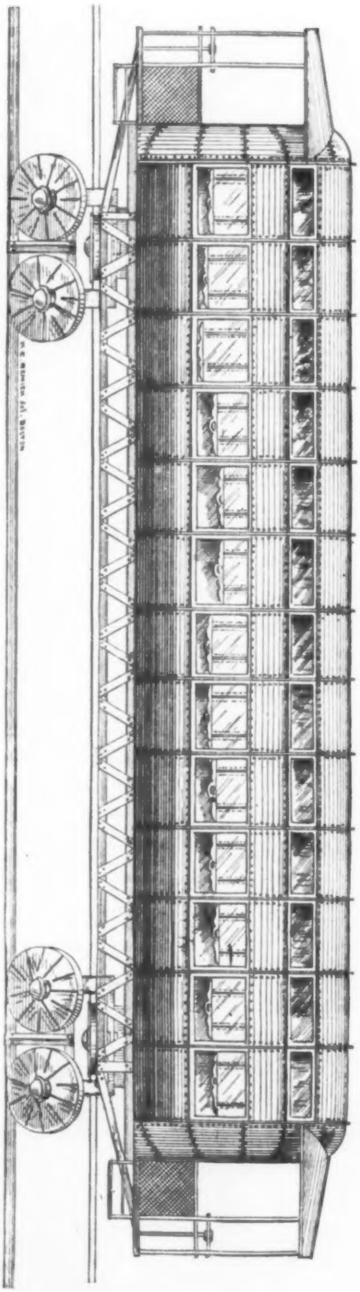


Fig. 55. Scale 1/8" = 10'

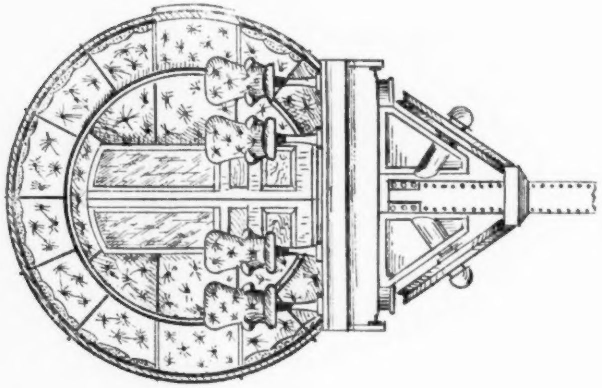


Fig. 57.

Scale 3/16" = 1

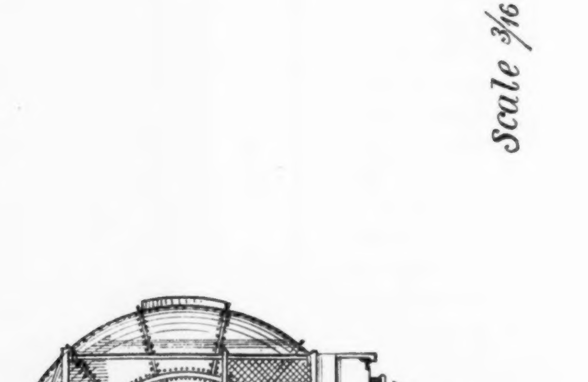


Fig. 56.

volving, and also folding up at will to gain space when unoccupied. They, like the whole interior of the car, except the windows, are upholstered, and comfort and luxury has been studied in every detail. The chairs, as well as the device for securing ventilation at each window without the annoyance of entering dust, are new and special devices of the inventor. If it were ever desirable, one would become more easily reconciled to rolling down an embankment in one of these cars than in that of any other known form, for the entire absence of sharp corners and salient points is noticeable.

VII. The Engine.

The engine or locomotive for this system comprises a platform car supported upon two trucks, one at either end, housed in similar manner in all respects to that of the passenger car. Its general appearance and principal working parts are shown in Figs. 58, 59, and 60, in outside view of engine and tender coupled, a front view of the engine, and plan of engine floor with the locomotive mechanism and boiler upon it, the position of the latter being shown by broken lines. The engine floor is 7 feet 6 inches in width and 29 feet 3 inches in extreme length; that of the tender, carrying tank and bin for the water and coal, being 25 feet 8 inches in length, allowing additional room for baggage or the transportation of employees, or for other purposes. Upon this floor, in the engine, covered with $\frac{1}{4}$-inch iron plates, are supported in effect two complete stationary engines, each connected with and operating a single driving wheel, the pair being horizontal in position and opposite each other on either side of the upper track-beam of the girder and midway between the trucks.

A boiler of the locomotive type, though shorter, being 60 inches in diameter of shell, and 15 feet in length over all, is placed over the engine mechanism, its center line being 61 inches above the floor. It contains 208 tubes, 2 inches in outside diameter and 7 feet long, with a grate 4 feet 6 inches square, containing 20.25 square feet area. For city use anthracite coal will be used for fuel. The grate in this case consists of water tubes having spaces between the sets at intervals of about one foot for solid round two inch wrought-iron bars, which may be withdrawn for the purpose of dumping the fire. The crown sheet is arched or elliptical in shape, stayed to the outer shell by radiating screwed stay rods riveted upon their ends, and is inclined downward 4 inches at the back end, to allow of

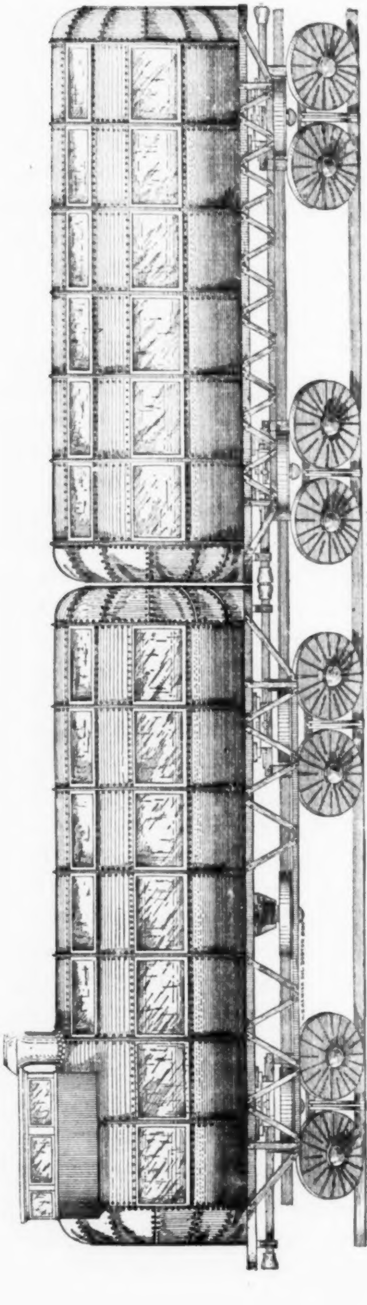


Fig. 58. Scale 1/8"=1'

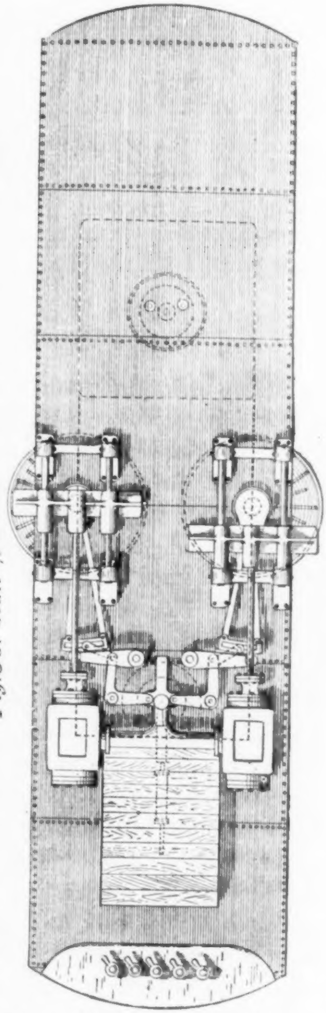


Fig. 60. Scale 3/8"=1'

the climbing and descending of grades equal to 800 feet to the mile, without exposing any portion uncovered by water to the furnace fire.

The cylinders, 12 inches in diameter by 22 inches stroke, are horizontal, their center lines placed 18 inches above the floor

of the engine and 61½ inches apart. The piston rods connect with independent cross-heads sliding upon steel guide rods 2½ inches in diameter and 22 inches centers, these being supported at their ends by cast-iron standards bolted to the floor beams.

The driving wheels, 44.6 inches in diameter, flanged upon their lower edge, in form and position similar to the balance wheels of the trucks, are supported and rotate upon short stout axles of steel, 6 inches in diameter, which extend through a sliding-box containing the journals. These boxes slide in cast-iron ways transversely to the longitudinal line of the engine, the axle having a crank keyed upon its upper end. The crank-pins rotate in square blocks which slide in a rectangular groove in the under side of the cross-heads, the arrangement being in effect the well-known device called the slotted yoke connection.

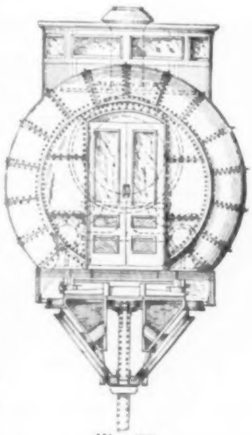


Fig 59.

The slide valves of the usual locomotive form, in steam chests upon the cylinders, are operated by the common link and double eccentrics which are here put upon the driving axles immediately above the floor of the engine, the weight of axles and wheels being supported upon a collar beneath. The only novelty in the valve motion consists in the horizontal instead of vertical position of the links, this requiring somewhat heavier and larger rock-shafts than usual, having vertical axes, the upper horizontal arm of the rock-shaft being connected with the valve rod by means of a short swinging link, and the lower arm carrying the pin within the link block.

For operating the links two bell-cranks shown in the plan are employed, the longitudinal arms of which are connected with the usual link-hanger and strap, though in horizontal position, and the transverse arms with a central sliding piece of wrought iron, which in turn connects with the plunger of a hydraulic cylinder, some 2 inches in diameter by 15 inches long. The throttle valve, link-rod, brake and coupling rods, as well as the connection between the driving boxes for producing pressure and adhesion upon the rails, are all operated by hydraulic power, though hand levers are also retained.

The method of obtaining adhesion of the driving wheels to the rails, by means of a cylinder and piston attached by pins and eyes

respectively to the sliding driving boxes, which is thus secured independently of the actual weight of the engine, enables it to be made considerably lighter than the ordinary locomotive for the development of the same amount of power. It also attains another very important object, that of a variable adhesion.

The extent of pressure is entirely controlled by a simple 3-way hydraulic cock admitting the working fluid, preferably glycerine, under pressure to the pipes connecting with the cylinder from a receiver or reservoir similar to the Westinghouse air-pressure drum. A hydraulic pump maintains automatically a fixed pressure in this reservoir, or by means of an accumulator ingeniously constructed two grades of pressure per square inch may be automatically maintained, one for governing the pressure between the driving wheels, and the other, less in amount, for all the other purposes connected with the operation of the engine. The object of the sliding boxes carrying the driving wheels is to enable the latter to follow the arc produced by the rails upon curves, and its extent of transverse sliding from a straight line connecting the two trucks is about 6 inches.

The engine driver occupies the front portion of the engine, the fireman attending to the furnace at the rear end. The former stands or sits upon an elevated platform, and has an unobstructed view of the way through the windows of the monitor roof. Before him upon a shelf are the five hydraulic cocks controlling, respectively, 1, the throttle ; 2, the reversing apparatus for the links ; 3, the adhesion of the driving wheels ; 4, the brake ; and 5, the coupling rods of the entire train, while just above are steam and hydraulic pressure gauges and indicators, speaking tubes to fireman and conductor, whistle and bell ropes, comprising those adjuncts which secure safety and convenience in practical operation.

With an engine having these provisions for a grip upon the rails, any grades from a horizontal to a vertical can be climbed, it being a mere question of supplying sufficient lifting power by the engine.

While no provision has yet been made for connecting the two engines with their driving wheels together, it being left for practical running to determine the matter, several appliances are in readiness to be added if necessary. This can be accomplished by a mechanical connection in the mechanism, or by the valve motion controlling the steam distribution, or by the independent means of a hydraulic cylinder or auxiliary steam engine to throw either driving wheel over the dead points, when needful.

VIII. The Couplings.

These are automatic in their action, interlocking when coupled, the nose of one draw-bar entering a socket upon the other to form a rigid bar between the two truck centers. A rod extending through them parallel to the draw-bars and making butt joints when two draw-bars are coupled, controls the coupling hooks, which are similar to the Miller hooks, by means of slotted links of variable throw, and these hooks are operated in such a manner by moving the rods hydraulically that the engineman can uncouple any car in the train, from the engine. The object of this is that in case of an impending rear or head collision, which are almost the only serious possibilities of accident existing in this system, the engineman, by one movement of the hydraulic cock controlling the couplings, can divide the train into sections consisting of separate cars, each having a brake which sets automatically upon detachment from the train, and thus, by partially destroying the momentum of the whole, cause the collision to take place by a succession of comparatively small blows from the engine and slowing sections consisting of the separate cars instead of by a single blow having behind it the momentum of the entire train. This is accomplished by a hydraulic cylinder and piston upon the tender, the piston-rod of which cylinder pushes upon the coupling rods, compressing springs upon them during traction, and which when pressure is relieved as by movement of the controlling cock or breaking apart of the cars from any cause, causes the rods to spring out, withdrawing the hooks and uncoupling the cars.

Since the cars can neither lift nor swerve from the track, by this construction of the coupling and of the trucks, end strains can only be brought upon the car platforms directly in line with them, strength to resist which is the leading feature of their construction.

The draw-bars and couplings have special devices for a continuous and positive connection throughout the train, although allowing the draw-bars to swing upon the truck centers, and contain new features of construction.

IX. The Brakes.

The purpose of a brake is to consume the power, momentum or energy of a moving body such as a train of cars, by creating friction upon the wheels or rails, and it is in this system intended to

be operated upon the horizontal or balancing wheels of the trucks, although they may be fitted upon the supporting wheels also. In the former case they may be automatically operated by powerful springs acting upon toggle-joints so as to cause the wheels to pinch the upper track-beam of the girder, or by shoes upon their rims, controlled in either case by hydraulic means similar to those of the couplings. It is considered preferable to employ spring pressure to throw the action of the brakes on, and positive pressure by the hydraulic cylinder, to throw the friction off, during draft of the train.

It has been found in the Westinghouse brake experiments that the most efficient action of the brakes exists at the point of greatest pressure upon them before slipping of the wheel takes place. If the wheels slip, the friction is at once greatly reduced. That is, more power can be consumed in a given time by braking upon the rolling wheels than by the sliding of the same wheels without turning, upon the rails. It is well known also that in the ordinary form of brake the pressure comes upon but one-half of the longitudinal cross-section of the axle or one-fourth of the brass, owing to the use of half boxes, and the position of the brake block with reference to them.

In this method it is resisted by the full half section of axles, which are made large for the purpose, and the momentum of the train is consumed not only by the friction at the brake-shoe, but upon the axles and rims of the wheels pressing upon the rails, *which latter is doubled, with the same braking force*, by the equal friction produced upon the opposite wheel on the other side of the girder.

The action of the brakes as well as that of traction can be best illustrated by the rails rolling between the rolls of a rolling mill. It is this well-known action reversed, and it is easily seen why no slipping of the wheels can occur by any pressure upon them. Indeed, as in the case of the transmission of pressure through the supporting wheels of the truck directly through the center of the wheels, which has been discussed, the greater the pressure or load the more these actions are insured.

It should be said that the brakes just described are additional to the ordinary hand-brakes with which each car is to be supplied for use in an extraordinary emergency.

Continuing the discussion of the principal details of this system which have been described, there remains room for the short examination of a few leading points only.

The weights of engine and cars as actually under construction will be about as follows:

Weight of each truck, complete				6,400 lbs.
" engine carriage, exclusive of boiler and engine				20,336 "
" engine mechanism				11,075 "
" boiler, empty, about	6,500 lbs.	In ordinary working condition		8,500 "
" engine, with boil'r, compl'te.	38,000 "	In ordinary working condition		40,000 "
" tender	" 22,925 "	In ordinary working condition		42,000* "
" car	25,000 "	Loaded		32,000 "

There being the same number of supporting wheels for the weight as in the ordinary system, or eight for each engine or car, there will be:

Load per supporting truck wheel on engine, with ordinary working load.	5,000 lbs.
" " " " tender, " " " "	5 250 "
" " " " car, " " " "	4,000 "

Center of Gravity.—The center of gravity is considerably lower and the stability greater in this form of railway than in either the New York elevated or in the surface roads. It lies within a few inches of the floor of the car, when loaded, or about twenty inches from the way and rails, while in the other systems named it is nearly four feet above the rails. A principal feature in this construction of way is the low point of support for the load, in connection with a high-placed abutment for draft and brake power.

Curves and Centrifugal Forces.—Upon curves, the way is found to be even stronger in form than upon the tangents, since the action of centrifugal forces at these points has been found to act upon the line of tangent posts, while the hoop form of the girder, so long as it is preserved intact, effectually prevents any concentration of stress tending to overturn it, upon any single post. To avoid all locating of posts in the streets, which is always desirable, in turning from one street into another, diagonal trusses may be thrown across from the opposite corner of the street, and the track girders supported upon them, giving all required intermediate support.

Tractive Force and Resistances.—The actual tractive power which it will be necessary to employ in the Meigs system can be determined only by experiment. The resistance to movement of

* With 1,800 gals. water, weighing 15,000 lbs., and 4,000 lbs. of coal.

an ordinary freight car on a straight and level road at 10 miles per hour is about eight pounds per ton weight, and the total may be divided into a constant resistance due partly to the internal resistances of the friction of the axles, and partly to the external resistances of the rolling friction of the wheels upon the rails; and variable resistances. The latter include additional resistances caused by flange friction upon curves and by gravity on ascending grades, the friction of the engine and its machinery, the resistance of the atmosphere and of wind, and those caused by the lateral play of the wheels and transverse oscillations of the engine and train; and all these are affected by the condition of the engine and of the permanent way, the evenness of the track, curves and grades, the weather and wind. They are principally caused, however, by the speed, increasing as the square of the speed. The fact, ascertained by D. K. Clark, that the resistance of a train at 60 miles per hour was 21 pounds per ton, forms the basis of a formula deduced by Mr. Forney. Taking the constant resistance at 6 pounds per ton, since the resistance at any speed to that at a known speed, $R : R^1 = v^2 : v_1^2$, we have the resistance

$$R = \frac{R^1}{v_1^2} \cdot v^2 = \frac{21}{v_1^2} v^2 = 6 + \frac{v^2}{171}.$$

The resistances of the atmosphere also vary as the square of the speed, and according to the estimate of Mr. Zerah Colburn, in *Locomotive Engineering*, increase the above one-half, or 50 per cent. The ordinary resistance of curves is stated to be fully covered by a grade allowance of 2½ feet per mile per degree.*

The total train resistances then, were this railway upon the ordinary system, may be taken at from 16 to 20 lbs. per ton weight of train, at moderate speeds. This would be, for engine, tender and four cars, weighing in all 210 tons, from 3,360 to 4,200 pounds, exclusive of atmospheric resistances which would increase it to about 6,300 pounds.

The tractive power, or force exerted to move the engine with its train one foot is

$$T = \frac{2\,p\,A\,.\,2\,S}{2\,\pi\,R},$$

*H. Haupt, C. E

(p being the mean effective pressure of steam per square inch of piston; A the piston area, and S and R the stroke of crank and radius of driving-wheel, respectively); or, the tractive force which must be exerted in order to equal and overcome the above resistance for a constant speed of train may be found conveniently by multiplying the tractive power *per pound of effective pressure per square inch on the pistons* by the pressure per square inch of the steam upon the pistons. The first is obtained by means of the following formula reduced from that above, or

$$t = \frac{d^2 \times S}{D} = \frac{(12)^2 \times 22 \text{ in.}}{44.6} = 71 \text{ lbs.}$$

(d and D being the diameters in inches of piston and driving-wheel respectively). The mean effective pressure exerted through the stroke, assuming the boiler pressure at 150 lbs. per square inch, diminished to 130 lbs. initial pressure in the cylinders, with the cut-off at $\frac{1}{3}$ the stroke as usual, may be estimated at not far from 100 lbs. per square inch of piston area; which, multiplied by the tractive power per pound, 71, gives 7,100 pounds as the maximum constant force exerted by the engine at the rail, to pull the train.

Now the friction of the driving-wheels upon the rail, or the adhesion produced, which alone makes the exertion of the tractive force useful in moving the train, is in the ordinary system entirely dependent upon the weight or load upon the driving-wheels, and is but about $\frac{1}{5}$ of this load in amount, on an average. Thus it appears that fully to utilize the power exerted in the cylinders of the engine, there would be required in the ordinary system a load upon the driving-wheels, or a weight of engine upon them of 35,500 pounds, in order to produce the adhesion of the wheels necessary to prevent their slipping. It is estimated that the actual loss by slippage of the wheels and absence of means to prevent the bouncing or jumping of the wheels from the rails upon railroads is not less than one-fifth their circumference in every revolution, causing a constant loss of power to an extent of twenty per cent. of the entire power of the engine. The area of actual contact of these wheels upon the rails has been found by measurement to be surprisingly small, an average of many experiments giving an extent of but $1\frac{1}{4}$ inches transversely to the rail by $\frac{1}{4}$ inch longitudinally—an area varying from $\frac{13}{100}$ to $\frac{42}{100}$ of a square inch, and averaging but $\frac{31}{100}$ square inch; from which it follows that the concentration of press-

ure upon these limited surfaces varies from 26,607 lbs. to 85,961 lbs. to the square inch, these enormous forces causing great wear and tear on the rails. It has been stated by R. Price Williams, an authority upon the maintenance of way in England, that anything which will reduce the resistance due to deflection of track under these forces and consequent abrasion, by $\frac{1}{5}$ or $\frac{1}{6}$, will soon effect the saving of the whole cost of the rail.

In the Meigs system the bearings of the wheels upon the rails are increased about $5\frac{1}{2}$ times beyond those in existing railroads, while all bouncing from the track is effectually prevented, or at least its evil effects off-set, so far as adhesion is concerned, by a corresponding pressure upon the opposite wheel, at the moment of occurrence. There can be no slipping of the wheels, when sufficient power is supplied, in a variable adhesion system; they will always roll, under light or heavy loads equally.

Now an ordinary locomotive having 35,500 lbs. load upon the driving-wheels, to make available the 7,100 pounds tractive force we have assumed to be required in this engine, would have to weigh in all at least 60,000 lbs., or 30 tons, whereas this engine exerting the same power will need to weigh but 20 tons, a clear saving in dead weight to be transported of 10 tons, or 33 per cent. of the weight of the ordinary locomotive.

The importance of this saving, resulting from the use of means for producing a variable adhesion independent of the weight of engine and possible only upon the system of way adopted, or a similar one, demands particular notice.

Speed.—By this saving in power, an increase can be made in the size of the driving-wheels, to draw a train of equal size as now run, without a material increase in the expenditure of steam and coal; from which it results that with the same number of revolutions of the driving-wheels as that now customary, a large increase in speed is attainable, with the same power of engine. Thus, for example, a 10-foot driving-wheel, without slip, would require but 168 revolutions per minute to make 60 miles per hour, while the ordinary 5-foot driving-wheel requires to be driven at 336 revolutions per minute, even without slip, to run at 60 miles per hour, which it will be seen is nearly the practicable limit, producing a piston speed of from 1,200 to 1,400 feet per minute. Any large increase of speed beyond this amount can hardly be looked for upon the ordinary railroad, for the requirements of safety against derailment, and the safe working speed of the mechanism, together with

the exertion of sufficient power to move the load, are the limiting conditions which forbid the increase in size of driving-wheels against the rapidly and enormously increasing train and air resistances, as the speed is augmented.

So that, it requiring but 280 revolutions a minute at usual speed of a 10-foot driving-wheel to make 100 miles an hour, as compared with 336 revolutions with the ordinary 5-foot driving-wheel to make 60 miles an hour, it will be seen that an ample margin of steam is obtained, together with the absence of slip, to run at such speeds with equal or ordinary power. Thus it becomes perfectly possible and within the capacity of the present locomotive boiler and without increase of piston speed, to run trains at great speed upon the plan proposed, and the inventor confidently predicts regular working speeds of from 75 to 100 miles per hour upon this railway with equal and probably greater safety than that with which 40 miles an hour are now run. The entire absence of connecting and parallel rods in this engine should be noted as an important point in the attainment of speed with safety from breakage in the engine mechanism.

Power.—Of course the power developed by the engine will vary with the number of revolutions made. Although the term, horse-power, is applicable to stationary rather than to locomotive engines, since as a general distinction the office of the latter is to draw a load rather than to lift a weight through a certain height, its power can be reduced to an equivalent horse-power, this being equal to the product of a certain weight attached by a rope to the circumference of one of the driving-wheels into a certain height through which it is lifted per minute, divided by 33,000. Since the power exerted is equal to the gross work performed, it may be represented, calculated from the effective steam pressure and the speed, as follows:

$$HP = \frac{p \times 2 \times A \times 2 \times S \times N}{33,000} = \frac{4\,p\,A\,S\,N}{33,000},$$

it therefore depending upon the effective pressure, p, per square inch exerted through the whole stroke; the joint area, $2A$, of the two pistons; the length of stroke, S, in feet; and the number of revolutions, N, per minute. At 60 miles per hour with 10-foot driving-wheels, the foot-pounds of energy developed at 168 revolutions would be 13,921,606 per minute, and the nominal horse-power, 422. At 100 miles per hour and 280 revolutions per minute the

number developed would be 23,202,676 foot-pounds per minute and the corresponding number of horse-power would be 703.

As to the train resistances at these speeds it can only be said that no data now obtained are applicable. Their actual extent is simply a subject for conjecture, and can be determined only by actual experiment.

It is evident that the same proportionate advantages may be realized with the adoption of electricity instead of steam as the motive power, for which the system is especially well adapted, and which it is ultimately the intention to employ, with a consequent avoidance of dust and noise.

In conclusion and in review, there may be noted the following brief statement of advantageous points for the accomplishment of rapid transit, as indicated in this system.

Summary of the Chief Distinguishing Points of the Meigs Elevated Railway System for Rapid Transit.

1. Security from derailment by the construction of truck overhanging the girder and tied by the wheel-flanges to the rails.

2. The connection of the draw-bars and couplings to the trucks directly beneath the car platforms, preventing telescoping or rising of the cars in case of collision.

3. The increased security of the attachment of the car body to the truck, having four posts for each truck, in place of the usual single pivot-pin, and the tying of both together by interlocking flanges.

4. Obstruction in the streets and interference to light and view reduced to a minimum, due to the reduction in the width of the permanent way.

5. The advantage in strength of way due to bringing the stresses of the load directly over the central line of posts, by means of the diagonal supporting wheels.

6. The lowering of the center of gravity and consequent increase in stability both of the cars and way, as compared with the New York system.

7. The advantage of independently rotating wheels for the prevention of slipping and flange friction, enabling each wheel to follow the rail more closely than in the ordinary truck, and thus greatly diminishing its resistance upon curves.

8. The increase in breadth of tread of the wheels upon the

rails to prevent the concentration of the load upon small areas, and to thus reduce the wear upon the rails.

9. The saving of power attained in the production of pressure upon the driving-wheels by means independent of the weight of engine, and controlling the same so as to produce the variable adhesion required.

10. The lessening in cost of operation by the reduction in weight of engine and cars.

11. The cylindrical form of car and engine, for the diminution of air and wind resistances upon their sides and resulting strain upon the trucks and way.

12. The facility of turning sharp curves as small as those of 50 feet radius, enabling it to be constructed in narrow streets, not possible in the ordinary system, and the absence of increased friction and resistances upon the curves.

13. The facility and economy in climbing heavy grades without loss of power by slippage, and, in general, by the holding of the wheels against the rail without the possibility of rebounding from them, as in the ordinary system.

14. The superior advantage of the system proposed of brakes upon the upper or gripping rails.

15. The automatic coupling apparatus for diminishing the danger in head or rear collisions.

16. The increased safety of the switch due to its size and construction.

17. The freedom of the way from obstructions, or from snow and ice lodging upon the way and blocking it.

18. The entire absence of grade crossings, of trespassers, or the possibility of other trains crossing at grade.

19. The economy of construction and maintenance of way due to absence of surface grading, of embankments and drainage ditches, and diminished repairs in alignment and cost of the renewal of cross-ties.

20. The increased speed made practicable, with safety and economy, due to its special features as a system, as compared with the ordinary system now in use.

APPENDIX I.

AN ACT TO AUTHORIZE THE INCORPORATION OF THE MEIGS ELEVATED RAILWAY COMPANY.

Be it enacted by the Senate and House of Representatives in General Court assembled, and by the authority of the same, as follows:

SECT. 1. Joe V. Meigs, William S. Butler, William A. Russell, Roland Worthington, Thomas W. Pierce, Henry Hastings, Nathan Appleton, Franklin E. Gregory, Edgar E. Dean, George A. Alden, George E. Harrington, Frank Jones, J. W. Johnson, George J. Carney, Charles E. Powers, their associates and successors, may associate and become a corporation as the Meigs Elevated Railway Company in the manner provided by chapter one hundred and thirteen of the Public Statutes and acts in addition thereto, subject to all the duties, restrictions and liabilities contained therein, so far as the same can be applied thereto, except those parts referring to the "gauge" of the road, the amount of its capital stock, and the manner of paying in the same for the purpose of building, maintaining and operating an elevated railway between some point in the city of Cambridge and Bowdoin Square in the city of Boston. The location of said road across the Charles River shall not be south of the southerly line of West Boston Bridge, and shall thence proceed in the most direct practicable route to Bowdoin Square in Boston, and there terminate: *provided, however*, that the board of aldermen of the city of Boston may, for reasons of public necessity and convenience, to avoid unnecessary damage to property, deflect the route from the most direct line. For the purpose of applying said provisions of said chapter one hundred and thirteen to the corporation hereby authorized, it shall be deemed a street railway corporation.

SECT. 2. The amount of its capital stock shall not be less than one hundred thousand dollars for each mile of road. Not less than ten per cent. of said stock shall be paid in before a certificate of incorporation is issued, and the whole capital stock shall be paid in in cash before the construction of the road shall be commenced.

SECT. 3. Locations for tracks shall be petitioned for between the points named in section one of this act, and after fourteen

days' notice, of which notice a copy shall be left with the owner or occupant of each estate on the line of the proposed location, seven days before the hearing, a hearing shall be had before the board of aldermen of the city in which the location is asked, as provided in section seven of said chapter one hundred and thirteen, and after such hearing the board of aldermen may refuse the location asked for, or grant the same, in whole or in part, under such restrictions as they shall deem the public interests require, and the board of aldermen of either of said cities of Cambridge or Boston may, on like notice and hearing, revoke any location after the expiration of one year from the granting of the same, if, in their judgment, the public interests so require, and in case of such revocation may require the structures of the company to be removed and the location to be restored to its original condition at the expense of the corporation, in the same manner and with like requirements as in the revocation of locations for street railways under sections twenty-three, twenty-four, twenty-five and twenty-six of said chapter one hundred and thirteen.

SECT. 4. No location for tracks shall be petitioned for in the city of Boston until at least one mile of the road has been built and operated, nor until the safety and strength of the structure and the rolling stock and motive power shall have been examined and approved by the board of railroad commissioners or by a competent engineer, to be appointed by them, and to be paid by said corporation a price fixed by said board.

SECT. 5. The Meigs elevated railway shall not be built after the manner of the New York elevated railways, but shall be built according to the plans, methods and inventions of Joe V. Meigs, a copy of which shall be filed with the Secretary of State within sixty days of the passage of this act; and upon granting a location, the board of aldermen shall prescribe the height at which the lowest part of the girder shall be above the ground, and the width of the track, provided that its greatest width shall not exceed twenty-two and one-half inches.

SECT. 6. The provisions of sections thirteen and fourteen of chapter one hundred and thirteen, and sections thirty-eight to forty inclusive, and sections forty-three to forty-five inclusive, of chapter one hundred and twelve of the Public Statutes, and of chapter two hundred and sixty-five of the acts of eighteen hundred and eighty-two,* shall apply to said corporation. The corporation

* Provisions of the General Railroad Law.

may take, in the manner prescribed in sections ninety-one to ninety-three inclusive, of chapter one hundred and twelve of the Public Statutes, as much land as may be necessary for the proper construction and security of the road, or as may be necessary for depot and station purposes.

SECT. 7. The owner of any property taken for such railway, or of any property abutting on streets through which said railway may pass, not so taken, in any manner injuriously affected or lessened in value, whether by smoke, noise, obstruction of light, air or access, disturbance of quiet enjoyment or otherwise, by the construction, maintenance or operation of said railway, may petition for assessment of his damages, and his petition shall be heard and determined in the same manner and with like effect as now provided by law when real estate is taken for public highways. But said corporation shall not acquire title to any land, nor enter upon any street, until all damages to the owners of land and abutters on any part of a street occupied, or to be occupied, by its structure have been paid or secured in a manner satisfactory to the owner, or to be fixed by the superior court or any justice thereof, sitting in equity for the county where the land lies, upon the petition of either party and summary hearing. And the erection of the structures authorized by this act in any street shall be deemed a new servitude, for which damages may be claimed by any owner of land having a fee or an easement appendant or appurtenant to his land, in, on, or over such street, or by any tenant of such owner. But all persons claiming interests in the same estate shall join in one petition. And such petition for damages on any street shall be filed before the expiration of one year after the structures authorized by this act are built or operated in that part of such street contiguous to the petitioner's estate.

SECT. 8. The damages and costs recoverable by the persons petitioning therefor, as herein before provided, shall become and be a first lien without priority to any of said petitioners as among themselves, on all the property of the said corporation, having priority of payment in full, except over debts and taxes due to or assessed by the United States or the Commonwealth, or any county, city or town in the Commonwealth; said lien may be enforced for damages and costs in equity. If any damages recovered against said corporation, other than damages recovered by owners of land and abutters on any part of a street occupied by any structure of said corporation, or their tenants, as such owners, abutters or ten-

ants, remain unpaid for thirty days after final judgment therefor, the superior court may, by injunction or other suitable process in equity, prohibit and restrain the corporation from continuing the operation of said road, or maintaining any structure in any place or manner injurious to the person applying for such relief.

SECT. 9. Whenever said corporation shall make any excavation in or near any public highway, or shall set any foundation, pier or post, in or near the same, the surface of the street, sidewalk or other ground shall be restored, as soon as practicable, to the condition it was in before the excavation was made, as near as may be; and no interference shall be had with, or change made in, water or gas mains or pipes, sewers, drains or other subterranean works, except with the concurrence of the board of aldermen first had and obtained, and upon condition that the same shall be immediately restored to a serviceable condition, as good as before the change or disturbance, and at the sole cost and expense of said corporation. And the superior court in equity may summarily enforce the provisions of this section by injunction or other appropriate remedy.

SECT. 10. The provisions of section three of chapter one hundred and five of the Public Statutes shall apply to the corporation hereby authorized.

SECT. 11. This act shall take effect upon its passage. [*Approved, March* 18, 1884.]

DISCUSSION.

Mr. Durfee.—I would like to ask the author of the paper a question that occurred to me while he was reading it—whether those diagonal wheels are sustaining wheels or steadying wheels. The exterior diameter of the wheel is considerably larger than the diameter of the bottom of its groove, and, it seems to me, there must be a grinding action there.

Mr. Galloupe.—The diagonal wheels are the sustaining or supporting wheels for the weight. We do not suppose that that grinding action will be any worse than the present flange friction upon the ordinary wheel; but it can be entirely prevented by using the ordinary form of rail placed at the proper angle and changing the shape of the wheels to wheels having a flat tread with or without flanges at the sides.

Mr. Durfee.—It seems to me that the grinding action there must be very destructive both to wheel and track. It would bring a very awkward strain on the inner flange of each wheel.

Mr. Kent.—I did not notice any suggestion in the paper regarding the influence of wind strains. We know how the Tay Bridge fell down, because it did not have a wide enough base.

Mr. Galloupe.—All these questions have been carefully considered and gone over. It has been studied for some five or six years both by a Civil Engineer as well as Mechanical Engineers, and strain sheets made with every modification of the track structure, and besides the calculations of theory, tests by model were made so far as practicable. The whole system is now being built of full size, experimentally, with about one-half a mile of track containing

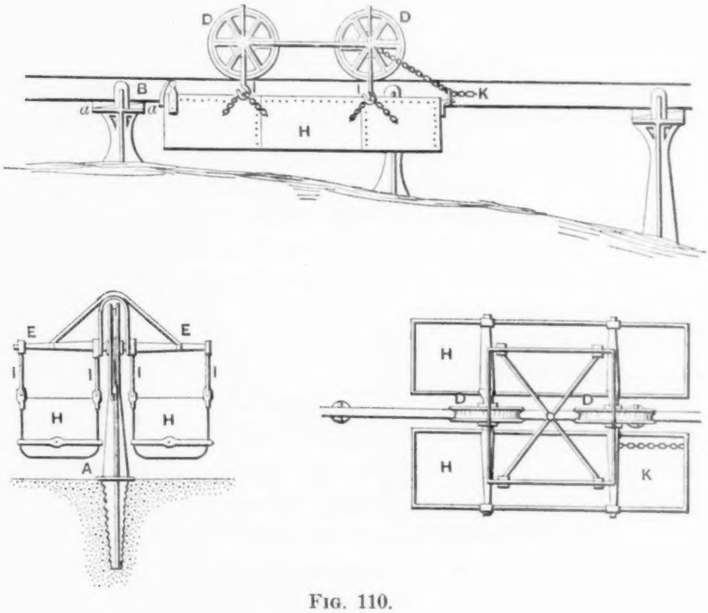


FIG. 110.

the most extreme conditions as to grades and curves and difficulties, which will ever be likely to occur in practice, and it is going to be tested practically to see if the theories will be upheld. All these things have been considered, and the maximum strains that can be brought on the way and on every part of the system have been taken in designing it.

Mr. Halsey.—I am not a railroad man, but I see one point in the design which appears to be very objectionable—the switch shown on page 94. In principle this is substantially the old-fashioned stub switch, the dangers of which are so great, even on sur-

face lines of track, that in the State of New York I believe its use is now prohibited by law. It does not seem possible with a track constructed on this plan to apply any of the modern safety switches. As the result of a misplaced switch of the construction shown, might be to run a train off the end of the track and drop it bodily into the street, the objection seems to me a serious—perhaps a fatal—one.

Mr. Durfee.—The idea of supporting a train of moving cars upon a single rail supported by girders sustained by posts is a very old one indeed. In the year 1824, five years before the opening of the Liverpool and Manchester Railway, Henry R. Palmer, civil engineer, published a "description of a railway on a new principle,"* which consisted of a single track supported on posts. The wheels and trucks were above the roof of the cars, which were suspended from the trucks like panniers, on each side of the track (Fig. 110). The center of gravity in that construction was very much lower than in the one under consideration. One of the

FIG. 111.

* Description of a Railway on a new principle, with observations on those hitherto constructed, and a table showing the comparative amount of resistance on several now in use. Also an illustration of a newly observed fact relating to the friction of axles, and a description of an improved dynamometer for ascertain-

illustrations of Mr. Palmer's book (reproduced in Fig. 111) is of especial interest as showing an early form of the cantilever bridge. The form of single rail, post-supported track described by Mr. Palmer has been several times before the public since the publication of his book. In the year 1834 Henry Sargent, of Boston, built at Chelsea a circular railway (intended for amusement only) on this plan, and commenced the construction of a larger structure in East Boston, on a location described as "a marshy piece

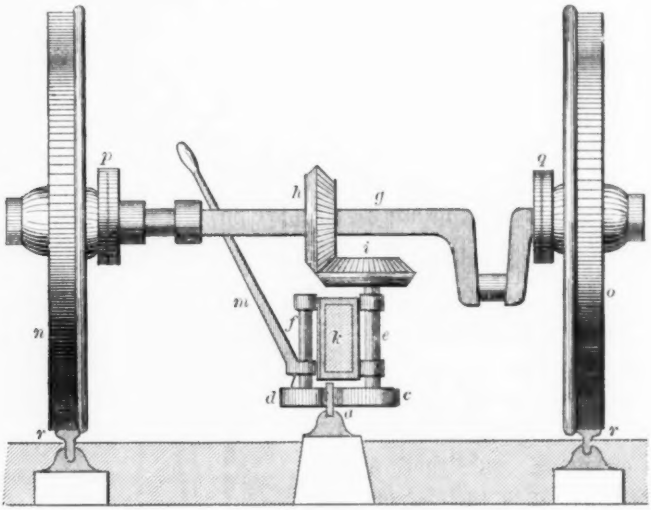


FIG. 112.

of ground, full of creeks and ponds, and much more unfavorable than the average surface of the country." At the Centennial Exposition there was a short section of single rail on posts, on which ran a peculiar locomotive having a passenger car attached. Other examples of similar attempts might be given. I fail to detect any conspicuous novelty in the car body described; substantially the same construction was proposed by a Mr. Robbins and an illustrated description published thereof many years since. The plan proposed to insure tractive power by means of a pair of horizontal gripping wheels was originally devised by Vignoles and Ericsson (Fig. 112), and an English patent granted therefor

ing the resistance of floating vessels and carriages moving on roads and railways. By Henry R. Palmer, Civil Engineer, Member of the Institution of Civil Engineers. With plates. Second edition, revised. London: Printed for J. Taylor, at the Architectural Library, High Holborn, 1824.

in 1830, and it has been patented several times since both in England and the United States. It was used on the Mont Cenis Railway, but was not regarded as a decided success.

Prof. Hutton.—There is one thing to which I look forward with interest, and that is to see how they will manage one of the details of the switch. If it is desirable to have the switch move over a very small angle it would seem as if it were necessary that a very long girder should be used to be swung at its heel in order that the permanent way of the siding may clear the permanent way of the main track. I know that that has been one very great difficulty in railways constructed on this principle heretofore.

Mr. Schuhmann.—During the Centennial Exhibition there was shown a short elevated railroad, and that had the rail in the middle and the wheels on top of the car. The car itself hung down, saddle fashion, on both sides of the rail, as in the form illustrated by Mr. Durfee.

The President.—Was the power contained within the car?

Mr. Schuhmann.—I believe it was; I do not remember exactly. The road was only about 150 feet long. They used to take passengers on it for five cents. It ran across Lansdowne Ravine in Fairmount Park.

ADDED SINCE THE MEETING.

Mr. Galloupe.—In answer to Mr. Durfee's remark that a slipping or grinding action of the truck wheels would bring a strain upon the inner flanges of the wheels, I would say that it was particularly intended to show in the paper, under the discussion of the truck's action, that such a thing could not occur, as demonstrated both by theory and actual trials with models. The stresses must be distributed on both flanges or sides of the truck wheels equally, or they will move upon their axles (since there is nothing to resist motion in contact with the hubs, and the position of the wheels is entirely governed by the rails) until this occurs. This has always been found a difficult point to establish, but will I think become clear upon a little reflection. It is one of those cases where seeing, rather than trusting to theory, is believing.

In regard to Mr. Halsey's comment upon the switch, I would say that it is difficult to judge of a thing involving so many new conditions by the analogies of the surface railroads. We have here no very small rail section or switch points, invisible to the engine driver beyond a few feet away, and hence necessary to have the protection of a safety frog, but in this case a large girder

two feet wide by four feet in depth comprising the switch section, its position being easily seen for a much greater distance, and hence largely increasing instead of diminishing the safety of this feature. Besides that, while the ordinary switch points have to be moved not more than an inch or two out of place to derail a train, this switch may be moved *fifteen* inches out of place with no effect resulting from a train running upon it in this position except to close it, an action following from the form and inclination of the truck wheels striking it.

The present safety switch system in use on surface railroads embodies the features of a continuous bearing for the supporting wheels, and this is by no means impossible in the Meigs system. It would involve the use of a girder of double the usual depth, and divided into two parts horizontally. The supporting wheels would run on the upper boom of the lower member, and this would be continuous except that it would have frogs. The upper member would be the one to be swung to connect the branch tracks and main line. The details of this safety switch have been worked out so far as to demonstrate that it is fully feasible, but in particulars, will depend largely in any instance on the details of girder selected, and on the maximum and minimum headway required or allowed by local authorities beneath the girder.

There is also a device to be soon applied to it that shall hold the switch in one position or the other, as by springs, either open or closed, so that I do not see that the switch is more unsafe than the ordinary one, but quite the contrary. The great desideratum is to prevent derailments, a thing which the ordinary switch is not too perfect in accomplishing.

Prof. Hutton's suggestion that a small switch angle would necessitate a very long girder for the switch is undoubtedly true but this truck will never require an inclination of switch section to main line greater than about 5°, and is moreover perfectly adapted to turning angles, not requiring a small switch angle on account of its independently rotating supporting wheels, and the central position of the upper wheels about which the truck turns. With the above inclination, the length of switch truss will be moderate, not exceeding 35 feet.

As to the Robbins car, the only feature in common with it and the Meigs is that it is round, the method and weight of the framing and construction being dissimilar throughout. They were both designed about the same time, I believe.

I am obliged to Mr. Durfee for alluding to the history of elevated or post line railroads. The earliest proposer of a single post line railway, of which I know, in England is Henry Robinson Palmer, whose patent is dated Nov. 22, 1821. He shows a beam carrying a single rail on top and supported upon a line of posts. Upon this rail runs a vertical supporting wheel for the load, which in this case consists of bags carried one on either side, like panniers. [Described in *Rep. of Arts*, Vol. I., 3d series, p. 129: *Newton London Journal*, Vol. V., p. 151, Vol. X., p. 32: *Mechanics' Magazine*, Vol. XXVII., p. 349: *Register Arts and Sciences*, Vol. I., pp. 97, 131; Vol. II., pp. 150, 353; Vol. III., p. 141; Vol. IV., p. 219; Vol. I., new series, p. 9; Vol. IV., p. 25: *Engineers' and Mechanics' Encyclopædia*, Vol. I., p. 615; Vol. II., p. 425.]

Since then probably over one hundred different patents have been taken out in England and the United States in this line, of which I will briefly refer only to the following.

Among the English patents, April 2, 1825, a patent was taken out by Jacob Jedden Fisher for a suspended railway, in which weights were shown suspended below the level of the rail on either side, the track itself being supported like the floor of a suspension bridge.

D. Maxwell, May 10, 1829, had a patent on suspended cars.

William Newton, an English attorney, took out a patent July 30, 1845, upon a rail of ordinary section, having horizontal wheels running upon its sides, close to the ground. Another was that of Robertson J. Clinton, June 4, 1846, who provided a central rail between the two ordinary rails, and elevated this rail to a higher position.

July 14, 1846, Sir Samuel Brown took out a patent on a central rail and a wheel having a notched or V groove to run upon it.

In the United States, we have Henry Sargeant, May 6, 1825, who patented a post line railway carrying a rail on top and vertical supporting wheels carrying panniers of wood, upon either side. He printed a pamphlet upon it which was published in Boston, April 30, 1827.

J. Stimpson, June 3, 1830, also patented a single post line wooden way, having side strips upon the posts.

In the patent of Bryant and Hyett, June 13, 1831, a vertical wheel is shown on a post line railroad, with the load supported by it hanging down on either side of the posts. A similar patent is J. Richards', patented March 9, 1832.

But the nearest approach to the method adopted in the system under consideration was that by U. Emmons, in the United States, April 17, 1837, who, in addition to the post line, single rail and wheel on top carrying the car pannier-fashion, and which extended down upon each side of the posts, employed side rails upon the posts upon which run horizontal steadying wheels for the lower part of the car.

July 2, 1872, a patent by E. Crew shows inclined steadying wheels.

To conclude this brief review of these crude and disconnected ideas but two more need be named.

The "Cameron" Pontoon Cart, proposed for South Africa, may be regarded as the simplest possible form for a railway. It consisted in fastening to the ground by rough, notched sticks a line of hollow logs cut lengthwise in halves, not unlike a wooden house gutter in appearance. In this groove was a single wheel carrying a basket with arms on either side. This "wheelbarrow principle," as it is called, required the equilibrium to be maintained and propelling power furnished by men or animals. Also log railroads have been used, one by Richardson Brothers, at a mill near Truckee, Nevada.

This conception, elevated on posts about three feet from the ground, was an idea suggested by J. L. Haddam, Engineer-in-Chief of the Ottoman Government, for a military railroad, and a section was built, with engine and rolling stock, the latter being in form like an inverted V, or on the "camel saddle" principle, hanging upon either side.

The single rail railroad, so called, built at the Philadelphia Exhibition in 1876, over Belmont Ravine, was invented by Gen. Le Roy Stone, of New York. It was elevated about 35 feet and was about 500 feet long, really consisting of *three* rails instead of one the section being not unlike the letter A, with a rail at each angle of the triangle. The supporting rail was at the top, the lower rails carrying the horizontal steadying wheels for the saddle-bag, car. It was previously built at Phœnixville, Pa. The engine was a rotary one of the La France pattern and connected direct to the supporting wheels by gears instead of cranks. One fatal objection to it seems to be its inability to turn curves.

Thus, while some of the ideas mentioned above undoubtedly enter as elements in the system under consideration, it cannot be said that any of them resemble it closely. It is a fact that this

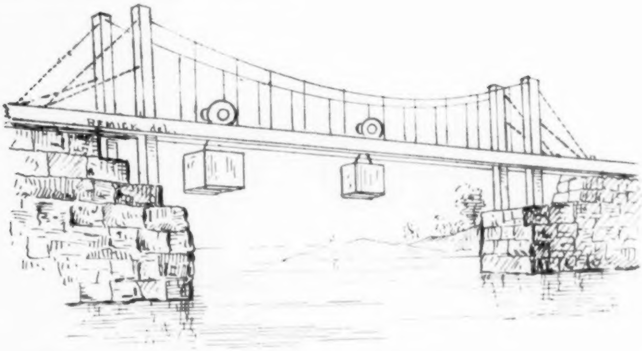


FIG. 140.—FISHER, 1825.

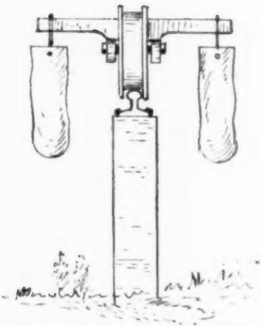


FIG. 139.—PALMER, 1821.

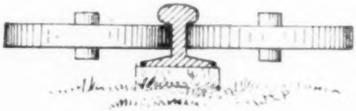


FIG. 141.—NEWTON, 1845.

FIG. 142.—CLINTON, 1846.

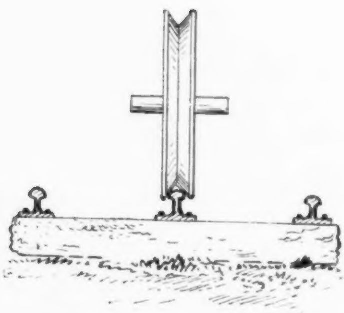


FIG. 143.—BROWN, 1846.

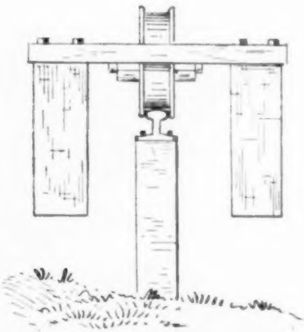


FIG. 144. SARGEANT, 1825.

F. E. GALLOUPE.

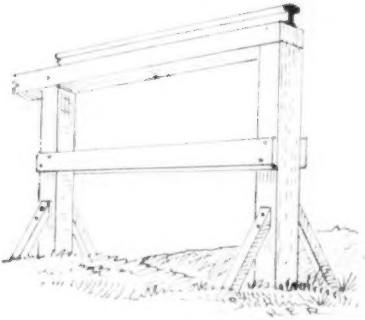


FIG. 145.—STIMPSON, 1830.

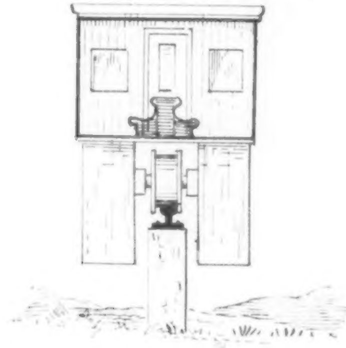


FIG. 146.—BRYANT AND HYETT 831

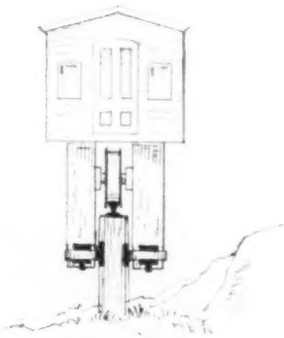


FIG. 147.—EMMONS, 1837

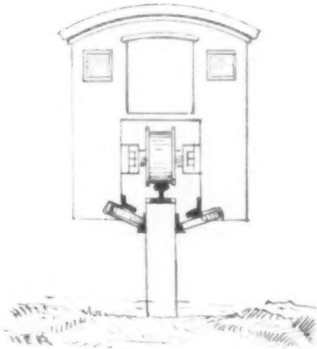


FIG. 148.—CREW, 1872.

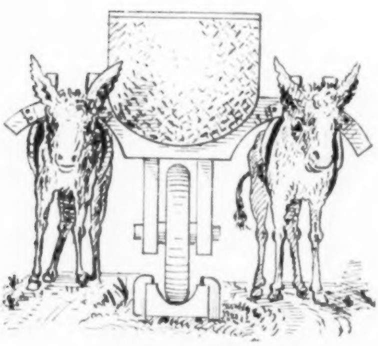


FIG. 149.—CAMERON, 1878.

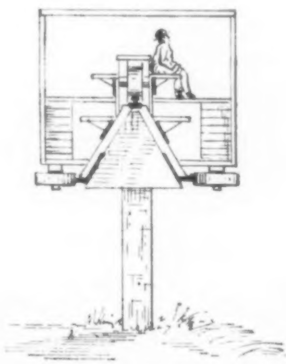


FIG. 150.—STONE, 1876.

APPENDIX III.

RAPID TRANSIT AND ELEVATED RAILROADS.

(*Page* 102.)

PROOF OF ACTION OF FORCES ON THE MEIGS WHEELS.—In Fig. 139, let *a G* be the vertical force due to gravity acting downward upon the wheels and guides. We may separate this central force into two vertical and parallel forces *b w*, *b w*, whose point of application is at the central point of junction of the axles and wheels. Now, the force *b w* may be resolved into the two forces of which it is the resultant, *b s* and *s w*, at right angles. *S w*, at right angles to the axle *a s*, tends only to transmit a pressure through the center line of the wheel, or normal to the rail at *c*.

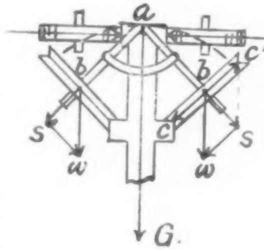


FIG. 139.

If we consider the axles produced until they meet at *a* as a rigid frame, being so constructed in the truck frame as to be immovable, but two motions are possible. 1st. A rotary motion of the point *a* in an arc passing through *a*, in which case the arms *a s* would slide through the wheels, which motion is prevented with the least supporting force by the position of the two balancing wheels pressing horizontally at the point *a*, or exactly opposite to the tangential force which would cause rotation. 2d. Since the angle cannot rotate, the effect of the load producing the force *b s* tends to crowd the axles through the wheels and to increase or spread the angle *s a s*, the direction of the force being *b c'*. Since this is balanced by a rigid resistance presented by the truck frame itself, its reaction is equal and opposite to the stress caused by the force *a s* and produces a pressure, *b c*, also normal to the axle and rail at *c*, or directly through the center of the wheel.

Therefore, the wheel runs as free in the inclined position as though it were running over a horizontal surface, and would do the same if it had a flat tread parallel to the axle, and without flanges or hub bearings, when it would lack only a guide.

To supply this, either hub bearings must be provided, which are inadmissible, to prevent the wheel from moving out of position, up or down upon the axle, or light flanges to keep it upon the rail. The wheel itself rolls as free as any wheel rolls over a plane surface. Neither flange in a flat tread-wheel would bear any portion of the load (except the small load due to the weight of the wheel itself, which of course acts vertically downward upon the upper flange), but only serves to guide the wheel in position.

Since the direction of all loads on the car is the same as the force of gravity acting on the trucks' weight, the action will be the same for all, light or heavy loads, equally. F. E. G.

plan existed previous to the designing of the New York system, and was offered at the time the latter had been determined upon. Generally speaking, the differences from any previous ideas proposed are that it is a truck system like the ordinary railway system, and as such adapted to turning curves with facility. Then, it is *not* a single rail system, but has four rails instead of one, two or three, and the supporting wheels for the load are the lowest instead of the highest rails where more than one is employed. It also has four supporting wheels for each truck, or the same number as the ordinary railroad, which was not the case in the previous plans.

These early ideas were not practical: they had no truck system, and nobody would put in money to develop the practical details. As in the case of the telephone and other great improvements, it is the man who can develop the crude ideas, put them in practical form in all their details, and who creates a new and useful appliance by a new application of principles, though they be very old, who is entitled to the larger share of credit. In considering a railroad, unlike a machine which may be complete in itself, we must have a *system* which is complete. Previous attempts have failed because the system necessary could not be carried out without meeting objections fatal to success. Besides, so far as appears, but few of those mentioned ever advanced beyond the conception stage to anything like a practical working.

CXC.

TWIST DRILLS.

BY WILLIAM H. THORNE, PHILADELPHIA, PA.

The following paper is offered as an inquiry into the requirements of the cutting edges of twist drills, and as a suggestion of a mode of procedure by which a proper form for them may be determined.

Each lip of a drill is required to cut a shaving from a metal surface and should be in the shape of a wedge. The angle included between the faces of the wedge, will be called the cutting angle, and that between the surface being cut and the adjacent face of the wedge, the angle of clearance. In order to secure the greatest efficiency in cutting different metals, these angles should be varied, but, on account of the inconvenience of having to keep a different set of drills for each metal, a compromise has been evolved which is reasonably efficient for them all. It is known that the best cutting angle for wrought iron is about 60 degrees and for cast iron about 70 degrees, the angle of clearance being from 3 to 5 degrees; hence, if we take 65 degrees as the cutting angle of a drill, it will answer for either metal.

The body of a twist drill is a cylinder, grooved on two opposite sides. The intersections of these grooves with the end of the drill form two lines, the straight portions of which should be parallel with each other and at a distance apart equal to about one-eighth of the diameter of the drill and leading from the circumference a little past the axis, then curving backward in a direction opposite to that of the rotation of the drill and again touching the circumference at a distance of rather more than a quadrant from the starting point, as shown by the accompanying drawing (Fig. 61). If these grooves were parallel with the axis, it is evident that the cutting angles of the drill would be 90 degrees, less the clearance, or say 85 degrees. In order to reduce these angles to 65 degrees, the amount required, the grooves are cut spirally, making the angle of the spiral about 20 degrees, or

its pitch about nine times the diameter of the drill. This makes the cutting angle correct only at the part of the lip farthest from the axis, while it will increase in magnitude from this point in to the point nearest to the axis; but, as the speed diminishes at the same time, this increase of the cutting angle is not objectionable. For brass, the cutting angle should be from 80 to 90 degrees; therefore it is best either to use a flat drill or to alter the cutting angles of the twist drill by grinding off the lips by planes parallel to the axis and to each other. Another object in making the grooves spiral, is the lifting or screwing out of the chips from the hole which is drilled.

The distance between the parallel sides of the grooves at the axis gives the necessary strength to the center or web, while the strength and stiffness of the drill is further increased by the metal adjoining the curved parts of the grooves. The cylindrical portions of the drill, which are left between the grooves, should be backed-off from their forward edges, to an uniform eccentricity, in order to diminish the rubbing surface and to prevent undue friction and sticking.

The angle of clearance of the lips is obtained by the shape to which the end surfaces of the drill are ground, and the straight part of the intersections of these end surfaces with the grooves forms the cutting edges. The most efficient shape for these end surfaces can best be determined by considering that the surface of the metal being cut is in the form of a hollow cone, truncated a certain amount depending on the thickness of the web of the drill. The cutting edges of the drill will touch this hollow cone in straight lines. Now, imagine a portion of a conical surface to coincide with this hollow cone and then to be rotated about one of these straight lines through an angle of, perhaps, 5 degrees. It is evident that this would produce an uniform and similar clearance throughout and would be perfect. It is the object, therefore, to approximate this in shaping the end of the drill. As the two cutting edges are straight lines, parallel to each other but at a distance apart equal to the thickness of the web, and as each is produced by the intersection of a conical surface with one of the grooves of the drill, these conical surfaces must be selected so that their intersection with each other will produce a proper form of chisel point, and so that they will approximate as nearly as possible to the surface of the hollow cone.

The accompanying illustration (Fig. 61) represents on a scale of one-quarter size a 2-inch and ½ inch drill as they should be ground

by a well-designed machine, the complete conical surfaces and intersections being shown by fine lines. In these drills, the apices of the conical surfaces are moved away from the apex of the hollow cone a sufficient distance to produce, by the intersection of the surfaces, a proper form of chisel point; the axes are inclined so that the radius of curvature of the surface at the outer end of the cutting edge corresponds with that of the hollow cone; and these axes do not lie in the same plane but in parallel planes, at a distance apart greater than the thickness of the web of the drill, so that a tangent to the conical surface will make an angle of 5 degrees with a tangent to the hollow cone at the outer end of the cutting edge.

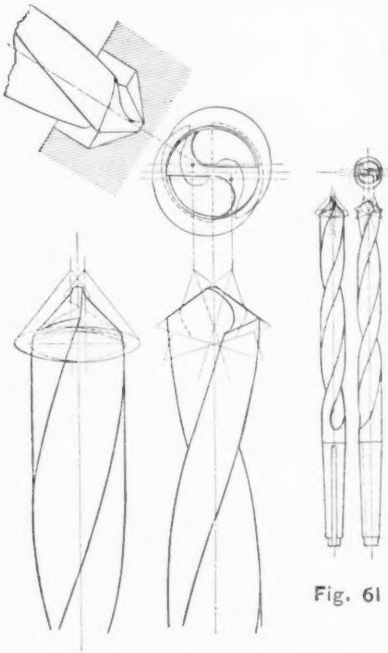


Fig. 61

The shanks of twist drills are turned tapering, but unfortunately there has been no sensible standard adopted either for their diameters or the amount of taper. Each machine-tool builder has made the sockets in the spindles of drilling machines, lathes, etc., as circumstances or the fancy of the moment happened to suggest. The Morse Twist Drill Co. established a standard, which has been adopted by most drill manufacturers and some tool builders, but, by some unaccountable perversity, the diameters are uneven and irregular, and each different size has a different taper. They also adopted, as a method of driving the drills, the use of a flat tongue, milled on the end of the shank and fitting in a slot across the bottom of the socket, which method has unfortunately been almost universally followed. This tongue frequently twists, its corners get rounded, and it injures the slot in the socket, soon rendering the latter useless. A far better device, to which the writer has been long accustomed, is a key fitted permanently into the socket and extending the entire depth of the latter. This key fits a groove in the shank of the drill and supplies a perfect means of driving the

latter, with a minimum of wear and strain. The end of the shank, for a short distance, is turned smaller, and is hardened to prevent any upsetting by the use of the drift or wedge in removing the drill. The amount of taper, proper for the shanks, is a disputed question. The Morse taper averages less than $\frac{5}{8}$ inch in diameter per foot of length, or an included angle of about 2° 54′, but with drills driven by means of a key, $\frac{3}{4}$ inch per foot is better, as it enables the drills to be more readily removed from the socket, and at the same time prevents them from falling out by their own weight.

The following table gives the proportions of the Morse taper drill shanks. The diameter is measured at the mouth of the socket. The length is measured from this point, parallel with the axis, to the end of the shank. The taper is the amount of increase in the diameter per foot of length.

MORSE STANDARD DRILL-SHANKS.

DIAMETER.	LENGTH.	TAPER.	THICKNESS OF TONGUE.
.475″	2.4 ″	.604″	$\frac{13}{64}$″
.699″	2.86″	.6 ″	$\frac{1}{4}$″
.936″	3.51″	.601″	$\frac{5}{16}$″
1.231″	4.5 ″	.615″	$\frac{15}{32}$″
1.746″	5.76″	.625″	$\frac{5}{8}$″

The writer suggests the following as a table of standard proportions for the shanks of twist drills.

PROPOSED STANDARD DRILL-SHANKS.

DIAMETER.	LENGTH.	TAPER.	THICKNESS OF KEY.
$\frac{1}{2}$″	$2\frac{1}{4}$″	$\frac{3}{4}$″	$\frac{3}{32}$″
$\frac{11}{16}$″	$2\frac{3}{4}$″	$\frac{3}{4}$″	$\frac{1}{8}$″
$\frac{15}{16}$″	$3\frac{1}{2}$″	$\frac{3}{4}$″	$\frac{3}{16}$″
$1\frac{1}{4}$″	$4\frac{3}{8}$″	$\frac{3}{4}$″	$\frac{1}{4}$″
$1\frac{5}{8}$″	$5\frac{5}{8}$″	$\frac{3}{4}$″	$\frac{5}{16}$″
2 ″	$6\frac{3}{4}$″	$\frac{3}{4}$″	$\frac{3}{8}$″

DISCUSSION.

Mr. George R. Stetson.—In the matter of the proper grinding of twist drills, the manufacturers are interested, as the one thing standing in the way of their more general use is the want of care in their grinding.

The common practice, among metal-workers, of grinding a tool when it is dull, without regard to the proper form, works badly for twist drills.

The larger proportion of drills returned cracked up the center and complained of as defective, are broken by not having the clearance back of the cutting-edge properly removed.

There are several machines now in use that secure automatically the proper angle and clearance.

Their prices range from \$100 to \$200, the makers not being able to furnish them for less. I would suggest that any one making a machine for \$50 to \$70 to grind drills from $\frac{1}{4}''$ to $1\frac{1}{2}''$; or, better still, from $\frac{1}{8}''$ to $2''$, would be a public benefactor.

In hand-grinding, it takes but little practice to give the circular clearance. Rest the drill on the left hand and move the shank by the right hand.

The clearance can be measured by resting the drill point on a plain surface, and holding a common rule parallel with its sides and noting.

In a drill not much worn, the angle of 59 or 60 degrees, which forms the cutting lip, in combination with the angle of 27 degrees, which is the angle of the spiral at the point, will produce a straight cutting lip or edge. Any deviation in the angle of the cone or cutting lips produces a hooked or irregularly-formed cutting edge. These conditions change somewhat as the drill is worn toward the base, as the angle of the spiral changes.

To show that it pays to study to get the best results with drills, I give some figures furnished me by a superintendent of a works where drilling is an important feature. He writes: "I have drilled with one $\frac{5}{16}''$ drill 4,383 holes, each $2''$ deep, in annealed cast iron. I drilled 1,200 pieces in one day, equaling 200 feet deep. The drill is running 1,000 revolutions per minute and feeding $6''$ per minute. One thousand revolutions of a $\frac{5}{16}''$ drill gives about 82 feet per minute of speed at the periphery of the drill." I watched the working of a $\frac{3}{8}''$ inch drill in the same class of work. In this case the work revolved, the drill having only its forward or feed motion. The work made 528 revolutions per minute. Feed was 88 to the inch, and the work was drilled $4\frac{3}{4}''$ deep in 51 seconds, feed being $6''$ per minute, and velocity of periphery of drill nearly 52 feet per minute. In both the above cases the cast iron was thoroughly annealed. The table of speeds of twist drills published by the Morse Company, gives about 20

feet per minute for iron. In a good quality of cast iron this could be 30 feet. On steel as usually annealed, 20 feet per minute is fast. Better results are obtained at 16 to 18 feet per minute. The conditions of stock vary so much that any table of speeds must be used with judgment.

Regarding the standard taper for the shanks of drills, the idea of Mr. Morse was to establish a standard taper of $\frac{5}{8}''$ to the foot. He commenced in a very limited way to make drills in Bridgewater in 1862, and did not secure the aid of capital until the summer of '64, when the Morse Twist Drill and Machine Company was formed and his works moved to New Bedford. The first set of hardened and ground standards were made in 1872, and the original idea of $\frac{5}{8}''$ to the foot taper had been departed from or was not accurately established at first. The stock of drills in hand would have made it difficult to have made any change, and the error has been allowed to continue. The greatest deviation is in the No. 2 shank, and amounts to .025″ in a foot, or .0059 in the length of the shank. It would be much easier to correct this error than to establish a new standard. The objection to a sharper angle or taper is that the drill has a tendency to draw out of the socket when coming through its work. This twists the tongue and produces most of the trouble complained of, and would be best obviated by a taper less than $\frac{5}{8}''$ per foot. The Standard Tool Company, of Newark, established a standard of $\frac{9}{16}''$ taper per foot.

The desire to avoid the waste of stock required to make the taper shank to drills will ultimately lead to the more general use of straight-shanked drills and the chuck that will hold them strong and true, to get all the work out of the larger sizes of drills without their slipping, I would recommend as a secondary problem to the man who should succeed in making the ideal grinder for 50 dollars.

Mr. Hawkins.—Referring to the grinding of twist drills, I presume that we may make up our minds that for many years to come there will be a large proportion of it done by hand, even though a comparatively cheap and good grinding machine could be put on the market. I think that the suggestion made by Mr. Stetson that the heel of the drill should be ground away is not a good one, for this reason : I think Mr. Stetson would agree with me that if we could grind a drill by hand so as to have whatever is allowed to remain of the clearance angle a correct one, and

grind the heel from there away as much as we choose, we would have a perfectly acting drill. But the difficulty in grinding a drill of that character is that the clearance angle is so short a line that to arrive at a decision by the eye is very difficult; while with a twist drill, if the whole heel is allowed to remain, it forms a considerable guide to determine this angle of clearance, in the same way that we can determine the inclination of a long horizontal line pretty closely, while with a very short one we cannot. I have seen myself, men who ground twist drills on a grindstone of very small diameter and get them hollow from cutting edge to heel, so that the heel would be a cutting edge also to some extent, while the clearance angle at the cutting edge would be too great. I think, therefore, that in such a case the least observation would show the error; while, if the heel was ground away, the operative would have little or no guide: so that cutting away the heel of a twist drill would be an unfortunate thing to advocate in the shop among the men. It forms a guide, and a very good one, to determine the clearance angle in grinding such drills by hand.

Mr. Oberlin Smith.—I thoroughly agree with Mr. Stetson in advocating a straight shank drill. The taper shanks are a nuisance to hold on to and a great deal more expensive to make. The metal is so reduced and weakened, that it is not very easy to drive large drills. I think a good way to drive a straight drill and to assist the friction of the chuck jaws is to cut away half the upper end of the drill for a short distance, and let the projecting half run up into a like place cut away in the chuck. I have used a good many ordinary Morse straight-shank drills in that way. None of the drill-chucks in the market will hold a $\frac{3}{8}''$ drill to do heavy service. I never found a friction chuck which would properly hold a drill of any size. I introduce a little plug into my chucks at the rear of the jaws, cut half away, and then I cut away one half of all the drills (for a length of $\frac{1}{8}''$ to $\frac{3}{8}''$), no matter what size they are, from $\frac{1}{16}$ up. When the drill is put in, the half-cylinder of the drill goes up and locks with the half-cylinder of the plug, which is fastened in the drill chuck. There is play enough, so that when the chuck is tightened, it will bring the drill truly in line. That little half-round tail that sticks up is stronger than the ordinary flatted tail on a taper-shank drill. It is very easy and simple to make and it answers the purpose nicely.

Mr. Bond.—I might say in regard to the Standard Taper for

drill shanks, if any change is to be made, to make it, as Mr. Stetson says, $\frac{9}{16}$ to the foot. We use the latter taper in our own work, and there seems to be a tendency toward the use of a little less angle than is given in the Morse tapers. Some of our machines are fitted with the Morse taper rather than our own, but only because these machines are to be used with the Morse drills; but in our own practice we incline to a $\frac{9}{16}$ taper.

ADDED SINCE THE MEETING.

Mr. Thorne.—In a certain make of twist drills, the pitch of the grooves increases from the point to the shank, but what good purpose this serves is not clear. If the cutting angle, due to the pitch, is correct when the drill is new and long, it should be the same when the drill is nearly used up. A short drill, on account of its stiffness, is well adapted for starting holes accurately and for use in the lathe, and its cutting edges and angles should be as perfect as when it was new. Hence it would seem that the angle of the spiral should be the same for the entire length. If the object of the increasing pitch is to make room for the chips, its slight effect in this regard may be appreciated by considering that the groove of an inch drill makes only one turn in nine inches.

As regards the shank, the writer is familiar with the use of tapers of $\frac{1}{2}$ inch, $\frac{5}{8}$ inch and $\frac{3}{4}$ inch to the foot, and has found that the pressure required to feed the drill into the work, is sufficient to force the shank, having $\frac{3}{4}$ inch taper, into the chuck firmly enough to prevent its being drawn out when the cutting edges pass through the work, but not enough to prevent the drill being readily removed from the chuck when desired, and believe that the spindles of all Lathes and Drilling and Boring Machines should be bored with this taper, and suggests the table of diameters given in the paper for the sake of interchangeability.

CXCI.

THE FRICTIONAL RESISTANCE OF SHAFTING IN ENGINEERING ESTABLISHMENTS.

BY SAMUEL WEBBER, LAWRENCE, MASS.

A PAPER on the above subject, recently presented to the Society of Mechanical Engineers,* seems to give an impression with regard to the amount of power actually consumed in overcoming said resistance, which differs widely from the results of the experiments of the writer of the present paper.

The reason for this discrepancy is to be found in the assumption made in the previous paper that indicator cards, taken with all the machine belts running on the loose pulleys of the machines, are a correct representation of the power absorbed by the shafting.

This is to be denied *in toto*, as the loose pulleys are only a part of the machine placed on it for the convenience of the operator, to avoid the delay and annoyance, and possible danger, of throwing the belt off and on the driving pulley on the shaft, every time that the machine is to be stopped and started again, and is in no sense a part of the shafting. When the machine is in operation the loose pulley is not in use, but the power is taken from the shaft to the machine by the machine belt, which latter is merely an accessory to the machine itself, which cannot be operated without it, while the shafting can be.

The writer knows that this method of taking indicator cards to ascertain the power consumed by the shafting, with the belts running on the loose pulleys, has been the usual and common one, but it is none the less erroneous, as it only arises from the unwillingness of the mill owners or operatives to take the time and trouble necessary to throw the machine belts off for a few moments entirely while the indicator cards are being taken.

This amount of power consumed by the machine belts running on the loose pulleys, will average in a cotton mill fully 10 per

* Trans. Am. Soc. Mech. Eng., Vol. VI., p. 461.

cent., varying from 5 or 6 per cent. in the spinning-rooms to 18 or 20 per cent. in the weaving-room. This 10 per cent. is in this manner charged to the shafting, making an average as given in the paper referred to of 25.9 per cent. in a large number of mills for shafting and engine, which should not be over 16 per cent. in a properly shafted mill, and which is even much less than that in mills of modern construction, if the machine belts are thrown off before taking the indicator cards, a method of getting at the matter which has been accomplished by taking a Saturday afternoon for the purpose.

This 16 per cent. should be divided as follows: Engine, 6 per cent.; shafting and belting, 10 per cent., including in the latter all counter belts, and everything except the small belts actually driving the machines, to which their power, as has been said, should be charged, as they can neither be operated nor their power weighed without them.

A number of years since the writer had occasion carefully to weigh and determine the power consumed in a large cotton mill, which had just been entirely rebuilt and fitted with new shafting by one of the most judicious engineers in New England, the late E. A. Straw, of Manchester, N. H., and although the shafting was not of quite so small a diameter or run at so high a speed as has since been often adopted, it was very well arranged, and would serve as at least a fair example of good average dimensions, and not much heavier than I should advise to-day, taking into account the necessary stiffness to resist *transverse* strain, when belts were liable to be led from it at any convenient point to reach the different machines, rather than limiting it to the diameter sufficient to bear the *torsional* strain only.

The summary of the total power required by the machinery was 744.22 H. P., and in making up the account of the whole, 10 per cent. was allowed for the shafting, but subsequently the latter was calculated, as a whole, from weighings which I had made of a large part of it, assuming that which I had not weighed to require the same power in proportion to its diameter and velocity. These calculations gave a total of about 62 H. P., or only 8.3 per cent., instead of 10 per cent., and have been fully confirmed by many subsequent experiments.

The spinning-room in this mill contained 198 throstle frames of 128 spindles each, requiring at least 1.5 H. P. each, or 297 H. P. in all, and 12 filling winders and 13 spoolers, requiring also 21.75

H. P., or a total of 318.75 H. P. These machines were placed in ten parallel rows, lengthwise of the mill, and were driven by two lines of main shafting, each driving a set of machines direct, and two other sets to either side by counter shafts, each of which drove two machines. This made twenty-four short counters driven from each shaft.

The main shafts were each as follows: One length of 10 ft. 4 in., $4\frac{1}{8}$ in. diam., receiving the main belt, then divided equally to the right and left in lengths of 16 feet each; 80 ft. of 2 in. diam., 32 ft. of $2\frac{5}{8}$ in.; 48 ft. of $2\frac{3}{8}$ in., and 32 ft. of $2\frac{1}{8}$ in.; in all 202 ft. 4 in. each. The counter shafts were each 8 ft. 6 in. long and $2\frac{1}{8}$ in. diam., and the velocity of the whole was 216 revolutions per minute.

The required power to carry this shafting by dynamometer measurement was, for each main line, 1.587 H. P., and the coefficient of friction was only .0334. For each set of four counters, with their counter belts, the power was .357 H. P., and the coefficient of friction was .0413. The total power, therefore, for each system was,

main line	1.587 H.P.
six sets counters, four each @ .35	2.142
or	3.729 H.P.
which multiplied by two, gives	7.458 H.P.

as the total for the room, or only 2.34 per cent. of the power required for the machinery.

Now this is the extreme light point, as the spinning-room requires the least shafting, and uses the most power in the machinery of any room in a cotton mill. In a weaving-room for print cloths, the power for the shafting is about 20 per cent. of that required for the looms, or about the same as that absorbed by the machine belts running on the loose pulleys.

Having positively settled this fact in this mill, weighings were afterward made in other mills of a sufficient portion of the shafting to enable me very closely to compute the total, which I have only once or twice found to exceed 10 per cent., which basis I have therefore taken as a safe one to use in computations of the power required to operate a cotton mill.

As an illustration of the closeness of an estimate made on this basis, I was called upon some years since to decide upon the size of turbine required to replace an old-fashioned breast-wheel in a mill where every inch of water-power was of value.

Dynamometer weighings gave me as the power required for the machinery.................................... 214.24 H.P.
adding 10 per cent. for the shafting, or .. 21.42
gave a total of................ 235.66 H.P.
as the horse-power required.

One of the sizes of the Swain turbine, which was the most economical wheel in the use of water which I then knew, was guaranteed to give 240 horse-power under the available head of eleven feet, and as this wheel had been very thoroughly tested by both Mr. Mills and Mr. Francis, I advised the mill owners to put in this size of wheel, though apparently a very close fit for the required power, for, as above said, every inch of water was of consequence. The wheel was put in, and to the great delight of the owners, when the water was let on, and the machinery put in full operation, there was still a part of the last tooth in the gate rack of eight teeth left unhoisted.

A similar operation, in another mill, a couple of years later, with the same turbine, gave equally satisfactory results.

At both of the last two mills spoken of, the shafting was old, and in excess of the amount which would be used to-day for the same machinery. Soon after making the first one of them, I was called upon to weigh the power used by one of the new mills at Fall River, which I did, and was told that my result did not agree with Mr. Bacon's cards from the indicator, of which I knew nothing until my test was completed. I said that I did not include the engine, for which at least 5 per cent. should be added.

Mr. Bacon's cards gave a total power, 470.57 H. P., my weighings of the machinery only gave 408.94 H. P., to which I had added 10 per cent. for shafting, making a total of 449.83 H. P. This I then increased to 15 per cent. for "engine *and* shafting," making an addition of 20.45 H. P. more, and giving a total, 469.91 H. P., or a variation of less than one horse-power in the two results, with the estimate of 15 per cent. for the engine and shafting.

Indicator cards taken by me at one of the later mills in Fall River, when the machine belts were all thrown off from the driving pulleys on a Saturday afternoon, when it could be conveniently done, gave me only between 12 and 13 per cent. of the total for engine and shafting, and I am fully convinced, by these and other experiments, that 15 per cent. for "engine *and* shafting," or 10 per cent. for "shafting *only*," is an ample allowance to be made for a cotton mill in good running order, as they are now constructed.

While I thus dissent from the writer of the paper referred to, in regard to the *data* upon which it is based, I agree with him fully in the conclusions he draws in regard to undersized shafting and over-tight belts. Far more friction in the bearings will be caused by the springing of a flexible shaft, than would be due to the necessary excess of diameter to make it sufficiently rigid to resist flexure from the strain of the belts, nor is the substitution of steel for iron any material improvement in this respect.

From a series of elaborate experiments, made by Mr. Jas. B. Francis, C. E., of Lowell, for the Merrimac Manufacturing Co., in 1866, and published by him in the *Journal of the Franklin Institute*, for April, 1867, he deduces the fact that while a 2-inch iron shaft, "subject to no transverse strain other than its own weight," would admit of a distance between bearings of 15.46 feet, a steel one would only admit of 15.89 feet, although the diameter necessary to resist torsion need be only 0.855 for steel to 1. for iron.

In my early recollections of mill shafting, over forty years since, cast-iron shafts, of a cruciform section, on which wooden drums or cylinders were built up, reaching from beam to beam, were still in use, although wrought-iron shafts and cast pulleys were being substituted. The first formula I remember, for the diameter of wrought-iron shafts, was given by Buchanan in his "Mill-work and Machinery," and was, $D = \sqrt[3]{\frac{100 \times \text{H. P.}}{R}}$. This formula Mr. Francis still retained, after the experiments referred to, as a good one for jack shafts, or first movers, and for the first length of lines, receiving the pull of the main belts, computing the factor of safety, or power of resistance above the breaking strain to be 15.58. For transmitting lines, he reduced this co-efficient of 100 to 50, and for light counter-shafts supported close to the bearings to 33, and since the introduction of "cold-rolled shafting," I have found the latter co-efficient to answer perfectly for transmitting lines, although I prefer to keep close to the original formula for first movers, to resist the transverse strain without flexure, and when the bearings are from 8 to 10 feet apart, as is the usual condition in cotton and woolen mills, do not advise the use of any shafting much less than 2″ diameter, unless for the very last length of a line or for such light power as is required for knitting or sewing machines. Even in cases where the beams are 10 feet apart, it is well to use an intermediate hanger near the pulley, if any amount of power is to be taken off. I have seen a $2\frac{1}{4}$ inch shaft, at 250 revolutions

per minute, where about 4 H. P. was taken from it midway between beams 10 feet apart, so "*buckled*" by the strain that I could not bear my hand on it near the pulley, and in other cases have found the co-efficient of friction doubled in the same manner, when testing with the dynamometer.

While the above observations apply more particularly to cotton and woolen mills, still the same principle will hold good in all cases, and in the case of machine shops, where the percentage of shafting to the power consumed by the machine tools is much greater, the last counter-shafts with their loose pulleys are always sold with, and form a part of the machine itself, and the power for these should be charged to the machine and not to the shafting.

DISCUSSION.

The President.—The paper of Mr. Webber is a very interesting one, and one to which a great deal of study has been given. I think, however, there will be different opinions among engineers, in regard to the propriety of testing a main line of shafting, divested of all its connection with counter-shafts, etc., but in the class of machinery to which he alludes, I suppose it is not common to have very many counter-shafts. The point with some engineers would be, whether or not in arriving at the total amount of power used up in friction, the power used to drive the counter-shafting, and in fact all that is intermediate between the engine and the manufacture of the fabric itself, may not be chargeable as frictional resistance. That is, if you want to divide the power in a mill, between the power to drive its machinery, and the power to manufacture the fabric, you will necessarily be obliged to stop just where the manufacture of the fabric begins, in order to be able to say that the mill friction of shafts, etc., requires so many H. P., and the manufacture of the goods or articles which are made, a certain other number of H. P.

Mr. Henthorn.—I should like to say a few words in answer to the paper which has just been presented, in which exceptions are taken to a part of a paper prepared by me and read at the last meeting of the Society. In this paper, entitled "On the Frictional Resistances of Engine and Shafting in Mills," will be found, among other data, Table No. 1, giving the percentage of full load required for engine and shafting in some fifty-five different mills.*

* Transactions A. S. M. E., Vol. VI., p. 464.

Exceptions are not taken it seems to the facts, contained in said Table 1, column 16, collectively or relatively, but only upon the ground wherein the limit of the term "shafting" ends and the resistance of machinery, considered as a separate element, commences. I consider that all of the resistances outside of the producing machine, while such is not in operation, are elements to be considered and maintained, and whatever amount of resistance is due to the belts, running upon loose pulleys should be, and is included in my table, and should constitute a part of the term "shafting."

If we are justified in omitting the belts and their loose pulleys on the score that they are a part of the machine, we may have equally good ground to eliminate all counters between main lines, that are employed for driving one specific machine; and such counters as serve two or more machines would then come under the head of general shafting, and chargeable to friction account.

If these loose pulleys and single counters were frictionless, so that none of the fuel burnt could be made chargeable to them, it would be immaterial whether they were or were not included under the term "shafting;" but, on the other hand, if they are elements which have to be maintained, and coal provided for such as well as for general shafting and engine, then we certainly should include them in our estimates for frictional resistances.

If a manufacturer were to run a little overtime in his spinning or weaving department the probabilities are that the remaining belts would be run upon the loose pulleys of the idle machinery and run together with the main line of shafting and counters; and it is equally improbable that whatever belts and counters intervening as a necessity between the main line and those idle machines would be thrown off in the ordinary course of business. In view of this state of things, which is recognized by manufacturers as the ordinary course to be pursued, we should then recognize that power or its equivalent "coal" for maintaining these conditions.

While I am as anxious as any one to see this waste power reduced to a minimum, I must say that it is not in justice to parties interested to leave out elements which necessarily have to be maintained during the above stated condition of running.

If I were to ascertain the power required for driving engine-shafting, and friction of loose pulleys and corresponding belts it would be necessary to determine definitely without assumption, the amount distributed between these three elements going to make up the total.

But as long as we cannot practically determine these elements

separately for a sufficient number of cases as to admit of a comparison of one mill with another, I consider that if we have the sum total as contained in my paper sufficient evidence is collected for a basis of comparison by manufacturers.

It would be exceedingly interesting and desirable to have such a table covering a sufficient number of mills to be of value for reference and recognizing the different elements which go to make up the frictional resistances in mills, including, 1st, Engine, 2d, Shafting and Counters, and 3d (to meet the criticism of the paper just presented), Loose Pulleys and their Belts.

As this is impracticable in the general run of mills, although possible for one or two special cases, I have therefore made no attempt to give anything but the percentage of full load required for total resistances of friction; nor attempted to deduce from the total result (representing the friction of engine, shafting, and loose pulleys covering some fifty-five cases), an assumed constant factor which would be equally applicable for each mill, for its engine, or for loose pulleys, and their corresponding belts.

As these are elements which I believe change materially with each case and condition, it would hardly justify the arbitrary factor fixed upon in to-day's paper. I cannot agree that 6 per cent. is an absolute quantity for all mills, for engine, and 10 per cent. for shafting and counters, making a total of 16 per cent. for engine shafting and large belts, but not small belts actually driving machines.

Assuming for a moment that this estimate is admissible, and that 6 per cent. is required for engine and 10 per cent. for shafting, excluding loose pulleys and their belts; if we will refer to the table in my paper, it may be seen that we have two mills requiring less than 1 per cent. for loose pulleys and their belts; four mills, less than 2 per cent.; six less than 3 per cent.; seven, less than 4 per cent.; nine, less than 5 per cent.; twelve, less than 6 per cent.; thirteen, less than 7 per cent.; twenty-seven, less than 9 per cent.; and thirty-one less than 10 per cent.

If we were to assume a constant resistance of one element in among the several in our calculation of some fifty-five different cases, the results would lose their reliability, one opinion not confirming another in this assumption.

And if we recognize in a table all of the facts as they practically exist going to make up the total resistance due to friction while

the machinery is stopped and not producing, then I think we have accomplished the purpose of the paper criticised and each individual is left at liberty to assume what his particular engine may take (be it 5 or 7 per cent.), or that belts on loose pulleys take 10 per cent. or more as his judgment may dictate.

Dynamometrical readings for a total result of a given mill are as a class based upon an assumed like condition of running, for that class of machinery tested. And the same may be said for shafting tested under conditions different from those under which they work ordinarily. When we come to the engine, all classes of machines are generally treated alike, and a percentage of friction assumed alike for all; which is not the case. After a summation of the whole, I think that it is reasonable to grant, that although an indicator test recognizing the total frictional resistances as they occur in the mill is not absolute to a $\frac{2}{100}$ or $\frac{2}{10}$, or even a horse-power on a total of 200, yet it is equally as worthy of consideration by manufacturers when considering this question relatively, if they are based upon the same conditions.

Mr. Oberlin Smith.—I would like to call the attention of the Society to a great evil which exists in almost all our shafting plant, more especially in our machine shops, while in our cotton mills the evil is perhaps not so great. I refer to too slow speeds for shafting. In large *mills* the diameters of the pulleys and the regular speeds of shafts, counter-shafts, and all that, has been worked out to something like a system; but I know that in machine-shops the shafting runs perhaps not more than half as fast as it should. In many cases the tools that we buy of the machine-tool makers have their counter-shaft pulleys of such sizes that the main shafting has to run as slow as 100 or 150. I do not think that a line of shafting in any machine-shop should run less than 200; 300 would be better. One of the great difficulties is the straining of belts. Of course, if we run the shafting slow and put on wide belts, they give more strain than narrow belts, and besides this they are kept too tight. I suppose that a critical examination of a number of machine-shops would show that their speed of belts averages very much slower than it should. I know that if a lathe counter-shaft was made with very narrow light pulleys, two feet or more in diameter, and adapted to fast line-shafting speed and quick-running belts, there would be a good deal of power saved. Another advantage of such an arrangement is the ease with which a belt of that kind can be

shifted. We could then, with an inch or an inch-and-a-half belt running at two or three times the speed that they usually run (say on 20 or 24 inch pulleys), shift it so quickly that the practical result would be better than we get with friction clutches. I do earnestly advocate, as a needed reform, that line shafting in machine-shops and such like establishments should be run a great deal faster; that the machine-tool men should make their counter-shaft pulleys lighter and of larger diameter; and that belts should be run narrower and quicker. I now run line shafting at 200 and use ordinary machine tools, but the driving pulleys have to be very small. If shafting must have great transverse strains upon it, the ideal form would be hollow, but that brings in another evil, which my friend Professor Thurston just mentioned to me in conversation, of the large journals and consequent higher friction speed. That can be remedied, however, by reducing the journals; so that perhaps, all things considered, ideal shafting should be very large in diameter, hollow, very thin, and light, but with the journals reduced. Whether it is best upon the whole to make it so, considering the difficulties of construction, I do not know.

Mr. Babcock.—Perhaps some one here knows of an experiment which was tried some years ago in Fall River, with hollow shafting, running it at so high a speed that the belts were run directly from the shafting, pulleys being dispensed with. If so, perhaps he can tell us what was the result, and if it is still running. Professor Thurston tells me that the same thing is done in Colt's Armory.

While I am on my feet I wish to add a word in regard to what Mr. Smith has said about the tightness of belts. I suffered defeat from that cause at one time in a competitive test of steam engines. Our engine was tested first, and being loaded very heavily, the main belt slipped, whereupon the mechanics tightened it to the utmost possible extent. Afterward finding that all the power could not be used, the load on both engines was reduced, so that when the second engine came to be tested the belt was of normal tightness. The difference in the friction caused thereby threw the result in favor of the other engine, when it ought to have been on my side, of course.

Mr. Schuhmann.—I would like to ask Mr. Smith what difference it makes if he runs a shaft 200 revolutions with a 5-inch pulley or at 100 revolutions with a 10-inch pulley—whether that will de-

crease the strain of the belt any? I should think that the smaller pulley requires a tighter belt, and as it runs at double the speed, the frictional resistance will be greatly increased instead of decreased. If he wants to decrease the friction due to belt strain, let him increase the belt speed and make the diameter of the counter-shaft pulley to suit.

Mr. Oberlin Smith.—The objection to that is, that although the pulleys are larger, you put your weight where you do not want it, and bring in that kind of strain caused by the weight of pulleys. Of course an inch belt pulls only half as much as a 2-inch belt. I prefer to get belt speed by running the shafting at a high rate. Of course there may be a middle ground. With regard to the continuous drums that Mr. Babcock speaks of, I noticed those in Colt's Armory at Hartford several years ago, and since, when I have been there, and I believe it is a good system if it is properly carried out. One bad thing with them is that their shafting is a great deal too heavy. It must weigh a great deal more than a whole row of pulleys. But if such a system is carried out by the use of steel tubes mounted on flanges at their ends, I believe it to be the most perfect thing imaginable for shafting, *provided* the makers of the counter-shafts and of the machines to be driven would agree upon some common standard. Suppose, for instance, all machine-shops could have a standard of 200 revolutions and a standard pulley diameter of 10 inches or 12 inches, though I think 8 or 9 would be better, with 300 revolutions. If we could have 9-inch steel tubes 8 or 10 feet long, or whatever was found the best length, running at proper speed, and then if all machine-tool makers could agree on a standard (no matter what speeds the lathes, planers or drill-presses run) for the fast and loose pulleys on their counter-shafts so that they should be adapted to a fixed speed on a continuous line of drums forming the main shafting, there would be infinitely less trouble in every way, and a man when he bought a machine-tool would know that it would agree with his shafting, without having to think up a lot of pulleys.

Mr. Babcock.—If I recollect rightly, in that case the shafting was not very large, four or five inches, but ran at a very high speed, 600 or more.

Mr. Towne.—I can partly respond to the inquiry, not as to Fall River however. The Wheeler and Wilson Company, at Bridgeport, have their large shop shafted in that manner. The shafting is of cast iron, 12 inches in diameter, I think, and all belts are

taken directly from it without the addition of pulleys. In fact the shaft is a 12-inch pulley. I think this system is only applicable where the class of machinery employed in each room is pretty uniform. In the case of the Wheeler and Wilson Company the whole of one room is filled with milling machines, and another with profiling machines, etc., and it is comparatively easy to adapt the counter-shafts to a uniform size of line shaft. It would not be as convenient in a shop having a miscellaneous assortment of tools. I think it should be put on record that the percentage of friction for shafting deduced by the author of the paper we are discussing while applicable to Cotton Mills and some other similar establishments, is not applicable to all kinds of shops. If any young engineer were to adopt that proportion as the allowance to be made for the absorption of power in the shafting of the average machine-shop, for example, he would get into trouble. In the case of the Yale and Towne Works, I have taken some pains to ascertain what the consumption of power is with the machinery running and not running, and the difference between those two conditions, in that particular case, is nearly 2 to 1. The explanation of this large figure, however, is partly that there are a number of machines permanently connected with the shafting and never thrown off, several of them being fans of considerable size. In addition is the fact that the works are large in extent and the power carried in many cases through mule pulleys and other contrivances for turning corners and going up and down stairs and so on, all which absorb much more power than straight lines of shafting. Again, in a case of this kind the machines to be operated are scattered over buildings separated by wide distances, and there are long lines of shafting merely for transmitting power; whereas in a cotton mill the power is taken from the shafting in a more condensed form than in almost any other class of shops.

All of these facts should be considered in using any figures of this kind. The instance I have referred to is corroborated by the experience of William Sellers & Co., whose shops we all know are models of their kind, in which I am informed the difference in power between machines on and machines off is from 40 to 50 per cent.

Mr. Samuel Webber.—I am a very poor hand at discussion, but I want it distinctly understood I have drawn my line on shafting at the loose pulleys. When the machine is running, the loose

pulley is not under strain, and therefore should not be deducted from the power of the machine to get the power for the shafting. There is the kernel of the whole thing. You are not running the belt on a loose pulley while you are running your machinery. In a cotton mill you are running your mill $9\frac{3}{4}$ hours out of ten. You are not running your belt on your loose pulleys. If a good cotton spinner stops a day he does not run those belts on a loose pulley; he leaves the belts hanging. I have known mills where they have had fires by trying to do it for half a day. In Lowell I know a mill where they had to stop a spinning room for an hour or two occasionally, and where they had so much trouble from fire from pulleys getting dry that they had to bush all their loose pulleys with metalline, rather than to keep a man constantly oiling them. At the Atlantic Mills in Lawrence, the Agent has told me that 14 per cent. covered his engine and shafting. As to hollow shafting I saw that tried with hollow cast iron, a number of years ago. They did put it in, they ran it until they broke so much shafting that they took it out and put up heavy wrought-iron shafting to fill the place of the old hollow casting. They tried it a few years and decided to re-shaft the mill.

With regard to the matter of speed. I believe in as high speed and as light shafting as is properly adapted for the purpose for which it is used; but I do not believe in a cotton mill that you want the same speed on all your shafting. You are running your looms 150 picks a minute. Now it is all nonsense to drive that shaft so fast that you can belt it right off the shaft itself without any pulley. It ruins your belts to bring them around so quick. In your weaving room you may run your shaft 200 revolutions overhead and drive it with a 9 or 10 inch pulley on the shaft. When you get to the spinning room you want a high speed. The old-fashioned way to do that was to have a 6 or 7 inch pulley on the spinning frame. Now we put that shafting 350 or 400 revolutions a minute. Then a 32-inch pulley overhead drives a 12-inch pulley on the spinning frame. When you come to the carding room you have got to meet both conditions. You are running, say, 125 revolutions per minute on your cards. You have got a 16-inch pulley on them. At the other end of the room you have got your roving frames. You want to run them 500 or 600 revolutions per minute. Split the difference in your carding rooms. Put your shaft in at 250 revolutions a minute, belt from an 8-inch pulley on your shaft to a 16″ on your card, and from a 32 inch to

a 16-inch on your roving frames. Therefore I say that the speed of the shaft should be proportioned to the work it has got to do. With regard to the matter of loose pulleys, I do not consider that they belong to the shafting. They are used because the mill owners do not want to risk the life of the operative by throwing the belts off and on from the shafting, and they are merely a corollary to the use of the machine.

It should be remembered finally that the previous paper referred to herein, consisted almost entirely of the results of indicator cards taken in cotton mills.

CXCII.

THE UNEXPECTED WHICH OFTEN HAPPENS.

BY JOHN E. SWEET, SYRACUSE, N. Y.

If we had no experience or knowledge, or no knowledge of the experience of others, everything which happens would be unexpected. It is not so much the unexplainable as the unexpected which attracts our attention, excites our astonishment, or disturbs our mental equilibrium.

The man who devotes his life to experimenting with practical mechanics, is sure to meet with the unexpected, or else to be too wise for his generation. Some of us do not care to admit that we were ever caught with the unexpected, but I beg to expose a few of the many things that have come upon me unexpectedly, in the belief that they may be of use to others, and in the hope that others will explain their experience, that we may profit in return.

Things perfectly familiar to mechanics engaged in one branch of industry, are often matters of great wonder to workers in another branch. Men may work a lifetime in cast iron as applied to tools and machinery, and yet know nothing of what it will do in the heating stove of a blast furnace. To such a man the discovery that cast-iron heating pipes grow from six inches to a foot in length by use would be unexpected. To tell the blast furnace man that certain core bars used for casting pipes changed their length by three inches in casting twenty or thirty pieces, would be no surprise, until you supplemented the statement with the fact, strange to him, that they grew shorter, rather than longer.

Another example of a strange fact, well known to plumbers, and not to many others, is what is called an "air-trap." What that is can best be explained by an example. A cistern in the roof of a house has a pipe leading from near the bottom, down to the cellar, along the cellar bottom and up to a wash basin of the ordinary sort. When this cistern has once been filled and then emptied and again filled, air is trapped in the pipe. When the basin cock is opened the water will not force the air out and be discharged at the outlet as most mechanics would suppose. You ask the plumber about it, and he will say, "Why, don't you know what that is?

That is an 'air trap.'" You say "Oh!" and venture to suggest that you should think the water would force the air out. The plumber says, "It is an 'air trap,' and how can it?" You say "by gravity;" then he says, "Oh!" and you finally come to the conclusion that possibly he knows just as much about what an "air trap" is as you know about gravity, and no more. The thing is explainable of course, but is likely to come upon most men unexpectedly.

The unexpected sometimes comes from causes perfectly self-evident, after the thing had happened (as was the case in my experience by the clogging of a nail machine by the scale from the nail plate), and at other times from causes utterly unexplainable, or from causes which are difficult to fathom.

In practice, we use with a fair degree of success, for a piston-rod packing, simply an easy fitting Babbitt bushing. When these bushes become worn so as to leak, we close them up by compressing them in the water cylinder of a sort of hydraulic press. In this operation a mandrel somewhat smaller than the piston rod is put inside, and with all the pressure we can bring to bear, we have never been able to compress the bush so as to grasp the mandrel tight, and yet in two or three cases, perhaps a half dozen, we have had these bushes (one of them after running a year) shut down while the engine was running, so as to grasp the piston rod as if gripped in a vise, in fact so as to break the bushes asunder, or so that we had to resort to destructive measures to get them off.

In the formation of embossed work, male and female dies are used, and the female dies are often made by driving the hardened male die into a block of soft steel. This operation is easily performed by a few blows of the drop hammer. It drives in and raises the soft metal without distorting the block in any other respect. Whereas, if the same operation is attempted by means of the hydraulic press, the block may be upset one-fourth its depth, the sides bulging out or the piece crushed without producing any impression of the male die further than a slight line marking of it.

The unexpected comes upon us both by things not working when we think they ought, and by their working when common reasoning would indicate that they ought not. The man who first invented or constructed a lawn mower, must have been thought an idiot (or at least a man not familiar with the common laws of mechanics) to have imagined that he could with two light wheels get

traction enough to rotate a cylinder six times their own weight, at six times their own velocity, and cut the grass in addition. The worm that drives the bed of a Seller's planer, does not wear out half as fast as it ought, and I fancy there is something unexpected about it, even to the makers themselves.

An engine with a 12 x 18 inch cylinder had been running a year at 185 revolutions per minute, standing usually quiet on a cut-stone foundation. One day, without any apparent cause, it began to shake endwise, and before night had shaken itself loose and had a movement of three-sixteenths of an inch at every turn. The engine being self-contained, no harm came to it, except the loosening of the foundation, and as the work was of more consequence than the foundation it was allowed to go on with a view to repairing it at vacation time, a month ahead. Before vacation time came, the shaking stopped without any more apparent cause for its stopping than that which caused it to shake, and the engine continues to run perfectly quiet notwithstanding the shattered foundation.

One of our well-known electricians built and tested for three years a certain piece of apparatus which promised to be of extensive application. It worked perfectly, and was as good at the end as at the beginning. A large amount of capital was put into buildings and plant, for the production of these pieces of apparatus for the market, and a good many were built, but in no possible way that all hands could devise were they able to reproduce the original, either in effect or durability. I make this statement at second hand, but believe it to be true; first, because of the source from which it came, and second, because it seems the only explanation reconcilable with the action and business character of the parties interested.

The unexpected often happens to the scientist as well as the practical man, as must have been the case with Crooks, when he invented the radiometer. The story goes that he first invented the thing and then made it. But it turned out as tradition says the ship did, when some genius blew into the sails with a bellows; it went the other way. We laugh at the stupidity of the man with the bellows; the next generation may laugh at Crooks.

I venture the guess that there is many a man of science who (knowing nothing of the rolling of railroad rails), if asked to dictate as to which way the rail should be bent, to have it come straight when cold, would find that the unexpected would be

likely to have happened twice when he saw his plan put in practice.

The case is reported to me where the unexpected happened to two boilers, and they did not blow up either. Two boilers alike in size and shape were connected by necks of considerable area, both at the top and bottom, and the connections, both of steam and water, were without check valves. A fire was built under each of the boilers, which were both half full of water, and when steam was raised to working pressure the generators began to play shuttlecock with the water. It first went all into one boiler, and then all into the other. When the play got to its height, the boss, considering the premises and the lives of the men of more consequence than the cause of science, ordered the fires drawn, and the cause or consequences were never settled.

An engineer put in charge of our electric light station found them using oil of 26 gravity, for lubricating the engine and dynamos. Even when the oil was used freely the bearings would warm up and sometimes get hot. It was the practice to increase the quantity of oil as the journals got warmer and turn on water when oil would do no longer. To the engineer's surprise, one night he discovered that one of the bearings kept cool, and he noticed also that the oil cup was feeding only about one quarter as fast as had been the practice. The happy idea of "letting well enough alone" occurred to him, and he found it continued to run cool, and experimenting he proved that by feeding little enough oil he got the other bearings to run cool also.

In the case of an engine which was more than twice too large for the work it had to do, and which could not be reduced to one-half the speed conveniently, it was decided to bush the cylinder to about half its area, with a view, of course, to the saving of coal. The result was that it took a little more coal than before. I think that this result was one which would have been unexpected to most men who did not know of the experiment having been tried before.

For casting a chilled die to be used under a drop hammer, old chilled car wheels were used, and in which fourteen per cent. of spiegeleisen was melted under the expectation that a good chill would be produced, as had been our experience. The first surprise was to find that the die showed no evidence of chill whatever, and could be filed easily. Some pieces of work were required at once, and the die was put in with the expectation that it would only serve for a few, but the second surprise came when its

endurance proved to exceed the best of the chilled dies as two to one.

A large percentage of the unexpected comes in the development of original inventions. When in the experimental stages it is easy to brand the inventors fools or lunatics, and when predicted failures succeed, it is easy to forget that we ever expected anything else. It is not always the ignorant who are wrong, or the best informed who are never in error. If ten years ago the possibility of conversing with people fifty miles away had been publicly suggested, it would have been only accepted by the ignorant, who, remembering the marvels that have been accomplished, would in their blind faith admit of its possibility, while the best informed would have been staggered at the suggestion. Less than ten years have rolled away, and it is an every-day occurrence.

It is not always the uneducated, the insane or the stupid who produce failures, nor the best educated, most thoughtful, or most experienced who bring out everything according to the original intention. The unexpected comes to the good and bad alike, and so in our teachings to the young and our planning for ourselves, is it not well to have our statements and our speculations pretty well saturated with the elements of uncertainty?

It is an old and common custom to use the statement that "two and two are four" as an example of the certainty of certainties, and another, that "like causes produce like effects;" while as a simple matter of statement, the first can be easily shown to be twenty-five per cent. off, and the latter to hold along all the way from like results to results diametrically opposite.

DISCUSSION.

Mr. Towne.—I will mention a case of the unexpected in my own recent experience. The question was how to harden some steel castings, which we are all coming to use more and more. They were small and very light, weighing but a fraction of a pound, and the part which it was desired to harden was very thin, tapering to less than one-sixteenth of an inch. The castings were made by the open hearth process. It was stated that no difficulty would be experienced in hardening them, and that similar castings had been hardened in considerable quantities. We undertook to harden them in the ordinary manner with prussiate of potash, but when putting the file on them afterward, I found that the file took hold

beautifully. Other attempts were made, but with no better result; and finally I went down into the smith-shop myself and tried or had tried various methods, and found ultimately that apparently the only way in which we could produce a hardening on these castings, which would not allow the file to touch them easily, was by using cyanide of potash, but that took a great deal of time and was prohibitive in its cost. Finally other castings were hardened in various ways, some by heating and dipping in water, no chemicals at all being used. The castings were broken up to see what the result was inside, and, to cut a long story short, the fact that came out finally was simply this, that the steel hardens beautifully *inside*, but that there is on the outside a thin skin of metal, about three to four hundredths of an inch in thickness, which, except by the cyanide process, does not harden at all. In all of the castings that had been hardened there was perfect hardening under this skin, and finally, the moral of this, and of some of the other unexpected incidents that Prof. Sweet has mentioned, is that we should *look below the surface*.

Mr. Bond.—We find in our experience in making taps and reamers that in case the steel has been over-annealed and has thus been decarbonized, the hardening does not take effect except under the surface, so that frequently, taps which appear to be soft, if turned again will harden perfectly. I think perhaps the castings referred to by Mr. Towne, may have been over-annealed, and in that way a percentage of the carbon eliminated so that the hardening would not take effect upon the outside surface.

Prof. Rogers.—The unexpected happened to me the other day in the matter of a new astronomical clock, which I am testing at the observatory. It had run for a month at a constant rate of 0.81 seconds daily. Suddenly it dropped 142 seconds, and then went on with its old rate, for two or three days, when it stopped suddenly. When the clock was overhauled, it was found that the pinion of the second-hand wheel had no end shake. The end-shake, originally, was so very small that the change of temperature in the room of two or three degrees had tightened it up to such an extent that it prevented the free action of the movement. After the clock was again started it resumed its old rate, and has so continued to the present time. It would be difficult to find another instance in which a constant rate has been maintained under similar circumstances.

The unexpected has *always* happened to me in this matter of

obtaining hardened steel, which has a homogeneous temper throughout the entire mass. The nearest approach to an even temper which I have ever been able to obtain, has been at the works of Miller, Metcalf & Co., of Pittsburgh, and of Brown & Sharpe, of Providence. A short time since, I asked the latter firm to set a price upon a hollow steel cylinder six inches in diameter, three feet in length and with walls half an inch in diameter, hardened and ground on the outside only. The price which was set—from $300 to $500, without guarantee against flaws—may be taken as the estimate of the extreme uncertainty always attending any difficult case of tempering held by those who have a full comprehension of the difficulty of the problem.

The difficulty of giving a homogeneous temper to a large mass of metal is so great, according to my experience, that it is never perfectly done. The test which I apply as the gauge of an even temper is a very severe one. If all the lines ruled upon a highly polished bar of tempered steel have the same appearance, the temper of the graduated surface is good. I have, however, never yet seen a set of graduations in which the diamond has with a constant pressure cut all the lines to the same depth. The diamond acting upon this polished surface detects the lack of homogeneity in the most perfect manner. If there is any person in this country, or in the world for that matter, who can temper a bar of steel three feet in length and for a depth of even a quarter of an inch at any price, I should be glad to make his acquaintance.

Mr. Bond.—I can say in regard to the unexpected in hardening, that we had occasion to make a set of gauges in which the sizes were all two ten-thousandths of an inch larger than the nominal sizes, and five days after the gauges were finished, one of them suddenly gave way in the center, a crack extending around it spirally, but not so as to injure the ends of the gauge. Out of curiosity I thought that I would measure the uninjured parts to see if any change had come in the diameter, and I found at both ends the diameter had enlarged forty divisions of the micrometer which is equal to six ten-thousandths of an inch, and which, as magnified, represented a space to the eye of about three-sixteenths of an inch under the microscope. This shows, I think, that if steel hardens at all, the internal strain must be something tremendous. I think this will also explain why steel in being hardened through the center, has a tendency to shorten under certain conditions,

the capacity to resist this enormous strain becoming less and less, until finally it settles down to something like permanency.

Mr. Hammond.—Some years ago there was a gentleman who wished to harden some very large sheets of steel, and he applied to several persons who said they could harden them, but they did not succeed; finally he discovered that a film of steam formed between the steel and the water and prevented the plate from cooling rapidly, the steam acting as a non-conductor of cold, and he arranged his large plate so that the liquid could pass down rapidly upon the two sides, thus giving a chance for the steam to escape and allow the water to cool the plate. In that way he succeeded in cooling and hardening this large surface. I would like to ask if this film of steam should always be kept from forming next to the piece of steel or iron during the process of hardening, would not the pieces of metal to be hardened be uniformly treated and the "unexpected" not come to pass?

Mr. Ashworth.—This subject, the more you consider it, the more prolific it becomes. But the most noted instance of the unexpected, in my experience, occurred quite a number of years ago in a special line of business. The matter of procuring castings free from the minutest pores was the desirable feature, and failure to secure this result was so common that in ordering castings a dozen would be ordered with the expectation of simply producing one perfect one. Now a casting which would ordinarily be considered perfect would be entirely ruled out in this case, simply from the fact that when pressure was brought to bear upon the casting in service, the plastic material being treated would work into these minute pores and destroy the clearness of the metal, and would also prevent rapid delivery. A great cry in that particular branch of business went up to produce these castings. At a certain time an outsider suggested the idea that the castings be made upon a chill, and, as is frequently the case with those interested in a matter, it was hooted at and ridiculed at first, and yet in the course of time this matter was brought to a practical issue, but it was a misnomer to speak of a chilled core. The core was heated up to a very high temperature, placed in its proper position, coated with a lubricant, and as soon as possible after the cast it was removed from the sand and the core knocked out. The result was marvelous and it soon spread through this special branch of business, and yet a period of eight years elapsed during which it was looked upon skeptically, and yonder in South Boston it was for the first

time put to the test, and after that it was used from Cape Cod to St. Louis. This simple matter of putting in this heated iron core covered with a lubricant, and taking it out quickly, was what did the business. That was a striking instance of the unexpected.

Mr. Oberlin Smith.—A little thing occurs to me which may possibly be of interest. I recently had occasion to make a small cup, like the sketch [Fig. 115], about three inches deep, and two or three inches in diameter into which I wanted some water to leak at a certain definite rate of speed, on the principle of sand running through an hour glass. I wanted it to leak to the inside through a hole in the cup, by submerging the cup in the water, one quarter of an inch. I made a hole in the bottom about one sixty-fourth of an inch in diameter. The material was brass one thirty-second thick. The water would not leak in. I might perhaps have worked out the matter by the ordinary data for capillary-attraction, and viscosity of liquids, and found that it would not, but I worked upon assumption. I could'nt get water through that hole unless I submerged it in water more than an inch. I then found that by making the hole three one-hundred-and-twenty-eights of an inch in diameter, I could get it to leak with three quarters of an inch submergence. I tried the hole greasy, and dry, and smooth, and rough, and I found that by making the hole larger at the upper end it would leak much more readily and with less "head." I found by thickening the bottom of the cup and thus making a longer hole, and tapering it from one thirty-second at the bottom to something like one sixteenth at the top (like *c* in sketch), I got better results. With that shaped hole the water would run in very readily with about three-eights head. At *a*, in sketch is shown a straight hole, and at *b*, one largest at the bottom—the worst shape.

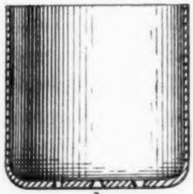


FIG. 115.

This experience was certainly the "unexpected," for it had never occurred to me that I could not make water leak through a hole. The same trouble of non-leakage happens when the water is inside the cup, that is to say, it will not leak itself empty if the hole is small.

Mr. Durfee.—Here is a case of the unexpected that occurred to me some seventeen years ago. I had an ordinary locomotive boiler unprovided with any pressure gauge, but having a safety

valve which was believed to be reliable, whose lever was graduated to 120 pounds. The man who was in charge of the boiler was instructed to keep the steam pressure at 60 pounds. The boiler was an old one, having been in use at least ten years, and how much longer I do not know. After the boiler had been at work for two or three weeks its fireman came running to me one day with every limb and feature indicative of extreme terror. As soon as he could speak he exclaimed: "The divil's got in the boiler, Mr. Durfee!" "What do you mean John?" said I. "The divil's got in the boiler, I tell ye; fur the weight's on the ind iv the liver an' 'tis up in the air as high's it con git; an' divil a mite iv stame's comin' out!" "Well," I replied, "we will go down and see if we can get the 'divil' out." On reaching the boiler I was surprised to find the weight at the 120 pound notch, and that it *was* as high as it could "git" (Fig. 113), and to my utter astonishment there was

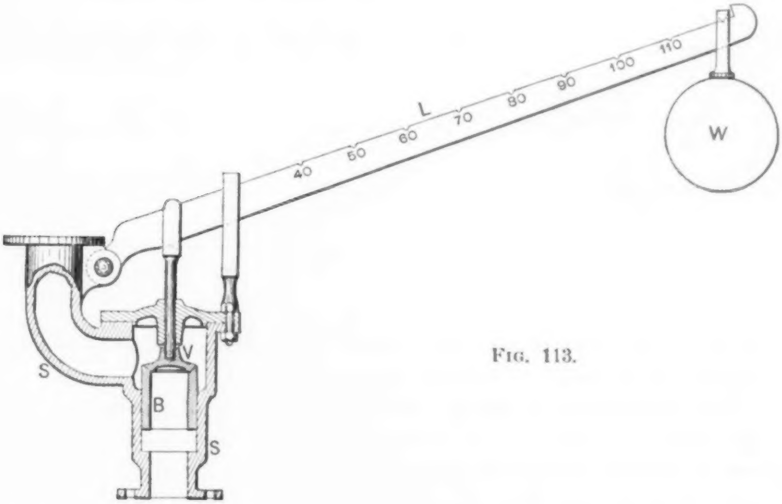


Fig. 113.

but a suggestion of steam escaping. My first thought was that in my immediate vicinity a boiler explosion was imminent; however, there was nothing to do but to ascertain the cause of existing conditions, and I therefore immediately tried the guage-cocks, found two full guages of water, and, from the appearance of the steam which escaped on opening the upper cock, it did not seem possible that there was 120 pounds of pressure upon the boiler. I then took a fire-hook and without much effort, pulled the safety-valve lever down, but on being released it slowly returned to its rampant

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position, and at no point of its movement was there any increase in the very small and feeble volume of steam escaping. I repeated this experiment with a like result, and then began to think that John's diagnosis was correct; for the boiler certainly seemed to be "possessed of a devil." Just how he got in, and how he produced the observed effects, I could not imagine; my previous experience with steam machinery, which I had been in the habit of regarding as embracing every species of "pure cussedness" that steam generators were heir to, did not furnish any clue to the mystery—for mystery it certainly seemed, and one which required prompt and decisive action for its solution. I therefore ordered John to draw the fire as quickly as possible, and in due time the valve was dismounted, and the state of facts disclosed is represented in the accompanying cuts (Figs. 113 and 114), which

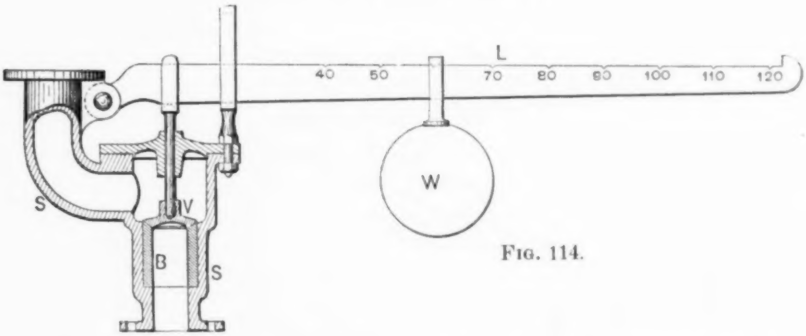


Fig. 114.

show the interior of the valve after, and before, it became "possessed of the devil" of deadly danger.

The engravings so clearly exhibit the details of the valve, and the relative position of its several parts, that very little explanation is necessary, and it will suffice to say that the shell S, of the valve was of cast iron, bored out to receive a cylindrical gun-metal bushing, B, which was *forced* into its place, and held there *simply by friction*. On the upper end of this bushing the seat of the valve V, was formed. As long as the bushing B, remained immovable, the steam pressure acted on a circular area of the lower surface of the valve V, having a diameter equal to that of the *interior* of the bushing B, but as soon as the repeated expansions and contractions incident to use and years had loosened the bushing B, the circular area upon which the steam could act had a diameter equal to that of the *exterior* of the bushing B,

which with the valve V, resting upon it, was then free to move upward (see Fig. 113) as far as the lever L, would permit, acting simply as a piston, and affording no outlet to the steam save that due to the trifling leakage arising from its imperfect fit in the shell S. The area of the last named circle was a little more than *double* that of the first.

It is not uncommon for a coroner's jury, while attempting to ascertain the cause of some disastrous boiler explosion, to be told by the confident witness (he is always on hand in large force on such occasions), that "the safety valves were all right, as they had been examined but an hour before the explosion occurred." Such evidence in the case of a safety valve like the one which I have described would be of no value whatever, as the fact of its being in good order when examined would be no assurance that within the next five minutes it would not be worse than no safety-valve at all. It is extremely probable that there are many such valves in use to-day, and also that valves of such construction have been the cause of many unexplained explosions.

CXCIII.

THE MICROSCOPE IN THE WORKSHOP.

BY WM. A. ROGERS, CAMBRIDGE, MASS.

It is a sound principle of mechanical construction that a secondary tool should never be used when the work can be as well done by a primary tool. If the capacity and the efficiency of the primary tool can be increased so as to meet every requirement, it is better to dispense with the secondary tool altogether.

In the ordinary operations of the workshop, the lathe and the planer are the primary tools, while the calliper, with the graduated scale, is the secondary tool. Let us take the most simple case. It is required to turn down a piece of metal to a given diameter. In order to make the assumed case as simple as possible, we will assume the required diameter to be an even inch. The calliper is set for this unit of length, either from a graduated scale or, more accurately, from an end-measure inch with parallel faces. The setting in the latter case is done by the sense of feeling. We thus introduce an additional element of complexity, since sight is at once the primary sense and the ultimate test of a given limit of extension upon which the workman must rely. When the market is supplied with graduated scales from which any required length may be taken by the sense of feeling, it will be in order to defend the practice of relying upon this sense as a final test in measurements of extension. As a differential test, it is both useful and accurate. As an absolute test it had better be abandoned. It is a make-shift at best.

Assuming that the calliper has been set to an exact inch, the workman turns the piece of metal to the required size by a series of approximations, with the ever-present risk of going beyond the required limit. During the final part of the operation he stops the lathe to test the diameter with his calliper. He then takes another chip, stops, tries, starts, stops, tries until the subtle and ever-varying sense of feeling satisfies him that he has obtained the correct diameter. But after all, the uncertainty in the setting of

the calliper remains, and this uncertainty is generally greater than that which would be found to exist in the comparative trials of the diameter. If now we increase the required unit, and especially if fractional increments are added, the problem of transferring a required length from a scale to a calliper becomes a most serious one.

Every machinist must admit that there would be a great gain both in time and in accuracy if he could be sure of knowing the exact amount of work done at any instant, if he could see and measure the varying diameter of his cylinder, and at the same time control the amount of work to be done by the manipulation of his lathe, stopping at the exact instant when the required diameter has been obtained—if he could turn two shoulders upon a cylindrical shaft to any required length in one operation, stopping the last chip at the instant the correct length has been obtained—if he could turn a shaft to a required taper, and be sure that the correct angle of inclination has been maintained during every part of the operation—if he could—but I forbear further enumeration, lest the enthusiasm of inexperience may lead you to overlook the gravity of the demand for a radical change in our present methods not only of obtaining, but of applying measurements of length; a demand made in the interest of *accuracy*, *uniformity* and *economy*.

It is quite worth our while, therefore, to discuss the question whether the microscope considered as an attachment to the lathe and to the planer will not enable us to dispense with that secondary tool, the calliper, in a majority of cases, and at the same time increase the precision and the economy of mechanical construction.

The microscope has been generally accounted a delicate instrument, especially adapted to the minute study of delicate organisms and to the measurement of minute dimensions. By common consent it has been relegated to the laboratory of the investigator and has been considered quite unsuited to the every-day operations of a machine shop. One reason for this view formerly had great force. Until the invention of the opaque illuminator, by the late Robert B. Tolles—a single prism inserted between the two front lenses of an objective—the illumination of objects in the field of the microscope was for the most part obtained by transmitted light, thus requiring a transparent substance. A previous invention by Professor Hamilton L. Smith, of Geneva, N. Y., and since patented under a slightly different form by Beck, of London, gives

equally good results, but the care and the time required in adjustment and the difficulty in manipulation would prevent its use in the workshop. With Tolles' illuminator, however, it is easy to obtain at once a perfect illumination of a metal surface under almost any given conditions. It is only required that one face of the prism shall be presented toward the source of light, whether it be an artificial flame or the open sky.

It has been assumed also that a machine to which a microscope is attached must be most firmly mounted upon solid piers insulated from the building and in a room in which a steady temperature can be maintained. This is by no means necessary in ordinary workshop practice. The difficulty with regard to solidity of foundation can be practically overcome by adding mass to the machine, and the question of temperature will be taken care of by having separate standards of length of the different metals in ordinary use.

Only one other objection remains to be overcome. It is the common impression that the delicate adjustments of the microscope which are continually demanded—especially the adjustment for focus—can only be made by the most delicate and sensitive means. No impression could be more erroneous. Give me a small lead hammer and I will set the stop of my comparator to a given line in half of the time and with greater precision than it can be set by means of a screw movement. Give me a vertical movement by means of an excentric disc and a long lever-arm, and I will bring the surface of a plate weighing 100 pounds into the focus of the objective quite as quickly and quite as accurately as a similar adjustment could be made in the hands of a professional microscopist.

Having thus cleared away some of the objections which would be very properly made *a priori* against the proposal to use the microscope as an essential part of the lathe and of the planer, it will now be in order to point out some of the ways in which it can be most effectively used in the interest of accuracy and of economy.

I shall, in this paper, limit the illustrations of the applications of the microscope, to four operations in lathe work, viz.:

(*a*) Turning shoulders upon a shaft to a required length.

(*b*) Turning a face plate to a required diameter.

(*c*) Turning a shaft to a required diameter.

(*d*) Turning a shaft to a required taper.

In Fig. 67, f^1 represents a microscope which is attached to the

carriage of the lathe. The microscope *f*, also attached to the carriage, is adjusted over a scale *g*, which rests upon a plate *h*. This plate is attached to the lathe bed. It has a fulcrum at h^1, and an adjustable movement in elevation by means of a screw h^2. Two or more flexure screws secure a parallelism of the upper surface of the plate at every point with the horizontal plane described by the carriage.

The shoulder next to the head stock is supposed to be already turned. The micrometer line of microscope f^1 is then set upon the limiting edge and the zero of the graduations upon the bar *g* is brought into coincidence with the micrometer line of microscope *f*.

It is obvious that if the relative positions of the two microscopes remain unchanged, the distance measured on the scale by the

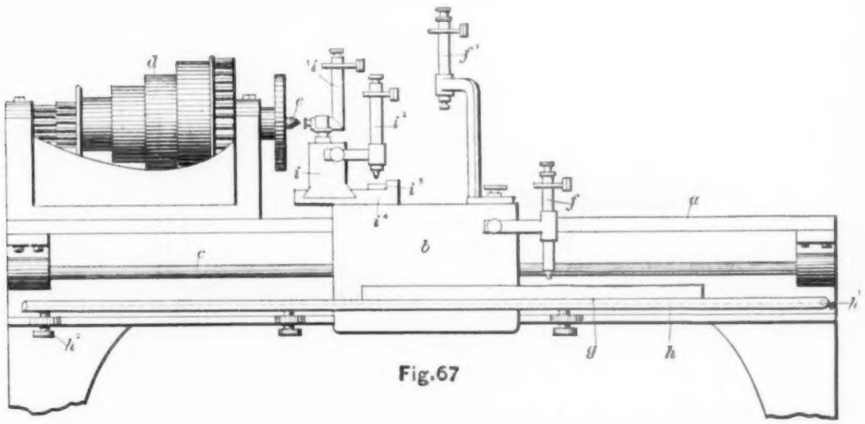


Fig.67

movement of the carriage will be indicated upon the shaft by microscope f^1. It will be seen that by the use of two microscopes, the necessity of adjusting the cutting tool with respect to the fixed points of reference is entirely obviated. The position of the tool bears no relation whatever to the dimensions sought. When the first chip is taken—a little outside of the required limit—the amount by which the carriage must be moved will be indicated by the micrometer line of microscope f^1. It is to be noted, however, that the tool will do its work for one-quarter of a revolution before the amount of work done is indicated by the microscope, but the proper allowance can be quickly made by means of a graduated scale in the eye-piece of the micrometer of microscope f^1.

In order to turn a face plate to a required diameter, adjust vertically the micrometer line of microscope *i* over the point of the face

plate which is stationary during its revolution. Adjust microscope i^2 upon the zero of the transverse scale. The required diameter will then be indicated by the movement of i^2 over the scale and the indicated limit of the circumference through microscope i.

In order to turn a shaft to a given diameter, it is necessary to set the micrometer line of microscope i^2 in the line between the centers of the lathe. Since it is not possible to do this directly, we introduce an auxiliary measuring gauge k k^1, Fig. 68, which will also be found to be of great service in testing the various adjustments of the lathe. k is a cylindrical shaft, ground to a true cylinder, *e. g.*, in a Brown & Sharpe grinding machine, while supported at its centers. k^1 is a ring which slides freely upon the shaft and is capable of being firmly secured to k by projecting flanges (not

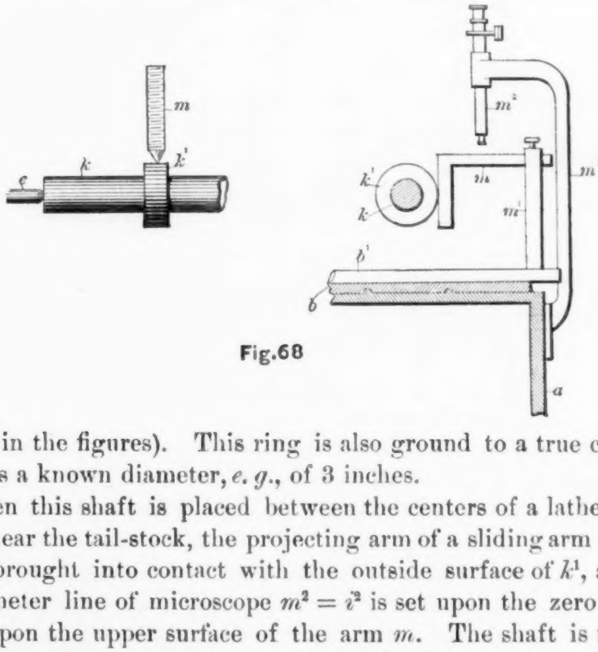


Fig.68

shown in the figures). This ring is also ground to a true cylinder and has a known diameter, *e. g.*, of 3 inches.

When this shaft is placed between the centers of a lathe and k^1 is set near the tail-stock, the projecting arm of a sliding arm m, Fig. 68, is brought into contact with the outside surface of k^1, and the micrometer line of microscope $m^2 = i^2$ is set upon the zero of the scale upon the upper surface of the arm m. The shaft is now removed and the carriage is moved inwards through the space of 3 inches, as indicated by the scale. The micrometer line of the microscope will now be coincident with the line between the centers of the lathe, if the proper adjustment of the tail-stock has been made.

It is not probable that this adjustment would need to be tested very often, if the microscope is firmly attached to the carriage.

If the point of the cutting tool is brought into adjustment under

the micrometer line of the microscope, the required diameter can be read off directly from the scale. Since, however, the wear of the cutting tool would probably be appreciable, this direct method of measurement would not probably be as satisfactory as the more indirect method of employing the additional microscope f^1 in connection with an auxiliary calliper scale.

For any diameter up to about one inch with a 1-inch objective, we may proceed as follows. When the contact of the arm with the surface of the ring k^1 is made, set the micrometer line of microscope f^1 tangent to the ring on the same side. Then, for any radius of the shaft to be turned, less than the working distance of the microscope, we shall have, after an inward movement of 3 inches, the micrometers of both microscopes coincident with the line between the centers of the lathe—one set upon the scale and the other over the shaft. The required diameter will then be obtained when the micrometer line of the microscope i^2 reaches the required point on the scale and the micrometer line of f^1 is tangent to the circumference of the shaft.

With a slight vertical movement at right angles to the plane of the ways, microscope i might advantageously take the place of microscope f^1. It would be necessary to raise the microscope in passing the center of the lathe, but since it would fall back upon the same seat, there would be little danger of disturbing the relative positions of the two microscopes by this movement.

In order to set the tail-stock for turning any required taper, set the ring at the end of the shaft adjacent to the head-stock and set the microscopes as described above. First set the ring at the point where the largest diameter is required, and then adjust the tail-stock in the usual way, so that the reading on the transverse scale shall indicate the lesser diameter.

It is obvious that two microscopes attached to the tool carriage of a planer, in connection with longitudinal and transverse scales, may be made to serve the same purpose as in the lathe. The microscope may be made especially useful in leveling up the bed of a planer. Place beside the lathe a shallow dish of mercury extending its entire length with means of adjustment similar to plate h, Fig. 67. Attach a microscope first to one end and then to the other end of the planer, and make the adjustment for level such that the surface of the mercury is sharply in focus under the microscope in the two positions. The bed-plate can then be blocked up at the intermediate points until every point is in focus.

With regard to the expense of fitting up a lathe with the microscopic attachments indicated above, it may be estimated at about $125, exclusive of the cost of the graduated scales. Only two objectives would be needed, at a cost of $20 each.

If it is urged that this direct process of applying dimensions in mechanical construction is not practical, or not well adapted to ordinary machine-shop practice, if it is insisted that the calliper *must* be used, I shall still maintain that a reform is needed in the method of setting the calliper for a required measure of length, and that there is a better way than that ordinarily followed. It is simply impossible to set a calliper with any degree of precision from a

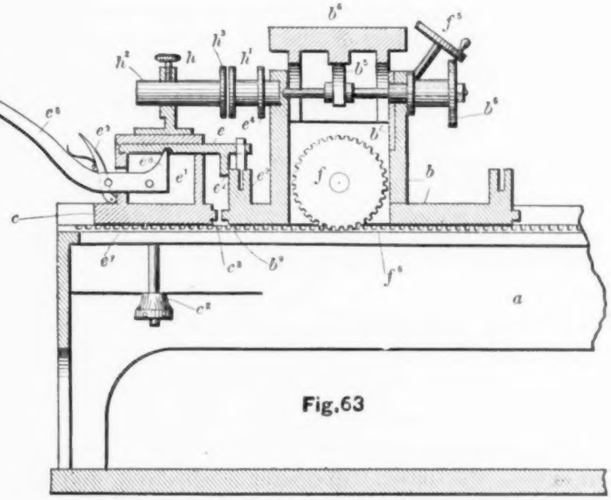


Fig. 63

line scale. End-measure gauges are expensive and they require the most careful manipulation. Moreover, as ordinarily made, they can only be used for aliquot parts of a given unit. They are useless except for a few special lengths, and the extreme length seldom exceeds 6 inches.

There are four requirements which ought to be met absolutely in any proposed system of obtaining and transferring measures of length.

First.—All measures of length must be referred to one line-measure standard. This standard should be at least one yard in length, and the subdivisions should be such that any required length can be taken from it directly to $\frac{1}{1000}$ inch. Subdivisions less than

this limit can be better obtained with the aid of an eye-piece micrometer in the microscope. The yard should be standard at 62.0° Fahr., and the subdivisions should be so exact that there would be no necessity of applying corrections.

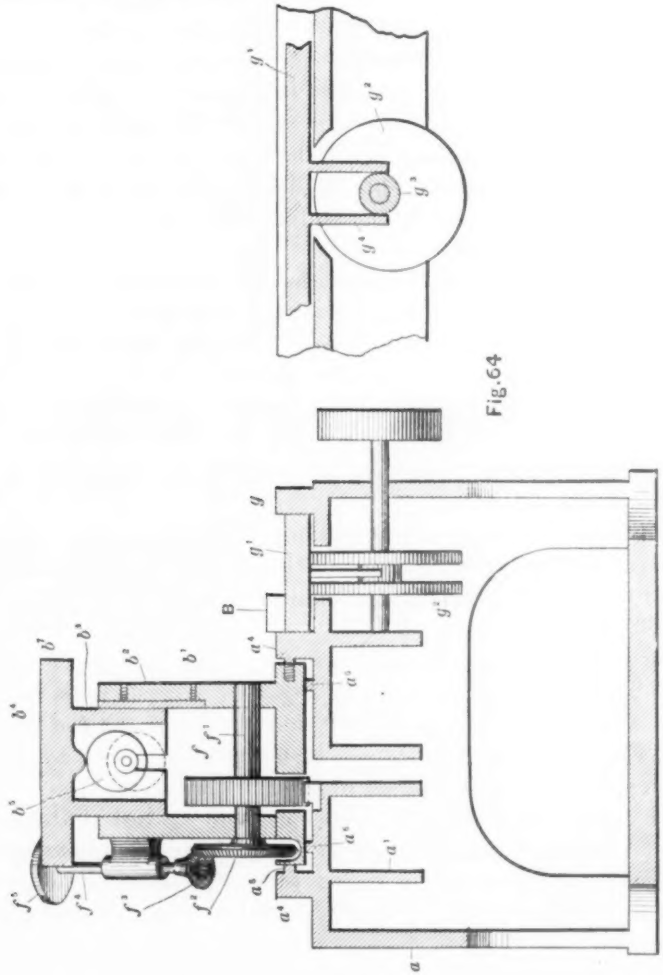


Fig. 64

Second.—The calliper gauge from which measures are to be taken for use in the machine-shop must be universal in its action. It must be capable of being set to correspond to any required length, aliquot or fractional, as indicated upon the line standard.

Third.—It must be so simple in form, so direct and so sure in

its action, that the amount of time required in its manipulation shall be less than that required in the present practice of obtaining measures of length.

Fourth.—The cost of construction must be such that any shop of moderate capacity can afford the outlay.

It is the experience of the writer that these conditions are fulfilled in the universal microscopic calliper shown in the Figures 62, 63 and 64. This machine has been in constant use in the comparing room at Harvard College Observatory for nearly a year, and while it has less conveniences than the larger universal comparator, it has been found to be capable of doing quite as accurate work. Its first cost, not including the calliper attachment, but including cost of patterns, was $320.

The main features of this apparatus and the method of operation will be seen from the following outline references. In Fig. 62, the microscope slide b^2, which is closely fitted to the projecting side bearings a^4 and a^5 and to similar elevated bearings beneath, is carried the entire length of the bed by the rack f^6 and the bevel gear pinions $f^2 f^3$ and the pinion f, Fig. 64. The microscope plate b^4 has a slow motion adjustment in elevation by means of an excentric b^5, Fig. 63.

The stops c c^1 can be set at any desired position upon the bed. They can be firmly secured without the slightest disturbance of the stops by means of large circular clamps beneath the bed plate at e^2, Fig. 63.

The plate r extends the entire length of the bed and is closely fitted between the walls g and g^1, Fig. 64. It rests upon two excentrics opposite g^5 and g^6, Fig. 62, and shown in the end view at g^1 g^2, Fig. 64. The adjustment in elevation is made by means of levers inserted in the wheels g^5 g^6.

The gravity lock of the microscope plate against the stops, is shown in Fig. 63. The weighted lever e^8 can be thrown out of connection by means of the spring catch e^9, when it is desired to make the contact with the stops by means of the rack and pinion movement.

The graduated bar B rests upon the bed of the machine and against a vertical ledge which extends the entire length.

The universal calliper s s^1 rests upon the plate r, and can be placed in any desired position. The two parts s s^1 move independently; s^1 being carried by two arms attached to the microscope slide b^2.

The operation of setting the calliper for any required length will be as follows:

(*a*) Clamp section *s* in any convenient position upon the plate *r*.

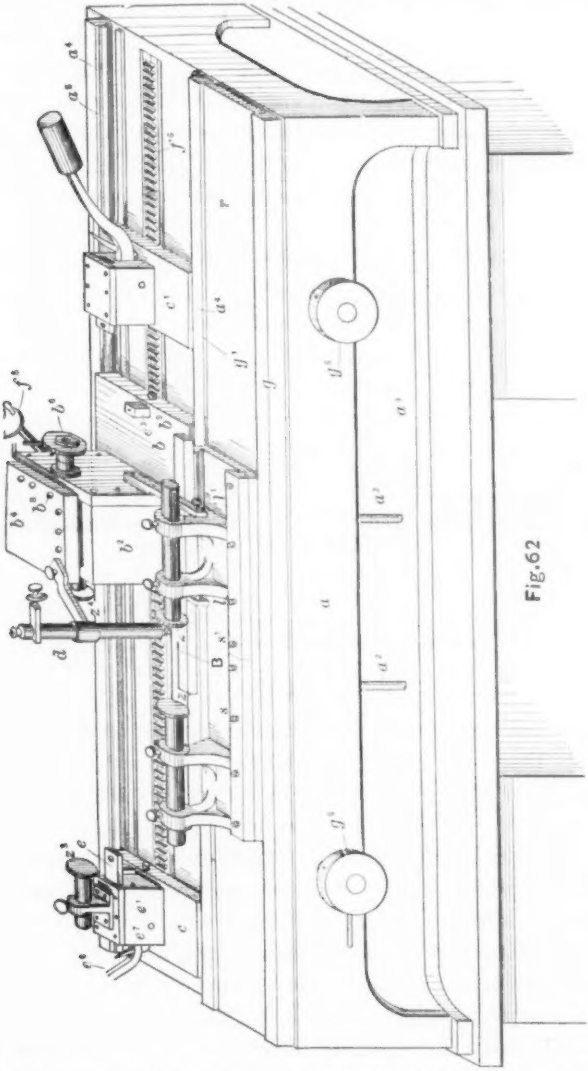


Fig.62

(*b*) Bring section s^1 into a position such that the stops z and z^1 will be in contact with a clear space between the bases s s^1.

(*c*) Make the connection between b^2 and s^1 by means of the screws at l and l^1.

(*d*) Set the micrometer line of the microscope in coincidence with the zero line of the graduated bar *B*.

(*e*) Then, when the microscope slide *b* moves over any required distance as indicated by the graduated scale, the stop z^1 will move the same distance away from the stop *z*. The cylinders which form the stops are hollow, and a rod (not shown in the figure) passes through both, which serves as a support for the transferring calliper for inside measures. For outside measures, allowance is made for the thickness of the two face plates in setting the microscope slide. For support, the transferring calliper rests upon the two cylinders.

In some kinds of work it will be found quite as convenient to attach one stop z^1, to one stop of the comparator and the other to the vertical face of the microscope slide b^2.

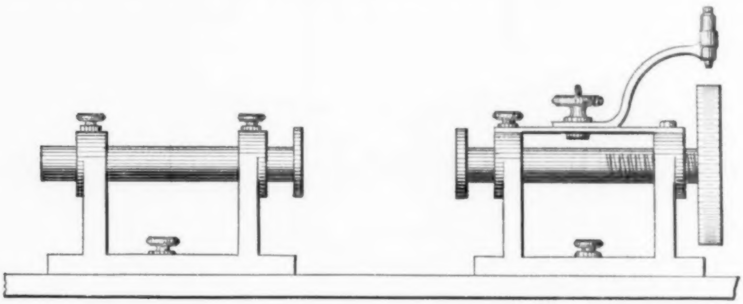


Fig.66

Another convenient form of the adjustable stops is shown in Fig. 66. They can be substituted for stops $c\ c^1$ with advantage when minute *differences* in length are to be measured.

This machine can be used to the best advantage by adopting the system of delivering measures of length from a "standards room," just as tools are now delivered from a tool room. This can be most economically done by the aid of the small boy. If a workman wishes his calliper set for any distance, he calls "boy." The messenger boy receives the calliper and a card on which is written the dimension required. He takes it to the "standards room" and after a brief delay he receives back the calliper properly set, together with the card on which are written the figures which have actually been used, and delivers both to the workman in half the time it would take him to set it from a scale with anything like equal precision.

There can be no more convincing way of demonstrating the feasibility of this method of obtaining and transferring measures of length than by performing some of the operations indicated. I enumerate below a few of them, with an estimate of the time which each operation requires.

It is required:

(*a*) To bring the surface of the graduated scale into a plane parallel with the plane described by the microscope carriage. Time, 30s.

(*b*) To set the longitudinal line of a graduated scale parallel with the ways of the comparator, 45s

(*c*) To "set for focus" upon a given line. By lever movement, 6s. With lead hammer, 4s.

(*d*) To set for exact coincidence with a line of the scale by tapping the bar with a lead hammer, 6s.

(*e*) To set the stops $c\ c^1$ to correspond with any given distance. For short distances, 50s. For long distances, 1m. 10s.

(*f*) To compare a Whitworth end-measure yard with a line standard and with a limit of error not exceeding one ten-thousandth of an inch, 2m. 30s.

(*g*) To compare Whitworth 12 inch, 6 inch, and 1 inch end-measures with corresponding line measures, 1m. 45s. each.

(*h*) To compare a standard end-measure inch with corresponding line measures, 1m. 30s.

(*i*) To illustrate the limit of accuracy in measurements by the sense of feeling by adding to or subtracting from an end-measure inch one twenty-five-thousandth of an inch, 2m.

(*j*) To set a calliper for the distance one inch, upon the machine and compare it with a Pratt & Whitney inch, 2m.

(*k*) To illustrate the method of delivering calliper measures for use in the machine shop.

As the result of experiment, it has been found that the actual time required to perform the operations indicated is less than one-half of the limit given above. For the experiment under division (*h*), the writer is under obligations to the Brown & Sharpe Mfg. Co. for the loan of the five end-measure gauges of their manufacture.

From a comparison of the different end-measure inch gauges, the following results were obtained:

A plus sign indicates that the corresponding space is *too short;* a minus sign, that it is *too long.*

Space.	Correction.	Space.	Correction.	Space.	Correction.
1	−.000006 inch.	7	+.000005 inch.	13	+.000000 inch.
2	+.000024	8	−.000004	14	+.000020
3	+.000000	9	−.000012	15	−.000016
4	+.000000	10	−.000042	16	+.000014
5	−.000024	11	−.000026	17	+.000016
6	+.000005	12	+.000036	18	+.000010

It must be understood that any apparent degree of accuracy expressed by a figure in the sixth decimal place in which the unit is one inch, is probably fictitious.

(ADDED SINCE THE MEETING.)

Since this paper was put in type, the writer has received from the Pratt & Whitney Co. an end-measure inch, kindly loaned for this occasion. This standard was constructed from a four-inch line standard, graduated by the writer in 1881, and which has served as the basis of all the gauges made by this company. It has no correction at 62.0° Fahr. The Betts Mfg. Co. also kindly loaned a standard inch, made especially for the present purpose. They also sent the Whitworth inch, which has served as the basis of their gauges of this dimension. The same company some time ago sent two additional standards for comparison to with my line standard. Thus, with my own Whitworth standard, bought in London in 1880, we have five independent standard inches for comparison under division (*h*). With the aid of a recorder, it has been found quite easy to make all of these comparisons without haste, inside of the limit given for a single comparison as given under (h).

It will perhaps be worth while to give the results obtained. The particular inch with which the comparison was made, is the first inch of a standard one-half yard, subdivided to tenths of inches. In order to obtain a standard for which no sensible corrections of any kind would be required, 96 separate trials and corresponding investigations were made.

The relative errors of the separate inches are given below. The half yard has no correction for errors of total length at 62.0° Fahr.

The Pratt & Whitney Co. inch is 1 millionth of an inch *too short.*

The new Betts inch (probably a copy of the P. & W. inch) is 21 millionths of an inch *too long.*

The Betts-Whitworth is 202 millionths of an inch *too short.*

The Rogers-Whitworth is 236 millionths of an inch *too short.*

The Betts inch, No. 1, made in 1883, is 202 millionths of an inch *too short.*

The Betts inch, No. 2, made in 1883, is 196 millionths of an inch *too short.*

It will be seen that these comparisons bear out the claim of the Betts Mfg. Co. that their gauges are practically exact copies of the Whitworth standard. *All* of the Whitworth gauges which have been examined by the writer have been found *too short.*

Referring to division (*i*), it may be said that the difference in the length between the first two gauges in the list can be detected by the sense of feeling with considerable certainty.

(The calliper, with its microscopes, although set up to be exhibited when the paper was read, was not described before the meeting, the author being prevented from at tendance. For the same reason, the paper received no discussion.)

CXCIV.

A NEW FORM OF STEAM CALORIMETER.

BY GEORGE H. BARRUS, BOSTON, MASS.

THE calorimeters ordinarily used for measuring the dryness of steam operate in an indirect manner. They first determine how much heat is contained in the sample tested. The condition of the steam with respect to dryness is then shown by comparison of the result with the quantity of heat given by the authorities for dry saturated steam. The sample contains moisture in proportion as the result is less than the authorized standard. It contains what is termed superheat, in proportion as the result is greater than the standard.

Suppose the steam has a pressure of 80 pounds per square inch above the atmosphere. The total heat given in the tables for this pressure is 1212.6 B. t. u. above 0 Fahr. If the calorimeter test yields, for example, 1190 B. t. u., it falls short of the standard 22.6 B. t. u., which is an indication that the steam contains $\frac{22.6}{885.7} = 2.5$ per cent. of moisture. If the test yields say 1225 B. t. u., it gives an excess over the standard of 12.4 B. t. u., which is an indication that the steam is superheated $\frac{12.4}{.475} = 26.1$ degrees.

Calorimeters which work on this principle do not give accurate indications of the amount of moisture in steam, unless thermometers and scales are employed which are sensitive and which register minute changes, and unless extreme care is used in the manipulation of the apparatus. In the case of the *barrel* calorimeter, the one commonly used, supposing the range of temperature to be 50 degrees Fahr., and the weight of steam used for a test 20 pounds, an error of half a degree in the observations of the thermometers, or an error of one-fifth of a pound in the observations of the weight of water in the barrel, causes an error of one per cent. in the result. A larger error than this might ensue, if the observa-

tion of each extreme was erroneous, and all the errors acted in the same final direction. Unusually close work is more important than would first appear, for the reason that the moisture in steam of ordinary dryness, does not often exceed 3 per cent., and a small error becomes large by comparison.

Calorimeters of the continuous type are more accurate in this respect, for they deal with larger quantities of steam and water and a greater number of observations for a given test. But they require equally careful manipulation.

The new form of calorimeter presented in this paper is offered because it so far reduces the errors referred to that they become almost inappreciable, and it greatly simplifies the operation of making an accurate test. In order to use it, it is simply necessary to observe thermometers which show many degrees change of temperature for a change of one per cent. of moisture. It should be said at the outset that it is intended to be used only for testing moist steam.

Unlike the calorimeters referred to, the new apparatus operates directly upon the moisture contained in the sample of steam tested. It evaporates the moisture, and determines its amount by measuring the amount of heat required for this purpose. The evaporating agent is a current of superheated steam, and it is the superheat of that steam which is utilized to do the work. The determination of the amount of superheat required constitutes the immediate object in view, and this is attained by observing the temperature of the superheated steam before and after its use. When the quantity of superheated steam equals that of the sample tested, the evaporation of one per cent. of moisture reduces the temperature approximately, 18.7 degrees Fahr.* In proportion, then, as the fall of temperature is greater or less than this number of degrees, the amount of moisture sought for is greater or less than one per cent.

It is immaterial what the exact quantity of steam is which is tested, so long as the relation borne to the current of superheated steam remains constant. Weighing is therefore dispensed with altogether, and the desired relation between the quantities is maintained by causing each current of steam to pass through an orifice of fixed size. To obtain equal quantities, which is the relation most to be desired, the two orifices are made of practically the same

* This is the exact quantity for 80 pounds pressure. For other pressures the quantity is obtained by dividing the latent heat by 47.5.

size and the steam is admitted upon them with a pressure common to both.

The general form of the apparatus is shown in the illustration, Fig. 86.

Two half-inch connections are made with the steam pipe or

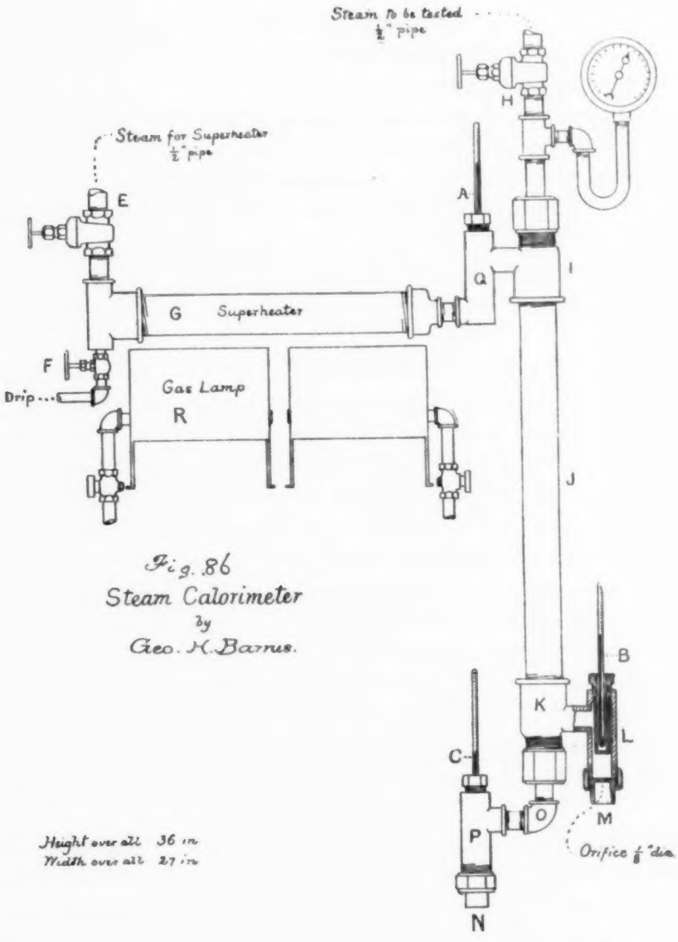


Fig. 86
Steam Calorimeter
by
Geo. H. Barrus.

boiler, one for the attachment of the pipe H, which is to convey the moist steam to be tested, and one for the attachment of the pipe E, which is to supply the superheater. The moist steam enters and passes through the pipe H O, surrounded by the jacket I J K. On its exit at O, it passes through the thermometer cham-

ber P, and emerges through an orifice at N, about $\frac{1}{8}$ of an inch in diameter.

The supply of steam at E enters the superheater G, which is a $1\frac{1}{2}$ inch pipe, and is there heated by the gas lamps R. It then passes through the thermometer chamber Q, and enters the jacket I J K. The hot steam traverses the length of the jacket, passes through the thermometer chamber L, and emerges at the orifice M, which, like the one at N, is about $\frac{1}{8}$ of an inch in diameter. Each thermometer chamber is provided with an immersed oil cup, containing a thermometer as shown at L. The ends of the jacket are provided with insulated stuffing-boxes (not shown in the drawing), through which the inner pipe passes, and this pipe is subdivided into a number of smaller pipes to increase the heating surface. The outside of the apparatus, with the exception of the superheater, is protected with non-conducting covering.

In operating the calorimeter, full pressure is admitted to both pipes, and the jacket steam is heated till the thermometer C commences to rise above a normal temperature, a condition which indicates that the moisture is all evaporated from the steam to be tested. As soon as the indications have become constant, the difference between the thermometers A and B is observed, and the quantity thus obtained is corrected for the amount which C has risen above the normal temperature. A still farther correction is applied for radiation from the outside of the jacket, which is found by passing steam of the same average temperature through the jacket, without admitting steam to the inner pipe, and observing its fall of temperature. With hair-felt covering, the radiation causes a reduction of from 10 to 15 degrees, varying according to the degree of temperature. The weight of steam used by each pipe when the pressure is 80 pounds, is about 60 pounds per hour.

The result gives the number of degrees of superheat, corresponding to the moisture which has been evaporated. To find the percentage of moisture, this number is divided by the number of superheat degrees, corresponding to one per cent. of moisture, that is, to one per cent. of the latent heat at the existing pressure, which, as already noted, is $\frac{\text{Latent Heat}}{47.5}$.

To revert, now, to the matter of the error to which the new calorimeter is liable, a difference in temperature of approximately 18.7 degrees appears when a change of one per cent. occurs in the amount of moisture. If an error, therefore, of 18.7 degrees was

made in observing the required difference of temperature, it would effect the result only one per cent. In the case of the barrel calorimeter, an error of 18.7 degrees would effect the result 37.4 per cent.

APPENDIX II.

EXPERIMENTS ON THE FLOW OF SUPERHEATED STEAM THROUGH AN ORIFICE.

It is proper in connection with the paper on a "New Form of Steam Calorimeter," to present the following memoranda relating to some experiments made by the writer in October, 1878, on the flow of superheated steam through an orifice. The experiments were made with different degrees of superheating, and in two series. In one, the pressure over the orifice was 28 lbs. per square inch above zero, and in the other the pressure was 58 lbs. above zero. In both cases the steam was discharged into a surface condenser open to the atmosphere, that is, against a pressure of 14.7 lbs. above zero. The orifice consisted of a cylindrical opening in a cast-iron plug 0.09 inch in diameter three-quarters of an inch long. The temperature of the steam was taken at a point about five inches above it. The results were as follows:

SERIES OF TESTS WITH 28 LBS. PRESSURE.

Kind of Steam.	Number of Degrees of Superheating.	Weight of Steam Discharged per Hour.
	Deg. Fahr.	Lbs.
Saturated	0	10.16
Superheated	10	10.09
do.	109	9 16
do.	210	8.48
do.	309	8.14

SERIES OF TESTS WITH 58 LBS. PRESSURE.

Kind of Steam.	Number of Degrees of Superheating.	Weight of Steam Discharged per Hour.
	Deg. Fahr.	Lbs.
Saturated	0	20.22
Superheated	10	19.73
do.	109	18.42
do.	209	17.18
do.	306	16.27

In round numbers an average increase of temperature of 16 degrees in these experiments reduced the rate of discharge one per cent. The results for saturated steam agree substantially with the theoretical discharge calculated from Zeuner's formula.

DISCUSSION.

Mr. Babcock.—There is an obvious necessity for some method of ascertaining the amount of water entrained with steam more accurate than we now have, and it is more pressing as we know more about the results with those heretofore used. At the Electrical Exhibition, the judges found it necessary to throw out all the tests made by the calorimeters and guess at the results, as they could guess nearer than they could measure. The way they found that fact out was by comparing results by three or four different methods which did not agree so well as their guesses did. Therefore, the guesses must be more correct.

We are indebted to Mr. Barrus for the suggestion of a new method of testing the quality of steam; whether it will prove to be any more accurate than the others is a question entirely in the future. I can suggest several difficulties. In the first place, it is a great deal more difficult than would be imagined to obtain two orifices of exactly the same size. You will recollect that Professor Blake in some experiments on the flow of steam, some years ago, tried to produce two orifices of exactly the same size, by driving a hardened steel mandrel, carefully ground to shape, through the openings in two diaphragms. When he came to test them by using first one and then the other in the same place, he found the results varied, and examination with a microscope disclosed a little speck of dust in one of them, which made a material difference. Now, I imag-

ine it will be more difficult to obtain accurate results by this apparatus than Mr. Barrus would suppose. Again, there is evidently a considerable difference in the rate of the flow of steam according to whether it be moist, or dry, or superheated. Mr. Barrus has anticipated that point, and given a table of some such differences. Whether it will be possible to ascertain accurately these differences so as to make proper allowances therefor in the results obtained by this apparatus, is questionable. Again, this is arranged for steam of one definite amount of moisture; that is, steam which is homogeneous. When it is set once it is supposed to be set for a continued experiment, but, as is well known, the moisture in steam will vary considerably at different times. A little difference in the velocity of flow of the steam from the boiler will frequently make a little difference in the quantity of moisture, and sometimes even a question of firing will produce a difference in the quality of the steam, so that it is not probable that the steam will remain homogeneous during any given test. These are some of the points that suggest themselves at first sight as possible objections to this very ingenious apparatus.

Prof. Lanza.—We have been using one of the old style Barrus calorimeters, such as is described in the report of the Boiler Committee of this Society,* but we have made a modification by means of which we bring the condensed water to the temperature of the air before collecting it to weigh. It has worked very well, as the results obtained from testing the quality of the steam produced by our boilers have been, as a rule, quite consistent, from one to two per cent. priming. I desire to call attention to one matter which seems to me to be the secret of a great deal of the trouble which often occurs with calorimeter tests in general, and that is the fact that correct results cannot be obtained when the steam pressure varies during the test. If we begin the test with steam of 75 pounds pressure, and then the pressure drops to 70 pounds, the walls of the pipe by which steam is conducted to the calorimeter, being at the temperature corresponding to saturated steam of 75 pounds pressure, will evaporate a part or the whole of whatever moisture is present. On the other hand, with a rising pressure the reverse effect would be obtained. In either case, the calorimeter would appear to give an erroneous result, whereas, in reality, the difficulty is not in the calorimeter at all, but in the pipe leading to it.

* Trans. A. S. M. E., vol. vi., p. 297.

Prof. Thurston.—I do not know, Mr. Chairman, when I have heard a paper which has interested me more than this, or which has put in so few words one of the most interesting improvements made in our apparatus for experimental work. I presume very few members of the Society who have not had experience in this direction have any idea of the importance of the work which Mr. Barrus has attempted in this little invention of his. Twenty years ago, fifteen years ago, even, the quality of the steam leaving a boiler was not usually regarded as a matter of any very great importance, and up to that time the only man, so far as I am aware, who had ever attempted to determine whether the quality of the steam leaving a boiler was a matter of importance in connection with experiments on the power or efficiency of boilers, was M. Hirn, who, in all his work, had taken a physicist's stand-point, and had tried to do good experimental work. As early as 1865 he began his attempts to determine what was the quality of steam leaving boilers, but up to 1871, there had never been any considerable amount of experiment in this direction, and the tests up to that time put forward were beginning to be looked upon as being entirely useless for important purposes. Assuming that a boiler was in good order, there might be no question that it would furnish fairly dry steam, and yet nobody knew, in any one case in which he might be specially interested, whether the steam were wet or dry, and if wet, to what extent it was wet.

In 1871, it happened to be left to me to determine in certain cases the relative value of a number of boilers of different designs which were placed in competition, and as the chairman of the committee in charge, I was compelled to determine upon a method of experimentation that should settle which of these boilers was best. But I could not then see the way through that problem. I could not see any way then of conducting the test in the ordinary manner, and of saying when the test was finished which boiler was best. We finally went to an expense that was considered at the time somewhat extraordinary. We obtained some tubes from M. Root, who showed the most intelligent interest in the subject we had in hand, and with those tubes we built up a surface condenser, and into that condenser we turned all the steam which the boilers could make. This occurred at the exhibition of the American Institute in 1871. Five boilers were tested, and in each case, the test continuing a half-day, all the steam that came from the boilers was turned into the surface condenser. My idea was mainly to ascertain by a method that

might be rough, but which should be in some degree satisfactory, whether we could rely on the older reports of the efficiency of boilers, and whether it would be safe to go on in the old way, and make tests without reference to the amount of moisture contained in the steam. The result was to show that with proper management, the percentage of priming could be kept down within five per cent., and that the old results were often fairly satisfactory. We, however, found it perfectly possible, by carrying the water where we chose, to reduce the amount of priming and to produce super-heating to almost any extent we might choose. These tests came out very well, and the quantities which were handled were so large that I have no doubt that we obtained accurate determinations of the performance of those boilers. But it would, of course, be impracticable to adopt that method in ordinary boiler trials; and the next question that arose was whether we could not find a way of determining the extent of priming by some simple apparatus. The first attempts were made by what has been known since as the barrel, or tank, calorimeter. As devised by M. Hirn, it consisted simply of a tank which might hold one, two, or three hundred pounds, as we might choose. Steam was turned into this through a hose. The amount of steam going into it being weighed, the change of temperature produced being determined with as great accuracy as possible, we could calculate the amount of priming or of super-heating. M. Hirn made his tank a simple barrel, put into it a stirrer so that the water could be efficiently stirred, and introduced a pipe leading down the side of the barrel, pierced with holes throughout its length, through which the steam could be distributed. That simple apparatus, I think, was used by Mr. Emery at about the same date, and a little later at the Centennial Exhibition of 1876, and it has been used from that time to this by a great many engineers, and remains in use to-day. I have sometimes been able to get results from it that were fairly satisfactory. At other times, in spite of every precaution, the results were absurd. In order to get satisfactorily accurate results with that piece of apparatus, it is necessary to have, in the first place, scales that shall have unusual accuracy. That of Mr. A. H. Emery and the new scales and balances that are coming into the market now will give all the accuracy that can possibly be desired. I have no doubt also that the torsion balance apparatus will perhaps give thoroughly satisfactory results. I have even found that, where properly made, the knife-edge balance would give results perfectly satisfactory in all ordinary commercial

work, and possibly for scientific work. The Fairbanks Company have made me scales that have done very accurate work. Work can be done with this calorimeter, therefore, that shall be satisfactory, so far as the weighing is concerned. The next difficulty is in getting thermometers which will be accurate at the start and remain accurate. These thermometers must be made with extreme care. The graduation must be very carefully made, and it must be possible to read at least to tenths of degrees. With accurate weighing, with accurate measurement of temperature, and with extreme care in securing a thorough intermixture of the entering steam with the condensing water, results can be obtained with a barrel calorimeter which will be satisfactory for almost all ordinary work. In most cases, however, the barrel calorimeter has proved unsatisfactory.

I only call to mind one engineer among all with whom I have conversed, and who is considered an authority on the subject, who is willing to trust to it. With the conditions all favorable, I should not, myself, object to using it, and have used it a great deal, but it has been found that the form of calorimeter which is best represented, perhaps, by Mr. Hoadley's device, is more reliable for ordinary work. That consists simply of a tank within which is a closed coil, usually of copper, thus forming a surface condenser. The steam issuing from the boiler passes into the coil, and is then drawn off and weighed separately. There are some sources of error which will, at times, cause difficulty in making accurate determinations; but, on the whole, I think, the results obtained by that instrument are far better than those obtained by the use of the simple barrel-calorimeter. Mr. Hoadley has shown us the highest refinements in the construction of the second kind of calorimeter, constructing a tank of which the thermal content shall be shown as accurately as possible, and so arranged and adjusted that all the measurements may be made with great facility and accuracy. I should say, then, that the Hoadley calorimeter might be taken as the best representative of its class. The system just brought before us was initiated twenty years ago, by Mr. John D. Van Buren, who proposed to use a continuous calorimeter, in which the flow of water should be determined by a standard orifice, and the condition of the steam by the form of a calorimeter to which I now refer. That form was used by a committee of which I had the honor to be chairman about 1873, and it gave very good results. That calorimeter was built at Mr. Van Buren's suggestion, by Mr. Skeel, a very prom-

ising young engineer who has since died, and the standardizing was very skillfully performed; the results obtained were very satisfactory. Several forms of continuous calorimeter were introduced later, one after another, all embodying his principle of a continuous flow of both fluids through the instrument. The most satisfactory of these continuous instruments of which I have known is the first form of calorimeter introduced by Mr. Barrus, in which there is a constant flow of steam and water, both being kept apart, surface condensation being adopted, and some very fine work had been done by it; but there still remained some of the difficulties met with in other calorimeters, and the necessity of having exceedingly accurate measuring apparatus, and extremely fine thermometers. The device just presented here by the same inventor strikes me as being a very promising attempt at meeting what is the vital difficulty in all these cases. The fact that he gets a range of 15 or 20 degrees for the variation of 1 per cent. in the amount of moisture in the steam, is of itself a most interesting fact, and I have no doubt that other difficulties will be met and overcome, and that we will utimately obtain something of this form that shall be probably more satisfactory and reliable than anything we have at present.

Prof. Rogers.—I desire to call attention to two difficulties in investigations of this kind, both of which can, however, be removed. First, with regard to the difficulty to which Mr. Babcock has referred, viz., that of obtaining circular apertures of known and uniformly equal diameters for every point measured, it may be said that this part of the problem can be readily solved by the method of local correction under the microscope. I have recently investigated the diameters of orifices in plates of brass and of iron, which were used in experiments at Holyoke. In every instance a different value was found for the diameter from that hitherto assumed, while the diameters at different points of measurement were found to be considerably at variance.

With regard to the necessity of an exact knowledge of the temperature, I should say that no special difficulty would be encountered if properly constructed metal thermometers were employed. I beg leave in this connection to refer to some experiments in which I am now engaged. In order to obtain a form of thermometer by which any change of temperature can be instantly detected, I fastened a strip of steel and a strip of aluminium side by side to a fixed point of support at one end of the comparator, mounted in the new equal-temperature room beneath the rotunda of the observatory of

Harvard College. These strips, which are one inch wide and 16 one-thousandths of an inch thick, pass over rollers attached to the other end of the comparator, and are fastened to the walls of the room, a little below the plane of the two strips. Two spring scales give the required tension to the strips. At a temperature of about 13° centigrade, a line was drawn across the face of the strips. It is obvious that the measurement of the deviation of these lines under any change of temperature which may occur, will be proportional to the temperature if the lines always come into coincidence at 13°. A sufficient number of observations have been made to show that this zero is constant, and that this form of thermometer is much more sensitive than the mercurial thermometer. Twenty divisions of the micrometer of the microscope correspond to a change of one degree in temperature. Since the separate pointings can always be made within one division, it would appear that the readings are reliable to twentieths of a degree.

In the experiments under consideration it would be better to employ a single strip of platinum suspended vertically. By measuring the difference in length for immersions in melting ice and in steam, two points of reference would be obtained which are independent of the readings of any thermometer. Any differential variations in length, for higher or lower temperatures, could then be expressed in terms of the unit thus obtained. This method of obtaining the temperature would have the decided advantage of allowing the strip of metal to be wholly immersed in the steam of which the temperature is desired.

Prof. Lanza.—The thermometers which we use in our calorimetric work are graduated to millimeters, and we have calibrated them ourselves; they are read to one-tenth of a millimeter, which corresponds to from 0.01° to 0.02° centigrade, and we think that this affords sufficient accuracy.

In regard to the proposed form of Barrus calorimeter, the objection has been made that it is almost impossible to make two holes of exactly the same size, and it has been claimed that the presence of a small speck of dirt would alter the size sufficiently to vitiate the result.

Without expressing any opinion as to the validity of such a claim, I will merely say that, if such a claim is valid, we may as well discard the calorimeter at once, as the difference of temperature that must necessarily occur in its use, would make more difference in the size of the holes than any speck of dirt.

CXCV.

IMPROVEMENT IN FERRY-BOATS.

BY WILLIAM COWLES, NEW YORK.

WHEREVER men are separated by unbridged water, there is generally a ferry of some sort. Between the grand *Etruria* plowing her great circle course and the small dug-out pushed by one man and a pole, there is simply the engineering question of demand and supply.

Where the demand of traffic is great, it would seem that engineering should supply an adequate transfer and keep it in the highest state of efficiency.

It can be made apparent that around New York, which is the greatest ferrying center in the world, this is not done, and it is the object of this paper to discuss possible improvements that may bring our ferry-boats up to the modern standard in efficiency and economy. To make an effective comparison for the basis of this discussion, a table of data has been prepared, together with small scale outline designs of four boats, as follows:

1*st.* The largest, newest, and in every way the best wooden paddle-boat of the purely conservative type that we have in the vicinity of New York, viz., the *Southfield* of the Staten Island Rapid Transit Co. The measurements and data of performance were taken by the writer recently, during several visits of inspection while the boat was running on her regular route and service.

2*d.* A wooden paddle-boat of same team and passenger capacity as No. 1, but with a compound beam engine, similar to that on the Sound steamer *Fall River*, also high pressure boilers, independent air and circulating pumps, and a surface condenser.

3*d.* A steel paddle-boat, in all other respects the same as No. 2.

4*th.* A steel boat with one screw propeller at each end, both screws on one continuous line of shaft, connected by flexible couplings and driven by a compound, inverted, direct-acting engine with independent pumps, steam reverse gear, and a "high press-

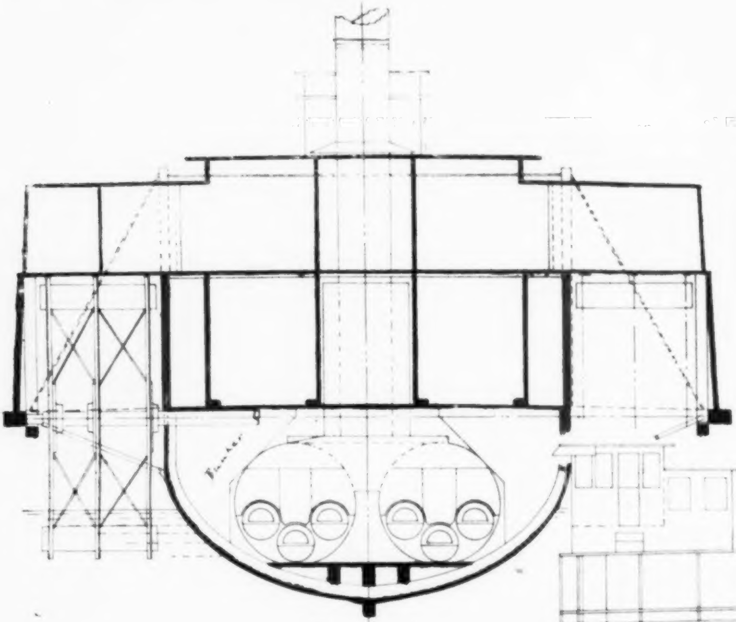
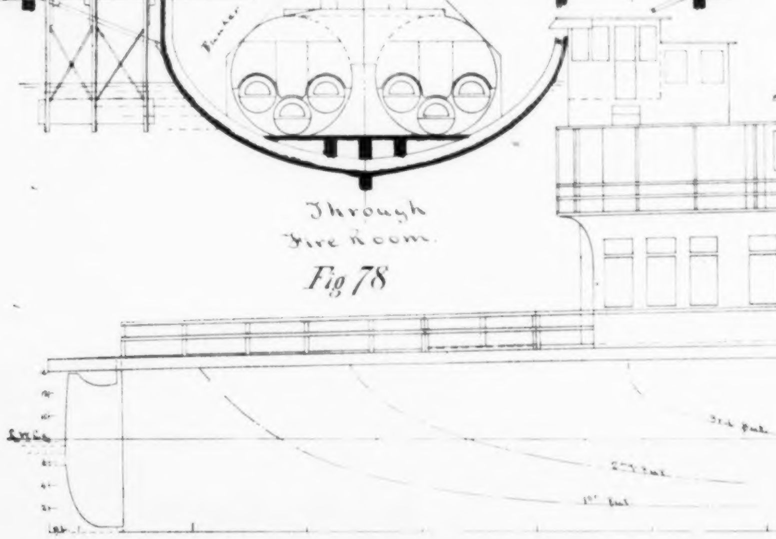


Fig 78



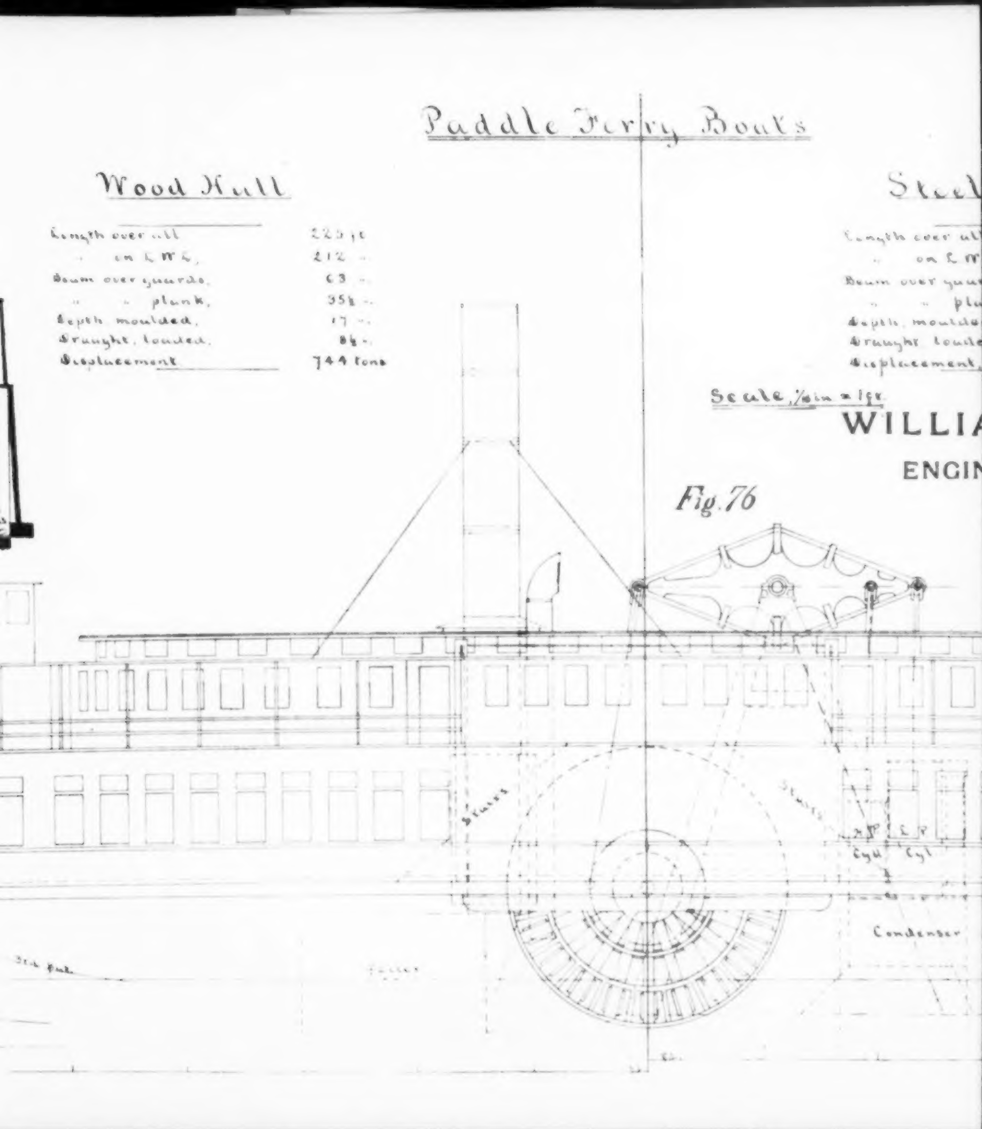
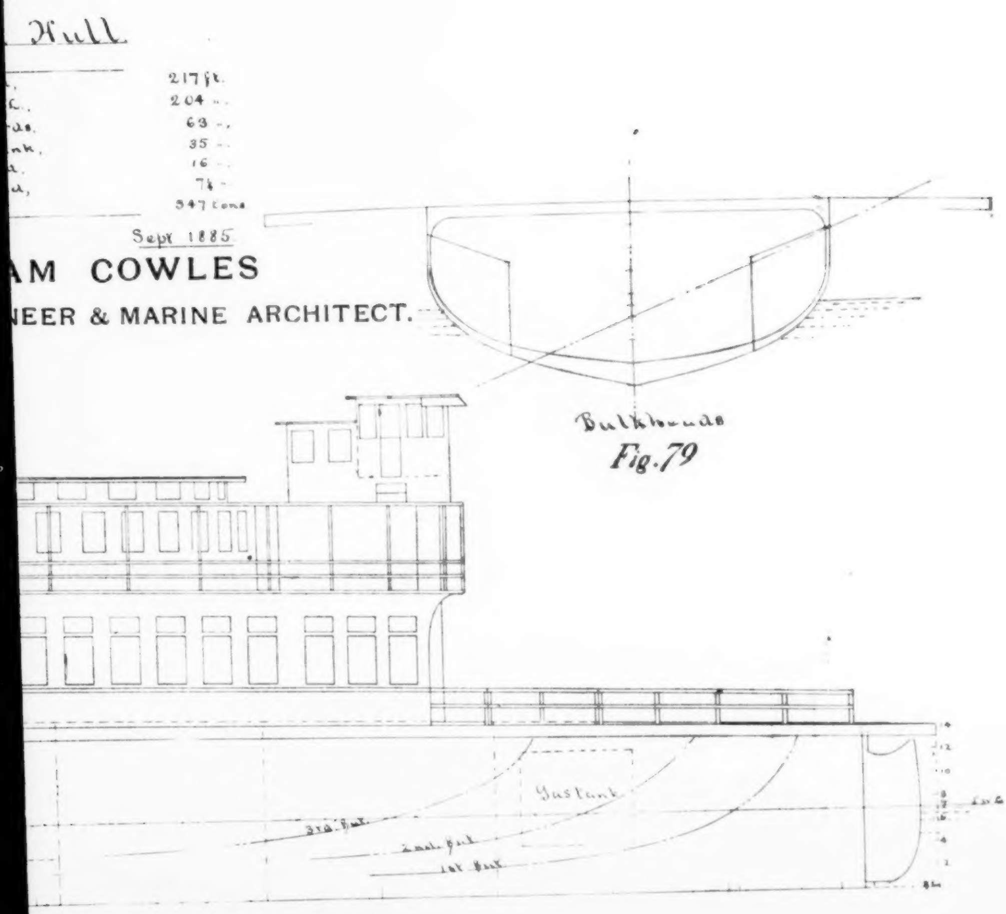
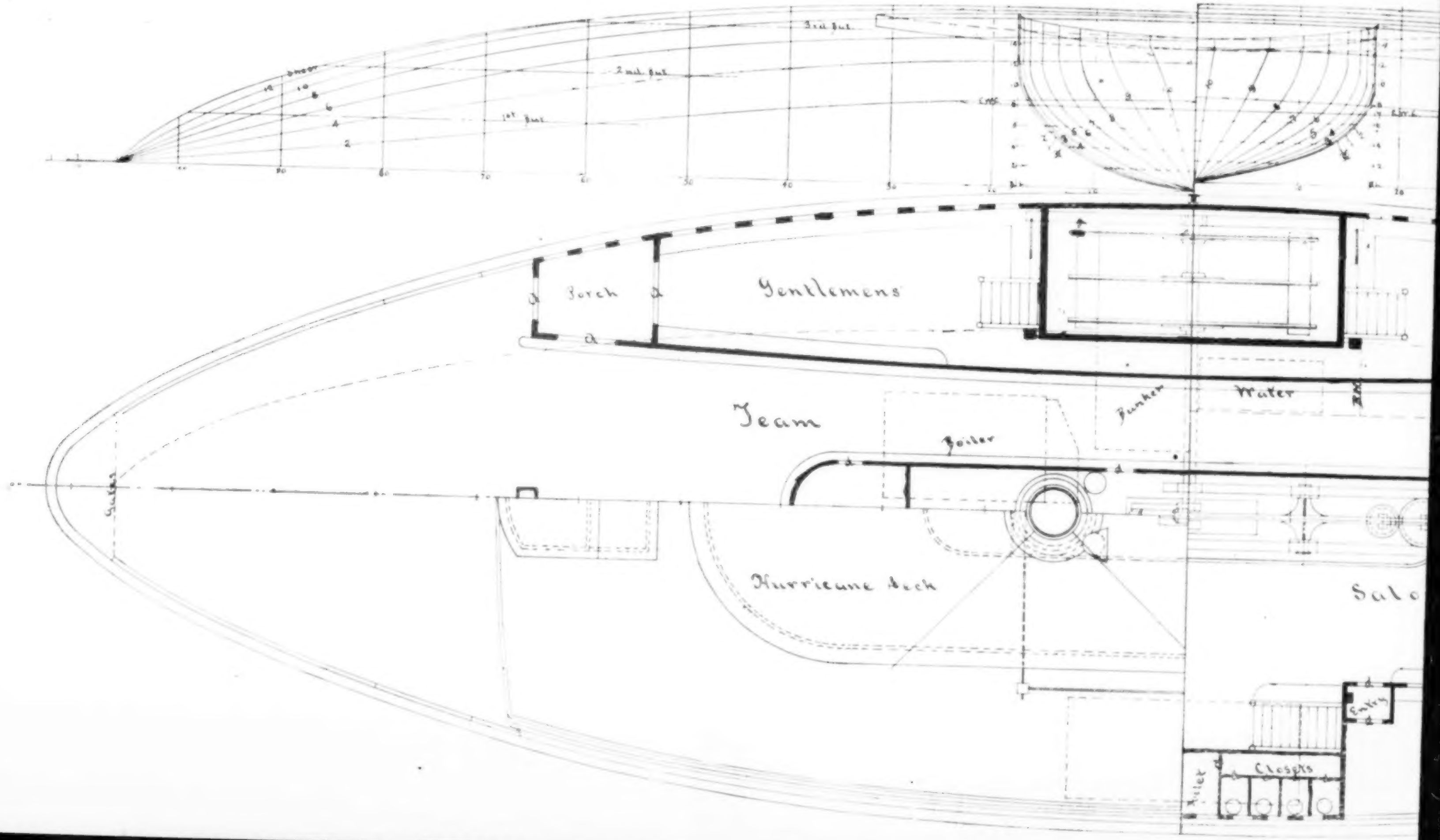
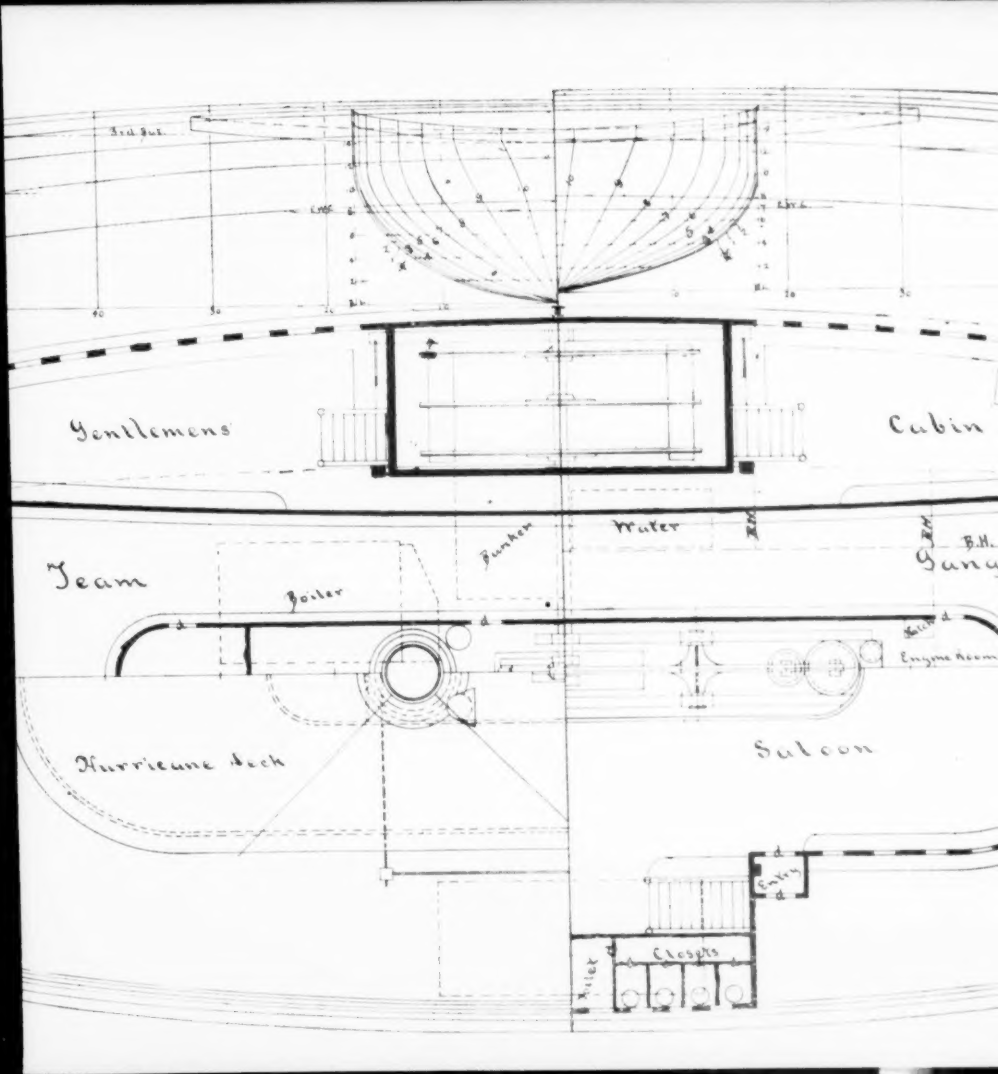

Paddle Ferry Boats
Wood Hull
Length over all 225 ft
" on L.W.L. 212 "
Beam over guards 63 "
" " plank 35½ "
Depth moulded 17 "
Draught, loaded 8½ "
Displacement 744 tons
Steel
Scale, ⅛ in = 1 ft
WILLIA
ENGIN
Stairs
Stairs
Condenser

Fig. 76



Bulkheads
Fig. 79


Porch
Gentlemens
Team
Boiler
Bunker
Water
Hurricane deck
Salo
Pantry
Closets
Water



Gentlemens
Cabin
Water
Team
Boiler
Engine Room
Hurricane deck
Saloon
Closets

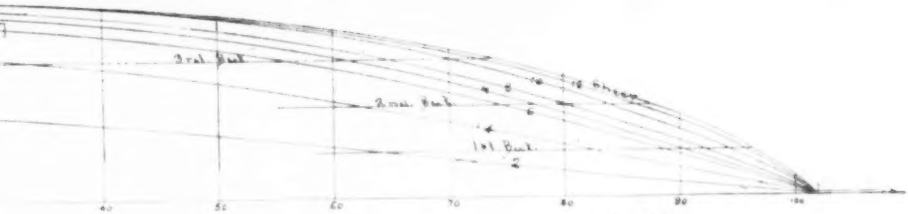


Fig. 80

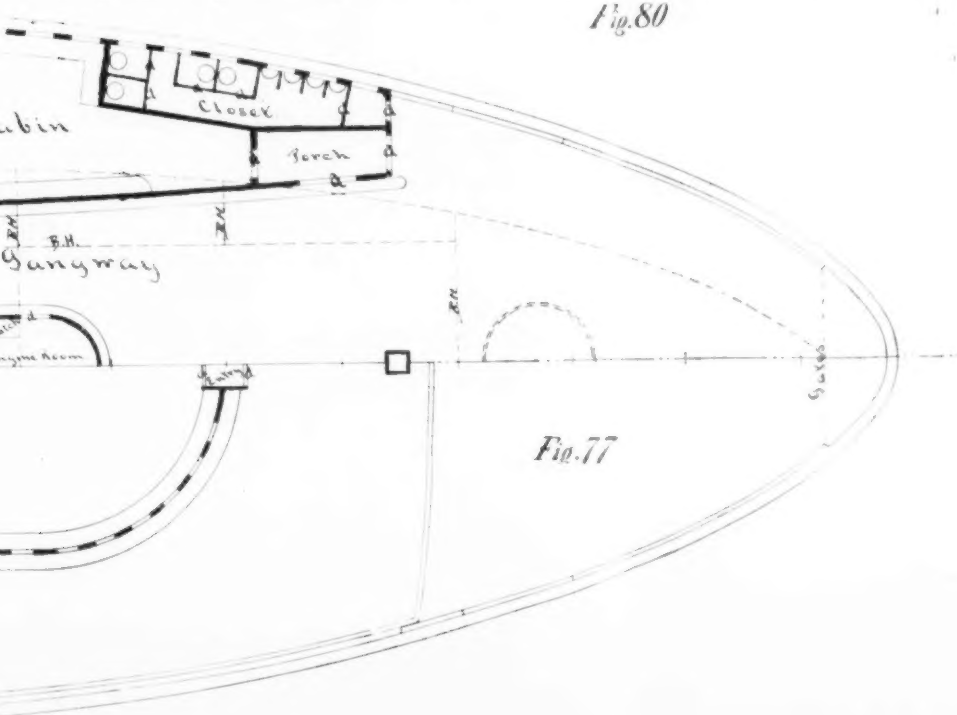


Fig. 77

ure" surface condenser. Boilers of the same type as in Nos. 2 and 3. This boat, although taken as of the same team and passenger capacity as the others, is really much larger, owing to the larger amount of deck space available and its improved shape and arrangement.

The left-hand half of the Figs. 76, 77, and 80 show the two wooden paddle-boats in elevation and plan, and the right-hand halves show the steel paddle-boat. Fig. 78 is a section through the fire-room, Fig. 79 through the bulk-heads, and Fig. 80 shows the lines of the two models. Figs. 81 and 82 show the steel screw boat in elevation and plan; Figs. 83 and 84 are sections through the fire-room and engine-room respectively, and Fig. 85 shows the lines of the model.

The arrangement of bulk-heads in the steel boat is that used in the best-designed boats now running in New York waters. The particular performance of 13 knots, or 15⅜ miles per hour, is taken in Nos. 2, 3 and 4 of the table, because that is the speed required to make 20-minute trips between the foot of Whitehall Street and the new slips at the northernmost point of Staten Island. No. 1 now runs nearly on this route, making the same distance in 25 to 27 minutes.

The conditions taken as the basis of discussion are those existing in New York. For other places it can be said that any boat should be designed only after studying the conditions under which she is to work.

The estimates and designs are approximate, of course, and constitute a preliminary study of the subject; the attempt has only been made to go far enough into details to put the matter in shape to talk at. The table in itself discusses the matter of steel hulls and compound engines for our paddle ferry-boats, and shows their advantages so plainly that it is considered unnecessary to put in a special plea in their behalf, especially when presenting the matter to engineers.

Iron and steel hulls are already adopted generally, and the compounding of the beam engines would be at the best but half a step. Except for the change from wood hulls to steel hulls, our ferry boats remain to-day practically the same as they were thirty years ago; and while the science and the practice of marine engineering has been improving almost every floating thing, these unwieldy hump-backed monstrosities remain invulnerable.

It would seem that something ought to be done in the case of

new boats that would put them on a level with other modern steamers. With this object in view your attention is called to the fourth boat in the table and to Figs. 81 to 85, in order to discuss the merits of the *Screw Ferry-Boat :*

Speed, Power and Action.—Given a certain load to be carried at a certain speed, the question of economy in carrying that load on a ferry-boat is much the same as on a railway train. It depends largely upon the ratio of paying to non-paying load. This non-paying load, *i. e.*, the resistance at the light displacement, in a steel double-screw ferry-boat is a minimum. It is about 11 per cent. more in a steel paddle boat, and about 24 per cent. more in a wood paddle-boat, all being of the same capacity and each with the best types of boiler and compound engine possible. This decrease in non-paying load allows the paying load to be transported with much less expenditure of power, coal, and money, consequent upon the finer model and less "immersed surface," in this feature alone making a pure saving of from 1 *to* 24 *per cent.* as above. Again the screw, as a propelling instrument, is much more efficient than the radial paddle with the same model, and the fining of the model allowed by using the screws, renders the screws themselves still more efficient, adding to the above economy. The compound direct-acting screw engine is much more economical in the use of steam than the compound beam paddle engine, because of higher piston speed and less condensation in cylinders ; also because of less "clearance" in cylinders, allowed by fewer connections, again adding to the above economy.

The screw in the bow of a boat has been proved by use on ferry boats and tugs to be more efficient in propelling than a screw at the stern. (This will be referred to again later on.)

This is a fact due, principally, I believe, to two causes :—the more solid water for the bow screw and the *pushing back* of the stern wave by the stern screw. This increased efficiency of the bow screw adds still further to the above economy, and this difference in economy of screws over paddles will increase as the speed is increased. The boat with screws will handle exactly the same and have the same speed when going in either direction, which is not the case generally with paddle boats because of the paddles not being exactly at the mid-length of boat. When a paddle ferry-boat is driven at even a moderate speed she "buries" her bow, making a difference in trim which sometimes at full speed amounts to more than a foot ; this causes a reduction in the speed,

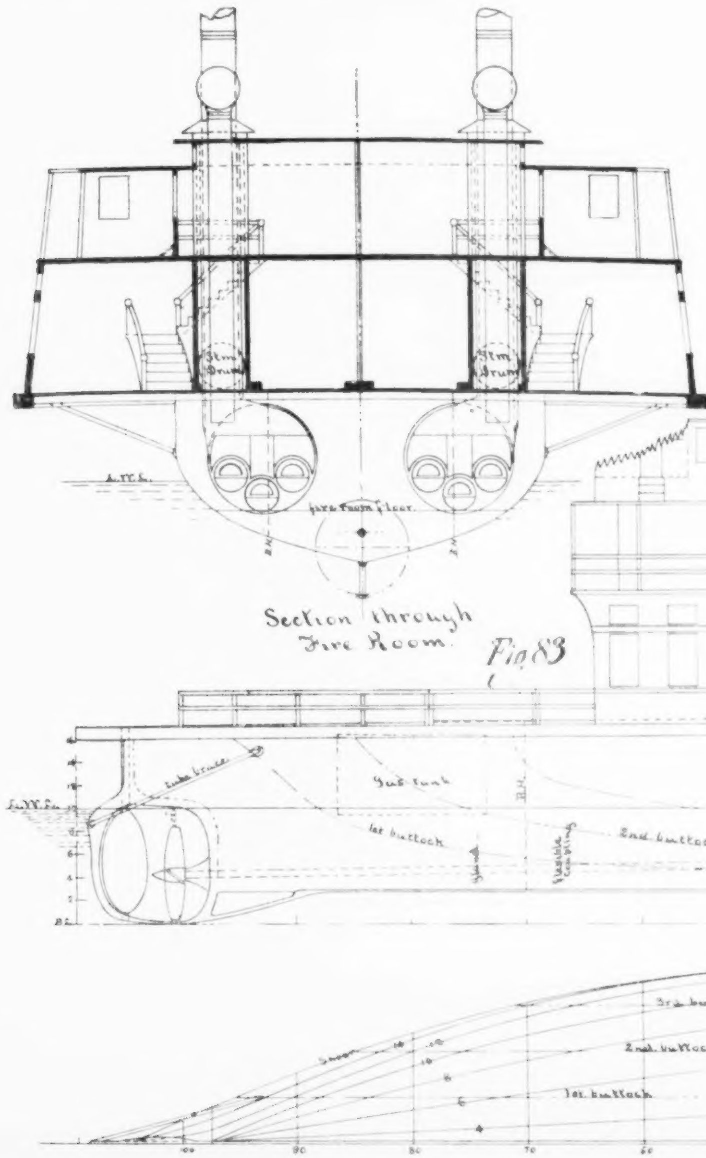
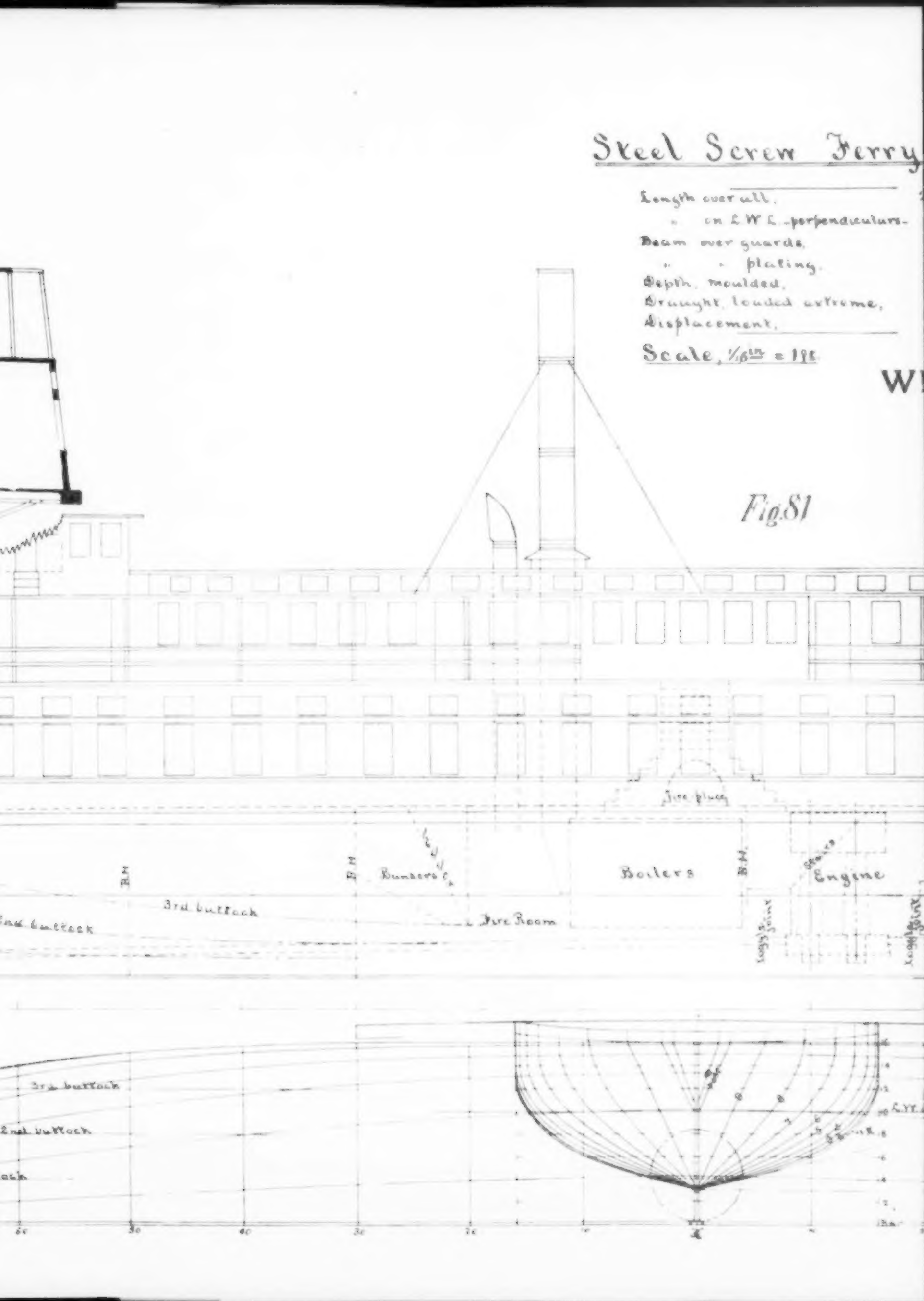

Stm Drum
Stm Drum
L.W.L.
Fire room floor
Section through Fire Room
Gas tank
1st buttock
2nd buttock
L.W.L.
B.L.
Sheer
2nd buttock
1st buttock

Fig. 83

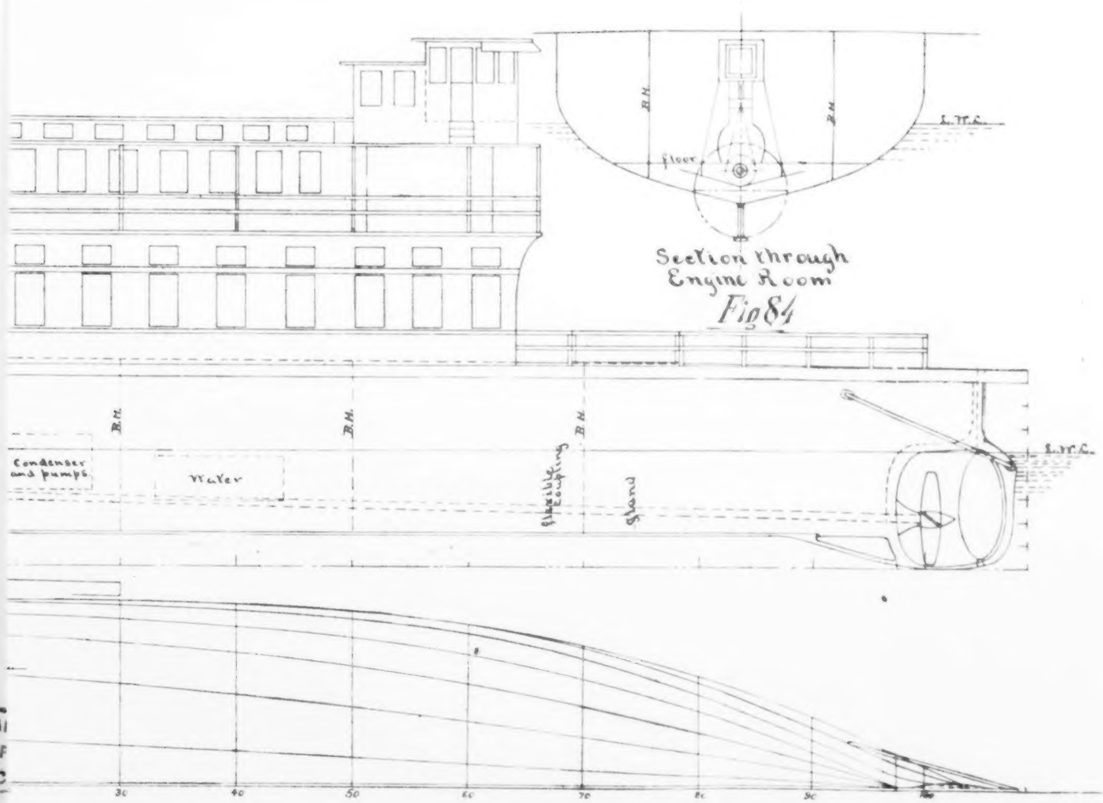

Steel Screw Ferry
Length over all,
" on L.W.L. perpendiculars.
Beam over guards,
" " plating.
Depth, moulded,
Draught, loaded extreme,
Displacement,
Scale, 1/16 in = 1 ft.
WI
Fig. 81
Fire place
Bunkers
Boilers
Engine
Fire Room
3rd buttock
2nd buttock
B.H.
L.W.L.

Boat.

217 ft
105 "
60 "
32 "
15 "
10 "
482 tons.

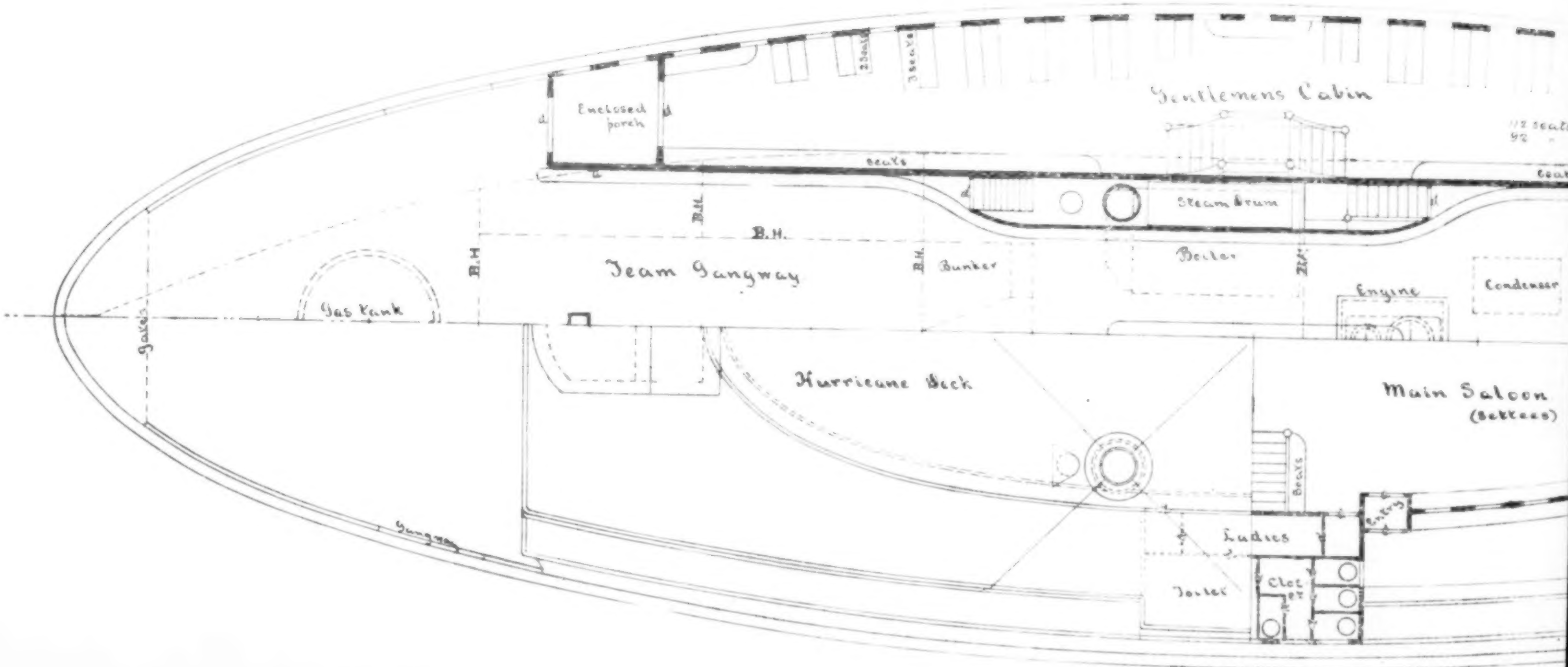
Sept. 1885.

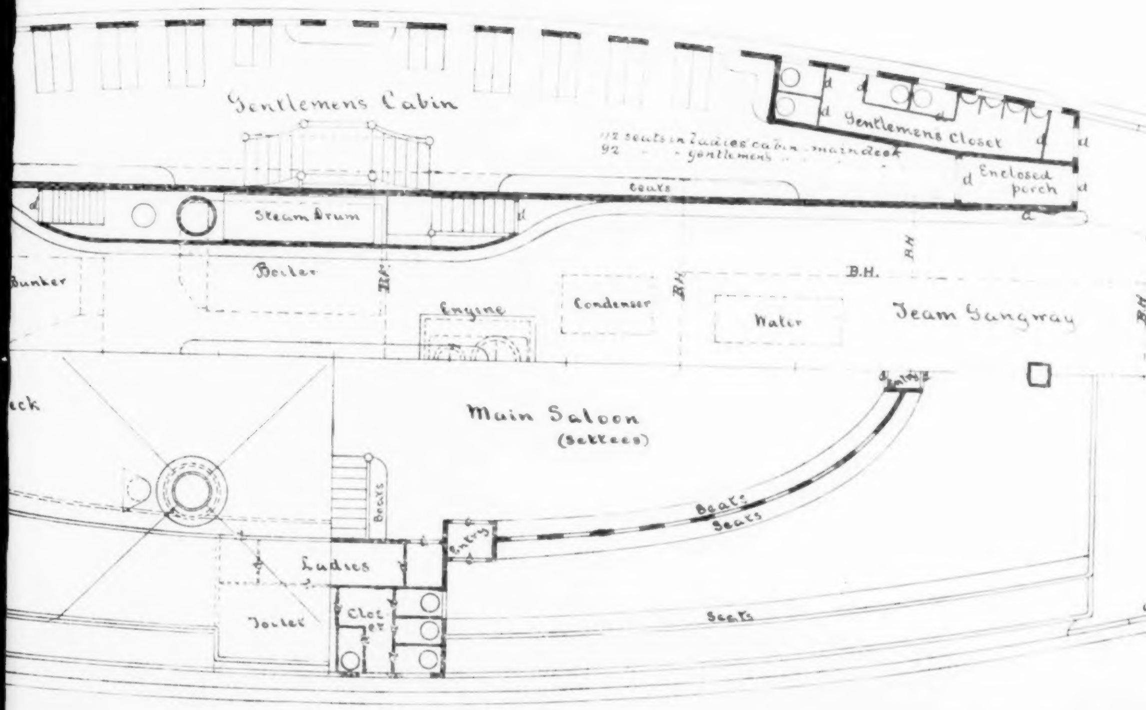
ILLIAM COWLES.
ENGINEER & MARINE ARCHITECT.



Section through Engine Room
Fig 84

Fig. 85


Enclosed porch
Gentlemens Cabin
seats
Steam Drum
B.H.
Team Gangway
Bunker
Boiler
Engine
Condenser
Gas tank
Gates
Hurricane Deck
Main Saloon
(Settees)
Ladies
Entry
Porter
Gangway



Gentlemens Cabin
112 seats in Ladies cabin main deck
92 " " gentlemens " "
Gentlemens Closet
Enclosed porch
Seats
Steam Drum
Bunker
Boiler
B.H.
Engine
Condenser
Water
Team Gangway
Main Saloon
(Settees)
Seats
Entry
Ladies
Toilet
Closet

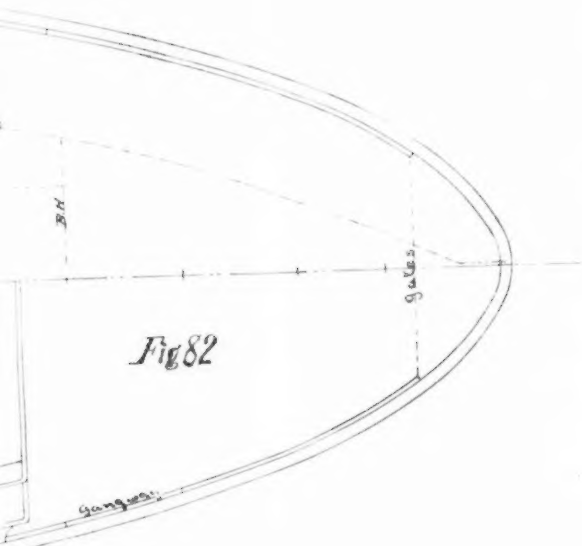


Fig 82

and cannot be entirely avoided because the thrust of the paddles is communicated to the hull at the level of the shaft center, some 12 to 16 feet above the center of resistance, thus causing an enormous up-ending moment. In a screw ferry-boat this cannot happen as the centers of thrust and resistance are very nearly on the same level.

Maneuvering Power.—From the point of view of safety from collision this is the primary feature of a ferry-boat. It is secondary to speed only when efficiency and time are considered. I have the best authority for stating that during the four years, alone, between 1879 and 1883, the stoppages and delays on the P. R. R. ferries between New York and Jersey City *increased* 50 *per cent.* on account of the growth in river and harbor traffic, without any increase in the maneuvering power of the boats. This trouble is increasing yearly. When a ferry-boat can maintain her regular speed in transit and *avoid* other craft instead of having to stop and wait, a very material decrease in the average time between slips will result, to say nothing of the very important decrease in danger of collision from this power of avoiding without stopping; for it is vastly easier to avoid a vessel by turning at speed (when you have the power to do so) than by stopping. And this power by decreasing the time of trips increases the earning capacity of a steamer.

This power in paddle-steamers is limited to the action of the rudder alone, and consequently the vessel is helpless to turn when steerage way is lost. In a screw-propelled vessel the rudder, besides having all the power of that in a paddle steamer, lies directly in the propelling current, and is capable of *deflecting that current* and so greatly increasing the turning effect upon the vessel. A balanced rudder increases this effect in a screw steamer, and with a screw and balanced rudder at each end the effect is practically doubled. There is no satisfactory arrangement of paddles and rudders by which this turning effect by means of deflecting the propelling current can be obtained. High maneuvering power increases the speed of transit, because that speed is the time from slip to slip, not the rate at which a straight away mile can be made through the water; and this power in its highest degree can be had only with screws and balanced rudders. Again, a compound propeller engine with steam reverse-gear can be started or reversed at *full power* almost instantly, whereas the paddle-engine even in its best form for ferry-boats is very slow,

and can be started or reversed at only about *one-quarter of full power* or even less, because of the small lift given to the valves by the toes on the starting rock-shaft. There is a vast difference in the two engines when used to check headway and avoid collisions.

If we adopt the idea of screws for ferry-boats it brings into play the many devices connected with them to increase the maneuvering power; for instance, the *Kunstädter system* could undoubtedly be applied with most satisfactory results.

Economy of Construction is in favor of the screw ferry-boat on account of the much smaller hull, less expensive engine and boilers and less joiner-work.

Economy of Operation points in the same direction, because of much less coal and stores to perform the same duty.

Economy of Maintenance again points to the screw-boat because of more freedom from damage in ice and less expensive repairs on machinery.

Arrangement of Cabins and Team Gangways is immensely better in the screw-boat. The midship house can be entirely done away with, thus allowing the team gangways to be brought together nearer the center of the boat as they should be, and also increasing the space for passengers in main saloon. The large paddle-boxes being cut out of the main deck cabins practically doubles their capacity, and far more than doubles their comfort and architectural effect.

The arrangements and fittings work in better in every way. A net saving of deck space results in cabins and gangways amounting to 1,822 square feet. This deck space in a ferry-boat is all important, because on it entirely depends the capacity, convenience, and comfort of the boat.

The Question of Ice in slips and on passages is always a serious matter with ferry-boats in New York. The present paddle-boats jam the ice up into the slips and against the pontoons until it becomes a solid mass; the wheels being 100 feet away from the bridge and on the surface are unable to create a current which will clear the ice out. The screw-boat, on the contrary, having one of its screws close up to the pontoon when in the slip, will sweep the slip clear at every trip. Some of the ferry companies when much bothered with ice back a tug into the ferry-slips, getting its stern up to the bridge, and then backing hard on the screw, thus sweeping out the ice in the same way a screw ferry-boat would naturally do.

Again, while making passages paddles are continually working directly on and in the thing to be avoided, thus causing a very expensive wear and tear and sometimes total disablement. In freezing weather the paddles become nearly solid cylinders of ice revolving in and rubbing hard against the surrounding cylinders of ice in the paddle-boxes. This causes an immense loss of power and efficiency. The transfer steamer *Wm. T. Hart* at Newburgh uses 25 per cent. more coal in freezing weather on this account alone.

The screws in the proposed boat have the tips of blades immersed 12 inches. The blades are to be of cast steel and removable. The form of cut-water and the tube braces tend to plow up the ice and throw it away from the screw. Tugs working with the screw-tips awash or projecting above the water often lose blades during two or three weeks of heavy ice each winter, but in propellers with the screws well immersed this seldom happens.

Steamers running to the arctic regions are invariably propellers. In fact, it is hard to imagine how a paddle steamer could work in such a locality. In the ports of the North Sea and in some cases on our great lakes, screw steamers are used as ice breakers, running screw first into the ice and cutting a passage through. In these cases, cast-steel blades are found to stand the work.

The Silting up of Slips is also a serious matter in the vicinity of New York. Periodic dredgings are necessary to maintain the depth at and under pontoons; paddles cannot form a current to prevent this, because on the surface and too far away. The screw-boat in going in and out of the slip would keep the silt washed out and maintain the proper depth without dredging. In cases where the slips have not already sufficient water for the shoe of the proposed screw-boat, the boat itself will cut and wash out a place for itself in half an hour provided the bottom is mud or silt.

Precedents of screw ferry-boats and tugs with screws at both ends are not wanting.

At Detroit and Port Huron there are two or more *transfer steamers* with twin screws (at one end) which work all the year round in much heavier ice than any we have in New York. These steamers are over 200 feet long, I believe, and transfer cars and passengers across the Detroit River.

At San Juan, Porto Rico, there is a small transfer steamer with a screw *in each end* used in running across the harbor. This

boat was built in England and sent out under her own steam, I think.

On the Mersey, between Liverpool and Birkenhead, there are now five large ferry-boats *with two screws in each end;* on the Clyde there are a number of similar boats. A description of the Mersey boats in *Engineering*, about August, 1882, says in substance as follows:

The traffic had been carried on by paddle-steamers for years. In August, 1879, the first screw-boat was placed on the line and, was so satisfactory that before August, 1882, she had been triplicated.

The navigation across the Mersey is at times very difficult, owing to the number of vessels at anchor, the heaviness of the traffic and the occurrence of fogs, and hence handiness of maneuvering is a property of great value in a ferry-steamer. This property the screw-boats possess in a very high degree, the double twin screws driven by two pairs of independent engines enabling them to be taken to and from their berths with great facility and to be at all times very promptly handled. When the paddles were in use the tariffs were high, but when the first screw-boat was placed on the service they were reduced one half, and they have since been further reduced. These reductions in the tariff, combined with more frequent service, and the great facilities for prompt loading and unloading, have resulted in an enormous increase in the tariff receipts, and there is every reason to believe that these receipts will increase still further.

The saving of fuel effected by the first screw-boat as compared with the paddle-boats was very important, the paddle-boats averaging 10^1 cwt. of coal per hour, while the average consumption of the screw-boat was but $3\frac{3}{4}$ cwt. per hour, the speed of the latter being at the same time higher.

The boats are 140 feet long by 45 feet beam over plating, by 13 feet depth of hold with decks 6 feet above water. Each boat has two independent, vertical, direct-acting compound engines; each engine with its shaft passing the whole length of boat and a screw at each end, four screws in all on each boat. It is believed that the bow screws, working as they do in undisturbed water, do the major part of the work. The latest boats built have steam steering gear, which in connection with their screws gives them exceptional handiness. The boats can each carry from 1,700 to 2,000 passengers.

The company ascertained after working her eight months that the third one of these screw-boats consumes 3 tons 11 cwt. of coal per day of twelve hours, whereas the newest of their paddle-boats burns 4 tons 14 cwt. for the same number of hours work.

This being so, the screw-boat appears capable of carrying her full complement of passengers upon 45 lbs. of coal per 100 passengers, continuously, whilst the paddle-boat, under the same condition, consumes 109 lbs. of coal per 100 passengers. Altogether the economical working of the screw-boat, combined with the greater safety from collision due to her great handiness, the absence of paddle-boxes, and the adoption of a form of hull capable of subdivision into a large number of water-tight compartments, renders the vessel entitled to be regarded as a decided advance in the accommodation of river passenger traffic.

These boats were built by Wm. Allsup & Sons, Preston, England.

James Howden & Co., marine engineers and contractors for steamships, Glasgow, Scotland, inform me under date of August 2, 1883, that they have built a number of steamers with screws at each end, principally tugs, but some for ferry purposes. The engines in each case had two cranks, a continuous shaft passing through the vessel and a screw on each end. They say: "As regards the efficiency of the propellers at each end, we found that we could apply a greater engine power to a shallow-draft steamer than we could possibly do with a single screw." * * * "The bow screw we found is rather superior to the after screw in regard to towing power. On a tug boat which was the same on both ends, we had the rudders, which were also at each end connected to the one steering barrel, and this gave a much quicker turning power than a single rudder would have given. The rudders were also perfectly balanced and very easily worked."

In conclusion, it would seem that there is everything in favor of using screws in ferry-boats, and nothing against it except the natural prejudice arising from lack of knowledge in this country of what has been done with screw-boats in other parts of the world. The building of a screw ferry-boat cannot be called "an experiment" in any sense of the word, in view of what has been done. It would only be a new thing in New York harbor, where all such matters are far behind the times.

Since preparing this paper, I have learned that Mr. Frank Stevens, of Hoboken, proposed, some twelve or fourteen years

ago, to the Hoboken Land and Improvement Co., a ferry-boat with screws at each end, which was to be of iron. It is said that the superintendent of the company at that time would not even listen to the proposed iron hull, much less to the idea of screws.

COMPARATIVE TABLE OF FERRY-BOATS.

	1st.	2d.	3d.	4th.
Description of Boat.	S. I. R. T. Co.'s. "Southfield."	Wood Paddle-Boat.	Steel Paddle-Boat.	Steel Boat with Double Screws.
Source of Information.	Inspection and Measurements taken on board.	Design Sketch to scale herewith.	Design Sketch to scale herewith.	Design Sketch to scale herewith.
Length over all.	225′—10″	225′—	217′—	217′—
Length on L. W. L.	212′—8″	212′—	204′—	Between perp's. 195′—
Beam over guards.	63′—	63′—	63′—	60′—
Beam over plank or plating.	35′—	35′—6″	35′—	32′—
Beam at L. W. L.	abt. 34′—	33′—	33′—	30′—
Depth molded (*center*) from base line to top of deck (*ends*).	abt. 15′—6″	17′— 15′—	16′— 14′—	15′— Not including scag. 14′—
Load draught.	abt. 8′—3″	8′—6″	7′—3″	Extreme 10′— To keel 7′—8″,
Load displacement.	abt. 800 tons.	744 tons.	547 tons.	482 tons.
Displacement { per inch at L. W. L.	abt. 12½ tons.	12.21 tons.	11.44 tons.	10.4 tons.
Bulk-heads water-tight.	None.	None.	16 See sketch.	13 See sketch.
Area of immersed surface.	abt. 7200 □′	6755.2 □′	6128. □′	5534. □′
Mean angle of water lines.	abt. 10°—0′	8°—54′	8°—0′	8°—0′
Co-efficient of augmentation.		1.0967	1.0777	1.0777
Area of L. W. L. plane.	abt. 5200. □′	5150. □′	4804 □′	4368.5 □′
Area of midship section.	abt. 210. □′	181.3 □′	142.36 □′	130. □′
Material of hull.	*Wood*, diagonal iron straps.	*Wood*, diagonal iron straps.	Steel.	Steel.
Estimated weight of hull, alone.		48% of Disp. 360 tons.	33% of Disp. 181 tons.	33% of Disp. 160 tons.

COMPARATIVE TABLE OF FERRY-BOATS.—*Continued.*

	1st.	2d.	3d.	4th.
DESCRIPTION OF BOAT.	S. I. R. T. Co.'s. "SOUTHFIELD."	WOOD PADDLE-BOAT.	STEEL PADDLE-BOAT.	STEEL BOAT WITH DOUBLE SCREWS.
Source of Information.	Inspection and Measurements taken on board.	Design Sketch to scale herewith.	Design Sketch to scale herewith.	Design Sketch to scale herewith.
Type of engine.	Simple, beam, jet condensing, low pressure.	Compound, beam, surface condensing.	Compound, beam, surface condensing.	Compound, inverted, direct acting, "high pressure" surface condenser. Single shaft.
Diam. and stroke H. P. cylinder.	None.	31″ d. × 6′–8″ str.	29¼″ d. × 6′–8″ str.	22″ d. × 32″ str.
Diam. and stroke L. P. cylinder.	50″ d. × 10′ str.	48″ d. × 10′ str.	46″ d. × 10′ str.	40″ d. × 32″ str.
Floor space occupied by engine, etc., which could be used for teams and passengers.	Midhouse, 684 □′ Cabins and saloon, 1543 Total, 2227 □′	684 □′ 1543 Total, 2227 □′	684 □′ 1543 Total, 2227 □′	Total, 405 □′
Estimated wht. of engine and machinery.	less than 80 tons.	80 tons.	74 tons.	42 tons.
Propelling instruments.	Radial paddle-wheels. 22′–1¼″ d. × 9′–1¼″ face.	Radial paddle-wheels. 23′ d. × 10′ face.	Radial paddle-wheels. 23′ d. × 10′ face.	Two screws on same shaft. 8′ d. × 12′ pitch.
Type of *boilers* and working pressure.	Cylindrical shell, water-leg furnaces return flue, iron—50,000. T. S. 52 lbs. when new in '82. Now 35 lbs.	"*Scotch*," furnace flues and return tubes. Steel. 65,000. T. S. 100 lbs.	"*Scotch*," furnace flues and return tubes. Steel. 65,000, T. S. 100 lbs.	"*Scotch*," furnace flues and return tubes. Steel. 65,000 T. S. 110 lbs.
Heating surface.	abt. 2400 □′	4266. □′	3840. □′	3200. □′
Grate.	91¼ □′	143. □′	120. □′	100. □′
Estimated wht. of boilers—*steam up.*	abt. 75 tons	—35½ lbs. 67 tons.	per sq. ft. heating 60 tons.	surface.— 50.4 tons.
Weights. Hull	....	360 tons.	181 tons.	160 tons.
Carpenter and joiner work	....	65 "	60 "	57 "
Fittings and equipment	....	27 "	27 "	27 "
Engine and machinery	....	80 "	74 "	42 "
Boiler and fittings	....	67 "	60 "	51 "
Load—600 passengers @ 150 lbs. and 24 teams @ 4½ tons	....	145 "	145 "	145 "
Total	....	744 tons.	547 tons.	482 tons.

COMPARATIVE TABLE OF FERRY-BOATS.—*Continued.*

Estimated performance at $15\frac{3}{4}$ *miles per hour, same ratios and allowance for all.*

	1st.	2d.	3d.	4th.
Description of Boat.	S. I. R. T. Co.'s "Southfield."	Wood Paddle-Boat.	Steel Paddle-Boat.	Steel Boat with Double Screws.
Source of Information.	Inspection and Measurements taken on board.	Design Sketch to scale herewith.	Design Sketch to scale herewith.	Design Sketch to scale herewith.
Indicated horse-power for $15\frac{3}{4}$ m. = $13\frac{3}{4}$ kts. I. H. P. $= \frac{v^3 \times \text{aug. surface}}{20{,}000}$	*Actual performance* at 12.5 mil's per hr. timed & plotted, I. H. P., 935. Calculated. No card.	910 I. H. P. (say 920.)	812. I. H. P. (say 820.)	$733\frac{1}{4}$ I. H. P. (say 740.)
Pressure of steam at cylinder.	27 to 30 lbs.	80 lbs.	80 lbs.	90 lbs.
Vacuum.	25 to 26 ins.	20 ins.	20 ins.	None. A "high pressure" con.
Cut-off.	Slightly over $\frac{1}{4}$ stroke.	$\frac{1}{4}$ stroke in each cyl.	$\frac{1}{4}$ stroke in each cyl.	Abt. $\frac{4}{10}$ths stroke in each cyl.
Revolution per min.	23 to $23\frac{1}{4}$	30	30	128
Piston speed per min.	460 to 470	L. P. 600′ H. P. 400′	L. P. 600′ H. P. 400′	$682\frac{2}{3}$′
Slip of paddles or screws in % of speed of cent. of press.	24.52%	30%	30%	$12\frac{1}{4}$%.
Coal per I. H. P. per hr. for all purposes.	Actual 2.9 + lbs.	" Fall River " per 2.4 lbs.	formance, 2.3 lbs. 2.4 lbs.	This can be lowered. 2.25 lbs.
Coal per 12 hrs. work on route.*	(at 1800 lbs. per cart.) measured 10.8 tons.†	174.82 lbs. 8.74 tons.†	15753.6 lbs. 7.88 tons.†	13338. lbs. 6.66 tons.†
Comparative total cost..	85	100	120	108

* Running $\frac{2}{3}$ds. of time. † Tons of 2,000 lbs. each.

DISCUSSION.*

Mr. J. F. Holloway.—The paper presented by Mr. Cowles is one which is of much interest to engineers. It treats of a branch of engineering which it is hoped will in the future receive more attention from members of this Society, namely, the construction and arrangement of hulls and machinery in marine and river steamers.

It is among the anomalies of engineering, as stated by the writer, that while so many and radical changes have been made in almost

* Communications to the Secretary forwarded in writing since the meeting.

every branch of the profession, there should have been few and slight changes made in marine engineering, as it pertains to "steam ferry-boats," especially in the harbor of New York. The ocean marine, which has become only a ferry on a larger scale, as between New York and foreign ports, has undergone vast changes in the thirty years past. What they are, as a whole, is not pertinent to the discussion of this paper, but the change proposed by the author, namely, the substitution of screw-propellers in the place of side wheels, is acknowledged on all hands to have been the all-important change, out of which has grown the wonderful results of speed and economy in the steamships of the present. It may therefore well be asked, why may not a similar change in the substitution of screws for side-wheels, and of upright compound for ordinary beam engines, also be productive of like benefits in ferry-boats of the class described in the paper. It is obvious that if it were not the fact that the same boat must be used during the winter, and amid ice, as is used during the balance of the year, a boat could be planned which would perhaps do better service in either one or the other seasons, than in both.

It has been found on the Lakes, and in the rivers and channels connecting them, that a model best adapted to use, when there is heavy ice, is one that shall not have sharp water lines, but on the contrary shall be full at the bow, with the under lines sloping gradually away under the hull; or, as it is sometimes called, a "spoon bow." The effect of such a boat when propelled as they all are, by screw-propellers, is to rise on top of the ice, and to crush it by the weight of the hull, and at each successive advance a new field of ice is broken and passed under the hull, when it passes back into the channel previously made. While this style of bow has been, as I say, found best when the boat must be used during the winter, it is not a shape out of which the best results of speed could be obtained at other seasons. It is, perhaps, unnecessary to say that a boat of fine water-lines when used in heavy ice, simply wedges itself fast from the bow to its widest midship section in the ice, with no power to penetrate the ice further, or to displace it sidewise, when it is at all heavy, and at such a time side-wheels have little or no effect, either in propelling the hull, or of opening a way into the ice field. It is probable that in the harbor of New York, with its shifting tides, and constantly passing boats, that the ice never attains either the thickness or firmness often found in Detroit River, the Strait of Mackinaw, or in the passage between

Grand Haven and Milwaukee, so that it would not be necessary to go to the extreme fullness in the lines of a boat used there which has been found desirable at the places named. As to the advantage of the screw-propeller when the slips are full of ice, there can be no question, or, indeed, of its advantage in propelling the hull through fields of ice, and it would seem that all question as to the economy of this method of propelling hulls at all times must be considered as settled by the experience on the lakes as well as on the seas, where side-wheel steamers, except on special service, have long since disappeared. The plan of twin screws, or one at each end of the hull, has much to commend it for the service required, and the adoption of the Kunstädter plan of placing a small propeller within the rudder itself, if it could be successfully carried out, would be of immense advantage to the New York ferry-boats, as it would enable them to overcome that most annoying of all delays, namely, the tide drifting, which so often occurs, and which obliges a boat, when quite near the slip, to go half-way back again in order to get a new start and better steerage way.

Mr. Horace See.—The advantages of the screw system of propulsion over the paddle, as employed on the Mersey ferries, where double twin screws, driven by independent compound surface condensing engines, with shafting in a straight line rigidly connected together are used, have been very satisfactorily proved after some six years of continuous service. Increased speed has resulted from the use of the double screw; greater handiness from the twin screw, and superior economy from the high-speed compound condensing engine.

How to modify this system to meet the requirements of navigation, in waters troubled with both large and heavy bodies of ice, is a problem presenting many difficulties not easily surmounted.

The plan proposed, of vessels with one screw at each end, both screws on one continuous line of shaft, and driven by a compound high-pressure condensing engine, sacrifices some of the advantages of the Mersey system, particularly those of handiness and economy, by dispensing with the twin screw and air pump.

It is also open to several objections, prominent among which are the peculiar arrangement of the cutwaters, braces, and rudders; also that of the screws to them, and the use of shafting not in a straight line.

The cutwater and braces intended to deflect the ice or any floating matter from the screws, may perform this work at the bow, but

not at the stern. Here the braces and cutwater together will act as a net to intercept such matter and not only force it into the space between the rudder and cutwater, so as to prevent the one from being moved past the other, but will also project it downward against the blades of the screw, with the great likelihood of breaking them.

In freezing weather, the rudder which is not used for steering, and is fixed, will be liable to freeze fast to the cutwater, so that when the time comes to use it for going in the contrary direction, some difficulty will be experienced in freeing it.

Gearing, which is objectionable on a high-speed propeller-engine, when working under ordinary conditions, is likely to become a positive evil, although in the form of flexible couplings, when the screws are working in ice.

Mr. E. P. Stratton.—While I greatly appreciate the able manner in which Mr. Cowles has dealt with this subject, I wish to call attention to his proposition of making use of the compound engine in connection therewith. Engines of this type have their advantages, especially when run for protracted lengths of time, or over lines covering a distance of several miles; but to adopt the compound engine for use on short ferries, like those from New York, Brooklyn, or even Jersey City, I would question the economy to result from so doing, for the loss in heat in having to warm up a high and low pressure cylinder every time the river is crossed would, I think, give an expensive and unsatisfactory result as compared with that now obtained with comparatively high-pressure condensing engines on such short ferries as those from Fulton, Catherine, Grand, Houston, and Thirty-fourth Streets in New York City. If Mr. Cowles were to place ferry-boats, such as he describes, on a long ferry like those across the harbor of Rio De Janeiro in South America, or some similar distance of six or seven miles, then his compound engines would be a very desirable feature in connection with these boats with a screw at both ends. The quickness of maneuvering in a crowded harbor with such boats is one of great importance, and must soon be considered by the great corporations controlling some of the many ferries on the rivers of the Atlantic seaboard.

Mr. Chas. E. Emery.—This is a valuable paper which should not pass without notice.

There is no question but what the economy in space and operating expenses which has been secured in large sea-going vessels can also be obtained by the application of the screw-propeller, and suitable

machinery to displace paddle-wheels and machinery on ferry-boats. The only questions which will arise, are as to the manner in which a screw should be applied to suit the conditions which obtain in our crowded harbors in these high latitudes. I have frequently thought that in a harbor like that of New York, independent paddle-wheels, or twin screws, should be used on ferry-boats for the purpose of maneuvering. The objection to twin screws seems to be the difficulty with ice. By long experience, paddle-wheels are now constructed which will stand the ice, not without care and considerable repair and renewal, but at least without disastrous failure. The action of paddle-wheels is to strike the ice from the top and drive it down, where there is plenty of room. Fragments only are carried up into the wheel-box, and do not stop the revolution of the wheels. With a screw-propeller, however, there is a shearing action across the stern and rudder posts which makes it possible to catch the ice in a way to fracture the blades. Of course the difficulty is greatly reduced by placing the propeller well under water, but if the bow wheel be arranged between stem and rudder posts in the same manner as is customary at the stern, it would seem impossible to avoid difficulty, for the reason that ice frequently banks up in front of a vessel and portions of it are driven under, exactly where it would be acted upon directly by the propeller. The only way, apparently, in which propellers could be operated in the bow of a vessel safely, would be to throw them out well in advance of the rudders, so that no shearing action could take place. This feature of the problem certainly needs further consideration.

I am sorry that I have not leisure thoroughly to study the plans and the prominent features of comparison brought out in this paper. The subject involves so many details that one would naturally prefer to postpone an elaborate examination until required to decide on a sytem suited for a particular place. Under such conditions the paper would be a valuable one for reference. I desire to thank Mr. Cowles for taking so much trouble in preparing it, and hope the Society will encourage the preparation and publication of papers of this kind.

Prof. R. H. Thurston.—The paper of Mr. Cowles interests me very greatly. He is taking the line of improvement that I have no doubt many naval architects have considered, but which no one has yet worked out as fully as it deserves. It is unquestionably true that the screw vessel greatly excels the side-wheel steamer in every point made in the paper; it is, I think, equally true that these

advantages would seem to be more palpably manifest in the case of the ferry-boat than in any other class of steam vessel. The necessity which is felt in their case of securing good speed, economy of fuel, clear decks, quickness and certainty of maneuvering, and especially of safety, combined with economy of first cost and of maintenance, exists there as probably nowhere else in naval engineering. All these desiderata are attained most satisfactorily, in my opinion, by the adoption of the steel boat with modern screw engines, with the possible exception of the fourth. I am not sure that the screw will excel in maneuvering power, for it is a matter of common experience that the screw has a tendency to sweep the end of the ship at which it is placed in the direction of motion of the lower side of the propelling instrument, and this tendency causes a serious inconvenience at times, especially when moving in a confined space and at low speed, or just starting from rest, or when reversing the motion of the vessel. But, in the design here presented, I imagine that even this difficulty is avoided by the introduction of a screw at the stem as well as at the stern. To what extent the advantages in other respects claimed for the screw in the bow may be anticipated I have no real knowledge, but I have no reason to doubt that a steel screw ferry-boat, such as is here illustrated, with its two screws, its high power, its lightness and quickness of response to the action of its machinery, coming, as may be expected, of its lightness of hull and power of engines, the high motive force and comparative absence of inertia, with its excellent maneuvering power, its clear decks, its moderate cost, both of construction and of operation, its safety from injury by ice in winter, or from the accidents to which the ferry-boat is usually peculiarly liable, will prove vastly superior to the now antiquated side-wheel boats so universally employed; and once introduced, it may be expected, I think, that it will rapidly displace them.

The dredging action of the screws in the slips, when the latter are filled up with drift-ice or with silt, is, to my mind, a very important advantage. To one who has been accustomed to go frequently to or from New York city, crossing either of the two rivers enclosing it, and especially if familiar with the Hudson River crossings, this last is likely to appear one of the most important of all the advantages claimed for the new style of boat.

The balanced rudder, properly proportioned and handled by men accustomed to the work, will be found an improvement on the ordinary form. I have been "shipmates" with these rudders, and

have watched their action on the Hoboken Ferry, where they were introduced by their inventor, Mr. Robert L. Stevens, many years ago, and are still in use; and, although sometimes giving trouble, from their very effectiveness, when in inexperienced hands, they are certainly better steering instruments than the ordinary form. Captain Ericsson adopted them for his ironclads, and they have been in use, in these places and elsewhere, a sufficient length of time to prove their value. It is merely necessary, in designing them, to see that they are not too perfectly balanced; perhaps one-third the area on the forward and two-thirds on the aft side the rudder post is as good a proportion as any. Very possibly the Kunstädter system may sometime come in here.

I notice that it is stated that Mr. Francis B. Stevens proposed this system of ferry-boat construction to the Hoboken Ferry Co. some dozen years ago. I am not certain but that I was myself the proposer of the plan. I, at that time or earlier, sometime about 1871, I should say, proposed the scheme to the late Mr. W. W. Shippen, the president of the company, and was asked by him to look into the matter a little. I was fully convinced of the practicability and of the advantages of the plan, but the discouraging reports received through, if I remember aright, the Vice-president of the N. Y. & N. H. R. R., in regard to the performance of a single screw-boat then used by them at the Connecticut or the Thames River crossing, together with the natural conservatism which is characteristic of all business men, prevented the serious consideration of the matter at the time by the officers of the company, and I have never since seen just the time to take up the matter again. It is a great pleasure to find the data gathered by Mr. Cowles, fully corroborative of my earlier convictions. (In parenthesis, I may remark that I had a similar experience in the matter of the introduction of the compound engine in this class of vessels.) The evidence here presented on the superior efficiency of the bow screw is to me as novel as it is interesting, and I hope that we may be able to see the experiment tried on this side the Atlantic.

One advantage, which seems to me a very great one, is not mentioned here. It is a matter of common experience in the harbor of New York, and, I have no doubt, elsewhere as well, that, in winter, with a strong wind blowing, and especially with ice in the river, and still more seriously if wind and ice and fog or snow combine to make trouble, the side-wheel boat is liable to drift entirely off

her course, and the pilot to become completely lost in a wilderness which is, nevertheless, not a wilderness, for it is full of dangers and threatening surprises. The screw-boat, with its lessened exposure to the wind and its better power of holding its course and of driving through thick ice, is certain to prove, in this respect, and under these most trying of all conditions, by far the safer, surer, and most comfortable boat. I have known the side-wheel boat to be hours adrift, and only then to reach its dock to find that it was the one from which it started; but I doubt if this would occur to such a screw-boat as this of Mr. Cowles once where it might happen twenty times with the other.

Mr. Cowles' paper impresses me as being as important as it is brief, and as creditable to its author as it is important.

Mr. William Cowles.—The value of a paper read before this Society lies often, not so much in the paper itself as in the discussion of it, and it seems to me that this is true in this case. I desire, before going further, to say that I feel amply paid for any time and work I may have put into the foregoing paper, in view of the very able and fair discussion which it has brought out from high and competent sources. The idea of a screw ferry-boat is no "hobby" of mine—an engineer can hardly afford to keep so expensive a thing.

The paper is simply the result of one of those studies and investigations which engineers are constantly making in the course of their profession, and if I wrote in a radical manner and went, in some cases, beyond what would seem the limits of conservative engineering, it was from the courage of my convictions formed after a careful consideration of many details impossible to mention in a "fifteen minute paper."

It is, perhaps, this enforced omission of details and extended explanations which makes my paper obscure on some points, and I am glad of this opportunity to reply to the discussion.

Mr. Holloway, in speaking of the best form of model for heavy work in ice, commends the "spoon bow," and I heartily agree; he admits that such a bow may not be necessary in New York because, in comparison with the Lakes, there is little or no heavy ice there. The proposed screw-boat as set forth in the paper is not one to meet general conditions *everywhere*, as such a thing would be an impossibility, because in a properly designed boat almost every detail is a variable, conforming to the special conditions of route, traffic, etc. The boat proposed is intended to fit

the conditions in New York, and more than this, a *special* route and traffic, viz., that between the foot of Whitehall Street and the proposed new slips at St. George, Staten Island; distance from slip to slip, five statute miles.

Now, in the harbor and vicinity of New York the ice comes almost entirely from the North River. The broken or fine ice presents only slight difficulties, except in the slips, as stated in the paper; it is the floe, or field-ice, which bothers boats on passages and it is a very severe winter in which we have over two or three weeks of large field ice. In coming down the North River from Forty-second Street to the Battery these fields have to cross ten regular ferry routes and about twelve regular car-float routes, to say nothing of the immense and constant river traffic in other bottoms, and, in addition, the foreign and coasting steamships. The result is that the fields become broken ice before reaching the Battery, and there are only a very few days in a severe winter when the harbor below the Battery contains large fields of floating ice which must be "bucked," split open and passed through. It can, therefore, be seen that this ice service on the Staten Island Route is a very different one from that on the Lakes where a channel has to be kept open for months through solid and *stationary* ice-fields on either side.

For a number of reasons, in my opinion, the most suitable form of model for the route and conditions in question is such a one as outlined in the design shown. The reason bearing more directly on Mr. Holloway's remarks is this: In passing through broken ice, or comparatively small field-ice *floating in free water, i. e.*, not confined by stationary ice on each side, I believe it is far better and more economical of power to *plow up* this ice, split it, and push it to one side (over-riding it only slightly with the upper parts of bilges), than to push it down bodily with the bow and beat it down with the paddles. In riding down a field of thick and tough ice the *buoyancy* of a great part of the field has to be overcome in addition to the force needed to break a channel; in plowing it up the weight of only that small portion of the field near the bow is raised in addition to the force required to break the ice; it is fair to assume, too, that ice is more easily broken by a force applied in the direction in which it is *not water-borne*. The shearing or breaking strength in ice is small when the ice is taken alone, but when that ice is *backed up* by water throughout its whole surface this strength becomes very much increased.

Mr. Emery's exceptions in regard to ice banking up in front of

the bow and being driven down to be sheared between the propeller blades and rudder or stern post, are certainly well taken, and it is this very objection which was the subject of most serious thought when making the design. It was the consideration of this point, together with the accompanying advantages of finer lines for speed, which led directly away from the "spoon bow," which in a more or less pronounced shape is present on all New York ferry-boats. The "spoon bow" on a screw-boat would undoubtedly cause the action which Mr. Emery describes; in such a bow the abrupt "shoulder" or "bilge" which overrides and bears down the ice commences at or very near the stem; in the design of the paper this "shoulder" commences, *very gradually*, about midway between stations 80 and 90 (see Fig. 85.) Forward of this any immersed section of hull is an extremely sharp wedge with *no tendency whatever to ride down the ice.* In fact, the shape of guard-stem and braces will tend to *raise* the ice or other floating matter in the wake of the screw, and the action of the bow-wave (when at considerable speed) will continue this rising tendency to at least station 90. I have tried to do exactly what Mr. Emery suggests and advocates, viz.: throw the bow-screw out *ahead* of where the ice would be banked up and pressed down. I have done this and at the same time kept the screw shielded, as it should be, by the stem-guard, braces and deck. I have also kept the rudder forward of the screw so that it may be in the most efficient position for *deflecting the propelling current* when this same screw is the stern screw (boat moving in reverse direction). It should be remembered that I can easily get 12 to 14 inches *more immersion* for the screw if thought necessary, and by using the Kunstädter arrangement, I can do this without increasing the draught. The design is, as I have said, only *in outline* to "talk at." It can most certainly be improved when studying out details and working drawings. The shearing action which Mr. Emery fears, cannot take place unless the ice is overridden and *pressed down* into the screw; if I avoid this pressing down until *after* I have got the ice past the screw I have avoided the trouble.

Independent paddles with two sets of compound, inclined, direct acting engines arranged below decks so as to take up the same deck and cabin space as in the present arrangement (see Figs. 77 and 78) would certainly be an improvement, but it is maintained that this would be only a slight one, compared with the advantages to be obtained with screws. There would be a material

increase in the weight of engines and machinery, and the weight of boilers, hull, etc., would remain about as in the third boat of the table, with no gain whatever in that very important item, *deckroom*. It would also involve two engineers, and more complicated and expensive machinery than the third boat. The only gain would be that of considerably increased maneuvering power and, in my opinion, this power would be considerably less than with screws, especially if the Kunstädter system, or some similar one is used, because, taking the model of screw-boat, the "arm" through which the turning force of the screw acts (*i. e.*, the half length,) is about $4\frac{1}{3}$ times as great as that of the paddle under similar circumstances (*i. e.*, the half breadth plus $\frac{2}{3}$ face of paddle), and the turning force of the screw and deflected current from rudder resolved at right angles to its "arm" cannot be fairly estimated as low as $\frac{1}{4}$ the turning force of the paddle. Besides, with the independent paddle-engines, we still have the old, slow movement. One of the important features of the screw is that the engine in maneuvering can, in emergencies, be started at full power, and reversed at same *almost instantly*. An examination of the maneuvering of boats with independent paddles, such as the *Wm. T. Hart*, at Newburgh, for instance, will not be very encouraging if we bear in mind at the same time some of the admitted advantages of screws in this particular.

Mr. Stratton misapprehends my meaning when he assumes that I propose the compound beam engine for short ferry-routes. I am decidedly of his opinion that it is not the thing for routes directly across the East River, such as Fulton, Hall Street, Catherine and similar ferry-routes. I am not so sure about the North River routes. There is evidently a point somewhere at which the fitness of such an engine for different lengths of route changes; this point, to my mind, is somewhere *between* ferries of such length as above mentioned and those equal in length (five miles) to the Staten Island Ferry. It was with special reference to this latter ferry that the compound beam engine was considered, and even then, as "at best but half a step," in view of the advantages offered by the screw-engine.

Mr. See believes that I have sacrificed handiness by abandoning twin screws with their separate engines, as in the Mersey boats, in favor of a single screw at each end both on one engine. With the use of the Kunstädter gear I do not sacrifice any part of my maneuvering power; although without that gear it is true that as much maneuvering power is not attained with single screws as

with twins. I simply give up in this particular case a portion of the handiness for the sake of simplifying the machinery and reducing the running expenses, still having a superior amount of handiness left. If two engines are used, either with two sets of twin screws or with single and independent screws, it will cost between $170 and $200 per month *extra* for engineers' wages alone (*two* extra for "watch and watch"), whereas with the arrangement shown, the screw-boat takes no more engine-room force than the "Southfield" type. The design of any boat consists of compromises; efficiencies on the one hand being reduced to obtain certain desired advantages on the other, and so make the boat as a whole more suitable for the special purposes intended. The matter of losing economy by cutting out the air-pump and substituting a "high-pressure" surface condenser, is in itself a very broad and comprehensive subject, incapable of being argued properly here for lack of space, and because there is much to be said on both sides. Briefly, however, I will give some of my reasons for preferring the arrangement proposed in the paper to that of the ordinary surface condenser and air-pump. I believe that very often on board ship the conditions are such that the most efficiency can be obtained by taking only enough heat out of the exhaust steam to reduce it to water of a temperature which can be handled by pumps and then forcing this water through an exhaust feed-heater back to the boiler. In this way it is perfectly practicable to keep the temperature of feed at the check-valve *over* 200°, instead of at or below 120°, as in the ordinary way. After the exhaust steam is reduced to water which can be handled and put back into the boiler, any further abstraction of heat is useless and causes a *direct loss*, because that heat must be put back again, and even if it is put back by the exhaust steam, there is a loss. To this loss (large in many cases) add the power required to run the air-pump, together with the first and running cost, weight, and room of that pump; also the care, attention, and repairs which it requires; the extra power for the circulating pump above that required with the "high-pressure" condenser, and the extra weight, cost, space occupied, and attention needed in the whole vacuum rig aside from the air-pump. Then set off against this the saving of steam or the actual gain in horse-power by the use of a vacuum.

The balance, to my mind, is strongly *against the vacuum* in the case under consideration. On shore it is an entirely different matter. A vacuum often costs absolutely nothing there, and is pure

gain; but in many cases afloat it costs in power, loss of *heat*, *space*, weight, and other things, far more than it comes to.

Therefore, instead of sacrificing economy by dispensing with the air-pump in this case, I thought I was getting rid of a *nuisance*. I must insist that the braces at the stern will not "act as a net to intercept" floating matter, simply because, in the natural course of things, the immediate wake of a vessel is always cleared of such things by the hull. Water, and matter which it supports, *will not flow in horizontally* along the "run" of a ship in motion, and in a vessel moving through ice, it is well known that the *immediate wake is clear*. Ice, or other floating matter, and the water, too, when pushed to one side, *stays there*. This is matter of *fact*, not matter of opinion. Even where ice is overridden, it is forced up the inclined bilges by its buoyancy and leaves the ship's bottom or side at some distance from the center line, toward which *it does not return*, unless drawn down and into the screw by the screw's suction, and this latter happens only to a very slight extent in tugs and similar screw vessels. I do not deny that, in backing and filling in ice, a chunk might get wedged into the guards, but this would be a chance accident, not a regular and natural thing. A little working of the rudder and a boat-hook, used from the deck, would clear the ice. I propose using both bow and stern rudders (independently) *all the time*, as intimated on page 193 of paper; the rudder blades are to be arranged with suitable preponderance and worked clear around, the two forward edges always pointing one way. In such case the rudders will not be "liable to freeze fast to the cutwater." If the boat is laid up during freezing weather the rudders will be blocked with ice at the water-line, undoubtedly, and will have to be "cut out" just as I have seen rudders and screws and paddles "cut out" under similar conditions. With good design and construction the toggle joint in marine shafting has been used satisfactorily under far more trying circumstances and transmitting far more power than required in this case, where its extreme angle is *less than* 2°; notably in the steamship *Britannic*, working through an angle of over 18°, I believe. The *Britannic* was altered and a straight shaft put in, because of trouble in the lowering and raising gear of her shaft and the structural weakness of her stern; not on account of the toggle-joint. The steamship *Stratheden*, with a toggle-joint working through 90° and under the most trying conditions, has been running without trouble since the early part of 1882. Instances could be multi-

plied. Prof. Thurston's relation of his experience with the Hoboken Ferry Company in regard to a proposed screw-boat in 1871, is most interesting news to me; the time was evidently not "ripe" then for the proposed advance, but now that our cousins across the water have shown us the lead in its practical application, it seems to me we can with advantage give more attention to this matter of screw ferry-boats.

CXCVI.

THE RATING OF STEAM BOILERS BY HORSE-POWERS FOR COMMERCIAL PURPOSES.

BY W. P. TROWBRIDGE, NEW YORK, N. Y., AND C. B. RICHARDS, NEW HAVEN, CONN.

THE use of the term horse-power to designate the capacity of a boiler for making steam has become so far universal, at least in this country, that it is employed at once to furnish in a general way, not only a basis for estimating the first cost, but also a measure of the performance of a boiler when in use.

Even if it were desirable there would probably be little prospect of success in any effort which might be made to substitute any other form of expression or to give any other name to the unit by which boilers are commercially rated. When a boiler is spoken of, or advertised as a ten, twenty, or fifty horse-power boiler, it is implied that a unit which in this connection is called a horse-power is used to measure in some way the magnitude, or value, or performance of such boiler.

The question then immediately arises, what is the character of this unit which, whether properly or not, has come into use in connection with boilers? That it has not the same meaning as when employed as a unit of rate of work is at once conceded; but that it is considered to be a unit of measure of some kind, is also certain, otherwise it could not have come into use commercially as designating, by its multiples, the value of what one man sells and another man buys—the basis of a contract.

In their most common uses, boilers may be said to be the sources of energy which is utilized or exerted in actual *work* performed through the medium of engines of greatly varying economical efficiency. But inasmuch as the efficiency or inefficiency of an engine in no way effects the possible performance of the boiler, or the possible value of the boiler, it is admitted that the term horse-power as a unit applied to boilers has no direct reference to work performed. There is no doubt that the expression originated in the idea that a boiler which is intended to supply steam for an

engine of a given horse-power may properly be called a boiler of a given horse-power; for this term, as applied to boilers, has come down to us from a remote time when there were few if any applications for the use of steam except for supplying engines which differed little from one another in their economical efficiency. The incongruity of the expression has, however, become more apparent as the use of boilers for other purposes than power has been extended, and for which purposes they are still sold by the horse-power, and it is becoming more important every year that the true significance of this unit as applied to boilers should if practicable be properly defined.

In commercial transactions, where a unit is employed to define the quantity or value of the thing bought and sold, that unit has a value at least approximately definite and universally accepted, either expressed by the unit itself, or implied through common usage, or by some statutory provision. Men buy flour by the barrel with confidence because they know that a barrel of flour must by law weigh a certain number of pounds. Land is sold by the square foot or by the acre, but it is understood that all surface measurements of land thus bought and sold are reduced to horizontal planes, otherwise there could be no universal unit of land measure such as is now employed; and in all standard commercial transactions where units of measure or value are used, law or custom sufficiently defines the unit of measure, so that there need not be any misunderstanding as to the signification of the particular terms employed; and it will hardly be disputed that both parties to a contract cannot be fully and equally protected unless both distinctly understand the scope and meaning of the specific terms used in the agreement between them.

An apparatus for generating steam is a complicated structure when considered in reference to the draft, to the proportions of its parts, to its details of construction, and to its management. A variety of circumstances connected with combustion, transfer of heat, and evaporation of water, combine to give it a specific value to the purchaser, who however as a general rule has little knowledge of the definite principles which govern its performances. Without better guidance than is now afforded, the buyer must trust largely to the manufacturer or vender to obtain what he needs; and if disappointment or disagreement occur the former is usually quite helpless from not having been able to understand distinctly what he has purchased, and from the absence of any common usage

or accepted standard of capacity to refer to. The committee on boiler tests in their very able report recently submitted to the Society, recognize this condition of things, when they state that "what is needed is a standard unit of boiler power which may be used commercially in rating boilers, and in *specifications* presenting the power to be demanded by the purchaser and guaranteed by the vender."

Can we look to the "horse-power" of a boiler as the unit of boiler power for a remedy, partial or complete, of the difficulty?

This unit, whether considered as it is popularly used or scientifically defined, must be, like most other units, a complex unit, *i. e.*, it must be composed or made up of two or more simple units. The horse-power considered as a unit of rate of work involves space, time, and force—three simple units. But it is well known that each of these simple units is arbitrary, and each refers to certain conditions which are universally accepted: the element of space to a standard foot, the element of time to the standard unit of time, and the element of force to a standard unit of force. If the unit of boiler power is to have a fixed, definite, and universally acceptable meaning, it also must be referred in its elements to standard or fixed conditions. Whenever a unit is employed to designate the quantity or value of a thing sold, the buyer and seller should be equally protected by having the elements of that unit definitely fixed, either by common usage or special understanding. If dissatisfaction occur, and an appeal to the courts is taken, unless it can be shown that both ought to be expected to have the same definite understanding in regard to the unit which was employed to measure the value of the property, that unit would be of no aid whatever in the settlement of the difficulty. The seller who has got his price would have the best of the bargain.

The question then recurs, what is this unit of boiler power, and can it be stated in such a way as to be of substantial value in commercial transactions? If not, then *it is difficult to see that it can have any special value whatever*, for in a boiler test, where all the special conditions of combustion and evaporation are noted, all an expert can do is to determine and note the results, whatever they may be. If it be a competitive test, the performances of two or more boilers reduced to the same standard conditions will determine which is the better under those conditions, without reference to any unit of power, and if it be for the information of the owner, the results stated in units of boiler power can have no special significance un-

less this unit has a distinct value or meaning on which some question especially depends; and we repeat, it is difficult to see how a unit of boiler power, by whatever name it may be called, can have special importance or significance, except as a standard by which the value or performance of a boiler may be commercially stated, or as a basis for contracts, agreements, or estimates. It does not otherwise appear to have any important connection with boiler tests.

The essential elements or simple units, which must compose the complex unit of commercial boiler power are: 1st, a certain weight of water evaporated in a given time; 2d, a unit of evaporation as determined by the temperatures at which the water is supplied and evaporated; and 3d, the quantity of fuel required to evaporate the given weight under fixed or specified conditions.

If either of these elementary units is left indefinite the whole unit will be indefinite. Let us consider them separately.

I. The weight chosen must be a pound, or the multiple of a pound.

II. The quantity of heat required to evaporate a given weight will be established when a "unit of evaporation" or the quantity of heat required to evaporate one pound is chosen. This requires that the temperature of the feed water and the temperature of evaporation shall be taken into account. It is a matter of mere convenience or expediency whether the old and well-established custom of assuming both these temperatures at 212° Fahr. shall be departed from and new temperatures selected.

The number of pounds of water, or the weight of water, which shall enter the unit of boiler power, has not been fixed either by statute or universal custom, while the third element is also one which has not been fixed.

The most recent effort to treat of this subject with definiteness is the very exhaustive Report of the Committee on Boiler Tests, presented to the Society at its XIth Meeting in May, 1885. It has been contended in the discussions which followed, that it is unnecessary to introduce the element of fuel definitely, and that the unit of boiler power is exact when defined with only the first two elements above considered, the third being practically left out of consideration or named in an indefinite manner. We believe the advocates of this view have failed to recognize the distinction between the "horse-power" unit by which a boiler's capacity may be rated commercially, and the simpler unit, which is a constituent element of the complex

unit under discussion, and which merely expresses the quantity of steam which shall be assumed to represent the average consumption of steam per horse-power per hour of a non-condensing engine. They doubtless refer to this simpler unit. The consideration of fuel consumption is, in our opinion, a vital point. Upon it depends entirely whether it is possible for this Society to suggest or propose a unit of boiler power so definite that, used as a basis of agreement between buyer and seller, both can be fully protected.

Under the present unsettled state of the question, a person who buys a boiler of a given horse-power is certain of but one thing—its first cost. He must trust to the seller for its performances. When the seller states that it is a boiler of so many horse-power, and the purchaser takes it as such, the latter tacitly waives all questions as to what the term "horse-power" means when used in this connection.

We will suppose that it be assumed that a boiler horse power is properly defined as a certain number of pounds of water evaporated from feed water at 100° F. and at a pressure of 70 lbs., all special conditions of draft, etc., being left out of consideration as having nothing to do with the unit whatever. A purchaser buys a boiler and has a guarantee that its capacity is a given power, according to this "standard;" he finds, however, that its performance is unsatisfactory, complains, and appeals to the courts; and experts are called to test the boiler according to the rigid, exact, and in every way very excellent processes recommended by the committee of this Society, one expert for the complainant and one for the defendant. Inasmuch as the rules for the tests do not specify invariable and uniform conditions in regard to draft or rate of combustion, the experts are left to their choice in these matters.

It is scarcely within human probability that their determinations of the power of the boiler would agree, if they work independently of each other, yet both might make their tests by the same rigid rules, one expert employing a very different rate of fuel consumption from the other; for even the committee's recommendation respecting "moderate draft" and "good economy" have no reference to the unit of boiler power as defined in that report and explained by its chairman in its appended discussion.

Of what use or value would their tests be in arriving at a judicial settlement of the particular point in dispute? And of what use in this connection would be a standard unit of boiler power, thus defined, since it has not been a protection to the purchaser, and has

been of no aid to the court? The seller may have been honest enough, but there was no definite understanding in regard to the meaning of the terms employed in the contract, not even such as might in some transactions be derived from common usage.

Again, we will suppose a man to purchase two boilers of the same nominal horse-power from two manufacturers. He may pay the same or different prices for the boilers. He confidently puts them at work under the same conditions of draft, etc., and quite naturally finds very different results. Experts are again called in, and each, independently of the other, tests both boilers; they find not only that their determinations disagree as to the two boilers, but neither boiler gives according to their rating the power for which it was sold. Surely in this case again the unit of boiler power as defined has been of little help even in standard tests. Let us suppose, however, that the experts agree beforehand as to the conditions under which they will make the tests; their results harmonize, but unless they have been invested with the power of *arbitrators*, the results are of no value in settling the dispute. They have agreed to agree on certain points, that is all. They have not settled the dispute between the contending parties.

If such things can happen in a common commercial transaction, a remedy is needed, and these hypothetical cases, if they be possible ones (of which there can be no doubt), show that the unit of boiler power as defined and explained by the committee, is not sufficiently definite and does not supply what is needed. This unit should be so defined that an expert in making a test in cases like those above mentioned must adopt certain conditions, fixed either by common usage or by the terms employed in defining the unit. His results will then settle a question in dispute—not as a matter of his opinion or judgment, but as a matter of experimental facts. His opinion, or his choice of dominating conditions or circumstances, need not, and should not, enter into the result.

A steam boiler is an apparatus or structure in which a part only of the heat evolved in the combustion of fuel is imparted to water, converting it into steam. The proportion of the whole heat so transferred per pound of coal, depends on several independent circumstances: to wit, the quality of the coal, the rate of combustion or draft, the management of the fires, and the amount of heating surface exposed to the action of the heated gases and to the radiation from the incandescent fuel.

The quality of the coal and the management of the fires are

under the control of the purchaser. · Custom or common usage, and even the ordinary principles of fair dealing, would demand that, in case of a dispute, good ordinary coal should be used, and that the management of the fires should not be intrusted to an ignorant or unskilled stoker. This the seller has a right to expect. Common practice and universal custom in both these respects may be relied on. The litigant who should insist on having his boiler tested with inferior coal, and under the management of an ignorant stoker, or who should claim that he must get the full rated power of a boiler under these circumstances, would have no standing in court. It is not so, however, with the quantity of fuel burned on a given grate in a given time, or the draft. This with a given boiler depends on the dimensions of the chimney, in cases of chimney draft, for which common practice or usage affords no rule, although the draft exercises a most important influence on the amount of evaporation per pound of coal burned.

The height of the chimney may vary in ordinary practice from 20 feet to 100 feet, the combustion varying between these limits generally from 60 pounds of coal burned to 150 pounds, for each square foot of cross-section of the chimney.

The quantity of heating surface and the proportion of the heating surface to the grate surface must almost necessarily be fixed when the boiler is made; and when once fixed are usually invariable; but the draft is a variable quantity. Here then is one of the principal points where the buyer and seller should be furnished with some common ground to stand upon. They cannot refer to universal usage or common practice. An "average" height of chimney or rate of combustion is too indefinite. They should both understand what rate of combustion is to be adopted when the boiler is to develop the power for which it is guaranteed—a certain number of horse-powers.

The capacity of a given boiler for making steam, the grate and heating surfaces being fixed, depends directly on the rapidity of the combustion or the quantity of fuel burned in a given time, the capacity increasing as the rate of combustion increases, but with a diminishing rate of increase.

This diminishing rate of increase in the power of the boiler arises, as is well known, from the fact that although the heat evolved by the combustion of one pound of fuel is the same whether the combustion be slow or rapid, yet the proportion of this heat which is transferred to the water in a given time depends on the

laws of transfer of heat through the metallic plates, a greater proportion of heat being thus transferred with slow than with rapid combustion; a greater proportion also being transferred the greater the amount of heating surface in proportion to the weight of fuel burned in a unit of time.

It is therefore necessary in establishing a unit of boiler power, that the rate of combustion as a limiting condition shall be fixed, otherwise this unit will be an indeterminate quantity.

By referring the rate of combustion to the heating surface this object is not only attained but a reasonably uniform standard for the economy of evaporation—a most important consideration in connection with the market value of boilers—is secured.

This idea of referring the rate of combustion to the heating surface was suggested by Rankine, and also by General Morin, and if it be impossible to introduce the rate of combustion as an element of the unit under consideration, it seems to us that it would not be advisable for this association to commit itself to any definition whatever, but rather to discourage the use of the expression horse-power as applied to boilers.

We believe however that the subject is well worthy of further consideration by the Society, not only with the object of giving, as far as possible, uniformity to the rating of boilers throughout the country, but for the purpose of affording in cases of actual dispute or litigation, some common ground for engineers and experts to stand upon in their determinations of the commercial rating of boilers.

It is not the object of this paper to discuss in detail the questions which must be considered in establishing a proper unit of boiler power; nor even to suggest or recommend any definite formula for such a unit; but rather to urge the subject upon the Society as *one which should be separated entirely from that of boiler tests*; and which, since it has come before the Society for discussion and action, should be treated with the care which its importance seems to deserve.

While commercial transactions in boilers are not very large and extended, compared with some other branches of trade, yet when the importance of the steam boiler is taken into account, and the many issues which may depend on a correct understanding of the term "horse-power" as applied to it are considered, it seems to us that the American Society of Mechanical Engineers may do the public a real and substantial service by making this subject one of special consideration and action.

Purchasers of boilers, who are chiefly interested, have no other resource for reliable authority or information than the deliberate decisions of an engineering society like this, which in its organization and membership is in every way competent, and the only public authority to which such questions can properly be referred.

In conclusion, we desire to state that we consider that the chief object to be attained in agitating this subject is the adoption of a unit so clearly and precisely defined as to remove as far as possible all uncertainties as to the conditions which form the basis of a purchase and sale, and under which precise conditions an engineer or expert must undertake a test in case of dispute.

The unit of boiler power which shall meet public expectation should evidently be one so defined that no two experts in determining the power of a boiler could substantially or materially disagree in their determinations.

DISCUSSION.

Mr. Babcock.—However much we may dislike the term horsepower as a rating for boilers, it is too late to change it now, and as the principal thing is to have some measure to use for that purpose, it matters little what name we give it. "A rose by any other name would smell as sweet," has passed into a proverb. It is certain that a bushel would be no less in capacity or usefulness if we were to call it a "horse-measure," though the name would be inappropriate. So, while we may not like the term horse-power, the name is of less consequence than the assurance of its accuracy as a measure. The main question is: Can we so define a boiler horse-power that its value as a unit of measure is capable of accurate determination.

If simplicity were all that is desired, we might adopt Watt's unit of a square yard of heating surface, and a cubic yard of contents. In fact, a similar system has been the basis of the practical rating of boilers to a greater extent than any other; the unit, however, varying from six square feet to twenty, according to the location, the style of boiler, or the whim of the boiler maker. But a horse-power based on amount of heating surface is as definite as would be a gallon which should be defined as a *pound of pewter made into a pot.*

Watts had another standard of horse-power in boilers which has been used to some extent, defined as one cubic foot of water evaporated per hour. This is better than a square yard of heating surface,

and is sufficiently simple, but is still far from a definite measure of capacity, and much too large for modern practice.

The committee of this Society in their able report discussed at the XIth meeting, have sought to give us a standard measure of horse-power in boilers, both accurate and convenient. The question raised by the paper just read is not whether such a standard is too large or too small, but whether it is sufficiently accurate for practical use; and the authors argue that it should have an additional element—namely, the cost in pounds of coal, or, as they express it, "the quantity of fuel required to evaporate the given weight of water under fixed or specified conditions." Would this add to the accuracy of the unit, or to the convenience of its use?

Let us suppose it has been adopted. A buys of B a boiler of 100 horse power, which horse power is to be thirty pounds of water evaporated per hour, from 100° F. into steam of 70 pounds pressure, *using not more than three pounds of fuel for that quantity.* A sets up his boiler with a chimney 40 feet in height, and burns pine wood. He finds he can evaporate the 3,000 pounds of water per hour under the specified conditions as to temperature and pressure, but that it requires not only 300 pounds of fuel per hour, but eight hundred, and therefore claims the boiler is not 100-horse power. But B says he did not mean pine wood when he said "fuel;" he meant anthracite coal. A tries anthracite coal, but can burn only 200 pounds per hour, which evaporates only 2,000 pounds of water. Again he condemns the boiler as below its rating. B now claims that the chimney is too short, while A complains that he cannot afford to pay $6 a ton for anthracite when he can get pine wood for $1 a cord, or Illinois bituminous coal for 10 cents per bushel; so B agrees that Illinois coal is "fuel," but requires the stack to be increased to 100 feet. After the delay and expense of these alterations A finds his boiler evaporating 3,600 pounds of water per hour, but requiring to do it 600 pounds "fuel," *i. e.*, Illinois coal. More complaints follow, and as a well of natural gas has been struck in the vicinity, it is finally agreed that that is the cheapest and best "fuel." It is therefore applied, when the boiler evaporates 5,000 pounds in an hour; but as there is no way of weighing the fuel, A refuses to pay for his boiler on the ground that he is not satisfied that it has the power guaranteed! How much better off is B for having the additional element of fuel in his definition of a horse power?

It needs no further argument to show the added difficulties of such a measure, and when we remember that under the term "fuel" is comprised all grades from sawdust to petroleum, with evaporating values from one to twenty times their own weight of water, it is difficult to perceive how such a variable element can add to the definiteness of the measure.

But even if terms could be found to express this element in definite form, would it add to the proposed measure of horse-power, either in definiteness or accuracy? We are told that an object may have either one, two or three dimensions—but that a body with four dimensions is unknown. A measure of capacity must have three dimensions, but has no use for four. A horse-power in physics, has three elements: force, space, and time. A horse-power in boilers, as defined by the committee on boiler tests, has the equivalent of static force or weight in the water evaporated, of space in the increased volume, and it has also the element of time. It has no use for any more. It needs the added element of fuel burned no more than a ton of coal needs the requirement that it should, in addition to weighing 2,240 pounds, cost no more than a specific amount. In the matter of horse-power, the question of fuel is a question only of cost, and it would be more convenient and desirable if this element were to be expressed, that it should be stated in dollars and cents, instead of pounds of fuel. And then we might go further, and establish a standard of cost outright per horse-power, of the boiler itself. Some bewildered purchaser, unable to determine the point himself, might thank this Society for doing so. Why not? It is claimed that we need a unit for "commercial purposes," as a standard between buyer and seller, and while we are about it, it might as well cover all the ground.

But the real question before the Society is: What shall be considered a standard of boiler horse-power? Such a determination is wanted, not only as a commercial question, but as a unit of measure among engineers. So far as can now be seen, professional engineers, like doctors and lawyers, must remain a necessary evil until "all the world and the rest of mankind" are educated to know everything. There is no more reason why a man should be his own engineer than his own lawyer or doctor, and there is truth in the adage, "A man who is his own lawyer has a fool for his client." The standard required, therefore, has no inherent necessity of being adapted to the comprehension and use of the boiler buyer. As properly could we demand that all the formulas of

medicine should be comprehended by the patient, though we cannot deny that the patient would be better off for knowing.

The simpler the formula of our desired standard, the better. We might define a gallon correctly as a measure 7 inches long, 6 inches wide, and 5½ inches deep; and to determine the number of gallons in a given vessel, we could divide the length in inches by 7, the width by 6, and the height by 5½, and then multiplying these quotients into each other, we should have the contents in gallons. A much simpler way is to say that a gallon is a measure containing 231 cubic inches, and dividing the contents in cubic inches of any vessel by that number, will give its contents in gallons. The standard proposed by the committee on boiler tests is of the first character, composed of a standard temperature, a standard weight, a standard pressure and a standard time. Instead of adding to these a standard cost, as advocated in the paper under review, these might be reduced one-half to great advantage. A dynamic horse-power in its simplest form is 33,000 foot pounds per minute. A boiler horse-power should be defined as 33,000 *heat-units per hour imparted to the water*. That represents a definite quantity of work, and is simple to comprehend and handy to use. It so nearly corresponds with the standard proposed by the committee as to be practically identical, while it eliminates the elements of temperature, weight, and pressure, or condenses them into the one term—heat-units—and being simpler in expression, is more readily understood and calculated. The objection which has been made that there is a slight difference in the statements as to what constitute a heat-unit, holds with equal force against our measures of length, as Prof. Rogers has shown us. Nevertheless, we do not hesitate to use our foot rules, and are not inconvenienced in practice if there be a slight discrepancy between them. If it be a radically new expression of the measure, that is no objection, for a society like this is fully competent to suggest and adopt a novelty, if by so doing it can better what has gone before, and advance the conveniences of mankind.

Mr. Kent.—The paper of Professors Trowbridge and Richards appears upon its face to be a criticism of the existing custom of applying the term horse-power, with the existing definitions of this term, to steam boilers, "as a basis of estimating the first cost," and also as "a measure of the performance of the boiler when in use." Precisely the same criticism which they make concerning the use and definition of this term as applied to boilers may be made con

cerning its use as applied to steam engines. In fact, in many cases in which they use the word boiler, the word engine may be correctly substituted.

To show this I quote from their paper, placing in brackets the words necessary to make their statements as correct in reference to engines as they are in reference to boilers.

"What is the character of this unit, which, whether properly or not, has come into use in connection with boilers [engines]? That it has not the same meaning as when employed as a unit of rate of work is at once conceded, but that it is considered to be a unit of measure [of engines] of some kind is also certain, otherwise it could not have come into use commercially as designating by its multiples the value of [an engine which] what one man sells and another buys—the basis of a contract."

"Boilers [engines] may be said to be the source of [prime movers conveying] energy which is utilized or exerted in actual work performed through the medium of engines [lines of shafting and machinery] of greatly varying economical efficiency. But inasmuch as the efficiency or inefficiency of an engine [the shafting] in no way affects the possible performance of the boiler [engine], or the possible value of the boiler, [engine] it is admitted [who admits this?] that the term horse-power as a unit applied to boilers [engines] has no direct reference to the work performed." A number of other instances may be given where the word "engine" may be substituted for "boiler" in their paper.

The term horse-power has two meanings in engineering literature: First, an absolute unit or measure of the rate of work, that is, of the work done in a certain definite period of time, by a source of energy, as a steam boiler, a waterfall, a current of air or water, or by a prime mover, as a steam engine, a water-wheel or a windmill. The value of this unit, whenever it can be expressed in foot pounds of energy, as in the case of steam engines, water-wheels and water-falls, is 33,000 foot pounds per minute. In the case of boilers, where the work done, the conversion of water into steam, cannot be expressed in foot pounds of energy, the value which the committee on boiler trials of this Society would give to the term horse-power is equal to the evaporation of 30 pounds of water of a temperature of 100 degrees Fahrenheit into steam at 70 pounds pressure above the atmosphere. This is equivalent to 34½ pounds of water from and at 212 degrees Fahrenheit, or 33,305 heat-units. Both of these units are arbitrary, the first, 33,000 foot pounds per min-

ute, first adopted by James Watt, being considered equivalent to the power exerted by a good London draught-horse, and the 30 pounds of water evaporated per hour being considered to be the steam requirement per indicated horse-power of an average engine.

The second definition of the term horse-power is an *approximate measure* of the size, capacity, value or "rating" of a boiler, engine, water-wheel or other source or conveyor of energy, by which measure it may be described, bought and sold, advertised, etc. No definite value can be given to this measure, which varies largely with local custom, individual prejudice, or individual opinion of makers and users of machinery. The nearest approach to uniformity which can be arrived at in the term "horse-power" used in this sense, is to say that a boiler, engine, water-wheel or other machine "rated" at a certain horse-power, should be capable of steadily developing that horse-power for a long period of time under ordinary conditions of use and practice, leaving to local custom, to the judgment of the buyer and seller, to written contracts of purchase and sale, or to legal decisions upon such contracts, the interpretation of what is meant by the term "ordinary conditions of use and practice." The boiler test committee so used the word horse-power as a rate for boilers in the paragraph in their report, in which they state that it is the opinion of this committee that a boiler rated at any stated number of horse-powers, should be "capable of developing that power with easy firing, moderate draft, and ordinary fuel, while exhibiting good economy; and further, that the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained." The committee purposely omitted to give any more definite rule or formula for the rating of a boiler, because it was impossible to lay down any law or formula for rating a boiler which would be at all likely to meet with general acceptance. I believe this is the only place in their report in which they use the term horse-power as meaning the rating of a boiler, except in the blank for reporting the trial, item 44, where they say, "horse-power—builders' rating, at . . . square feet per horse-power," showing clearly that they intend to have the builder rate the horse-power of the boiler according to his own ideas, they not attempting to tell him how he should rate it, except as in the paragraph before quoted. In all other cases, I believe, in which the term horse power is used in the report, the

reference is to the absolute measure of horse-power as a rate of work done, as defined in the first definition above.

It is evident in the first of the above definitions that a horse-power is an absolute unit, and that its elements are only, first, pounds; second, temperature of the water and steam; and third, time. The quantity of fuel required to evaporate the water has nothing whatever to do with the horse-power unit. Professors Trowbridge and Richards, on the contrary, assert that one of the essential elements, or simple units, which must comprise the complex unit of commercial boiler power is the quantity of fuel required to evaporate the given weight under fixed or specific conditions.* It may be interesting to know that this idea of introducing the question of fuel consumption into horse-power is at least forty years old, for I find that John Bourne answered it in his treatise on the Steam Engine in 1846. He says (page 110, 3d edition), "In estimating the proper proportions for a boiler and its appendages, reference ought to be made to the distinction between the power, or effect, of the boiler, and its duty. This is a distinction to be considered also in the engine itself. The power of an engine has reference to the time it takes to produce a certain mechanical effect, *without reference to the amount of fuel consumed*, and, on the other hand, the duty of an engine has reference to the amount of mechanical effect produced by a certain consumption of fuel, and is independent of the time it takes to produce that effect."

This confusion of the power or effect of the boiler with its duty is fundamental in the whole idea which is under review.

Whether the term horse-power of a boiler be used in reference to the rate of work of a boiler in a boiler test, or as an approximate measure of what the boiler ought to do in ordinary service, and of what it ought to be called in advertising or in buying and selling it, this term has reference only to quantity of work done in a given time, without any reference to cost of doing that work. Just as a 100 horse-power engine, sold as such, should be capable of developing 100 indicated horse-power under ordinary conditions of running without reference to the cost of engine, of steam, of lubrication, or anything else, so a 100 horse-power boiler, when rated as such, should be only capable of regularly furnishing steam enough to run such a 100 horse-power engine, or, as an average engine of 100 horse-power should require not over 3,000 pounds of steam per hour, a 100 horse-power boiler should be capable of furnishing that

* Transactions, Vol. VI., p. 318.

steam. The cost of the steam or of the boiler is not an element of the problem at all, so far as the meaning of the term horse-power is concerned.

In order to show more clearly, if possible, how well the unit which is already in extensive use, 30 pounds of water per horse-power per hour, works in actual practice, and how impossible it is that any unit which considered the fuel consumption could be of general application and use, I will give a number of illustrations of the use of the term connected with the purchase and sale of boilers, which have come under my own observation.

CASE A.—Mill owner having boilers and engines each of which he called 120 horse-power, has an accident with his boilers; thinks them not worth repairing, and telegraphs to a boiler maker, "for what price will you sell me two of your 60 horse-power boilers?" Receives price and telegraphs ship them at once. Boiler manufacturer writes accepting order, and sends also a regular printed form of specification, in which he agrees to furnish two 60 horse-power boilers, and states in parenthesis, horse-power equals 30 pounds of water evaporated, and particularly describes the boilers as having drums of such and such dimensions, tubes of certain number, length, and diameter, and grate surface of specific dimensions. Boilers are delivered and set to work, and mill owner soon telegraphs, "Boilers will not furnish steam enough to run mill: send expert to find out the trouble." Expert goes, reports that he has tested the boilers and finds that they are developing 160 horse-power, on the basis of 30 pounds of water evaporated being equal to a horse-power, and says that the trouble is that the engines are calling for more than 120 horse-power of steam. Thinks the engine is a wasteful one. Mill owner not satisfied, calls in engine expert who indicates engines, finds them developing only 120 horse-power, but from the shape of the indicator cards, estimates that the engines are using more than 40 pounds of steam per horse-power per hour; condemns the engine as being of wrong type for the service; recommends purchase of a modern automatic cut-off engine. Mill owner buys new engine and all trouble ceases. The two new boilers easily furnish sufficient steam to run it; thus far the question of fuel consumption has not entered into the matter at all, the whole question is one of horse-power for measuring the work done and as a measure of the size of the boilers by which they are rated, but by watching the coal bill, the mill owner finds that with the new combination of new boiler and new engine he

is saving 30 per cent. of the fuel used with the original combination. This fuel consumption, however, is a matter totally foreign to the question of horse-power. Boiler maker now demands payment of his bill; payment is refused on the ground that the new boilers were not 60 horse-power each; experts sent again to straighten matters out; mill owner says to him: "My old boilers were only 60 horse-power each; here they are out in the yard, measure them for yourself, and they run my mill satisfactorily; your new boilers would not run my mill, therefore I hold that they were not 60 horse-power boilers. By your failing to deliver two 60 horse-power boilers which I have ordered, you have put me to the expense of buying a new engine. You have caused my mill to be stopped for several weeks, and instead of my paying you for these boilers, I ought to sue you for damages." Expert says: "When we accepted your order for boilers, we gave you a detailed specification of exactly what we proposed to furnish you; we furnished you exactly what we said we would do, and, not only that, but furnished exactly what we had been selling for 60 horse-power boilers for the last twelve or fifteen years. These boilers are known in the trade regularly as 60 horse-power boilers, and they already develop by actual test 80 horse-power each. We will make no discount on our bill, and you must pay every dollar of it or stand suit." The man paid the bill without suit.

Case B.—Mill owner says to boiler manufacturer: "I like your boilers very much, but you overrate them, the 240 horse-power boiler which you sold me does not give over 200. I have another boiler of same general type as yours which is very much more satisfactory; no difficulty at all in raising steam with it, and I think it is as good a boiler in every respect." Boiler manufacturer sends expert to determine cause of this complaint. He tests both boilers; the 240 horse-power boiler will only develop 200 horse-power on the basis of 30 pounds of water evaporated, while the other boiler rated at 120 horse power develops over 200. As the 240 horse-power boiler was sold with the understanding that it would easily develop that amount, it is necessary to find the cause of its lack of power, and this, easily discovered. It has the modern ratio of heating to grate surface of 40 to 1, but in the circuitous passages through its tubes and between the boiler and the chimney there is such a bad arrangement of flue with three or four right-angle bends in it, that it is impossible to get a force of draft at the end of the boiler of more than 0.15 of an inch of water column. The other boiler has

the ratio of heating to grate surface of 20 to 1, and direct passage for the gases into the chimney, and a force of draft at the rear of the boiler equal to $\frac{3}{4}$ of an inch water column. These were the regular conditions or practice in that mill, and had been so steadily for at least two years. The engineer and fireman liked the 120 horse-power boiler much the best, for they could always get the fire to burn well under it, while it was frequently sluggish under the other. Expert tells mill owner your complaint about this 240 horse-power boiler is entirely a just one, you ought to have complained about it long ago; but the fault is not with the boiler, it was with the flue connections. If you would place these boilers each in the position of the other, and give this old boiler the poor draft, and the new boiler the good draft, you would have things about exactly right. The new boiler would then give you about 300 horse-power of steam, and the old boiler about 120, which it ought to do instead of 200. The new boiler is not working economically, because it is not worked up to its capacity, the fire does not burn fast enough to insure good combustion, and the radiation is a higher percentage of the total work done than it should be. The old boiler is also worked with very bad economy, because more heat is given out by the fuel than the boiler can absorb. The flue gases leaving the new boiler are too low in temperature to secure the best results, only about 300 degrees, while those passing from the old boiler are about 900. This involves a serious loss of fuel. That 240 horse-power boiler will easily give you 300 or 350 horse power if you set it properly. In this case also, the question of fuel consumption does not enter into the meaning of the term horse-power, either in the mind of the mill owner or the boiler manufacturer.

Case C.—Another boiler of 240 horse-power and identical proportions is complained of because it does not furnish enough steam. Expert tests it and finds it just does produce about 240 horse-power. The draft is excellent, coal may be burned when the fire has been thoroughly cleaned at the rate of 40 pounds per square foot of grate surface, but within two hours the coal clinkers so badly and the grates become choked so that no more than 20 pounds can be burned per square foot of grate; the clinker is very difficult to remove from the grate bars, and during the cleaning steam goes down to such a low point that the engine will not run. Mill owner says your boiler is not a 240 horse-power boiler. You have caused me serious damage by selling it to me as such, and if you don't straighten this matter out I will give your boilers a bad name all over the country.

Expert says the trouble is not with the boiler but with the coal, you ought to u e better coal. Mill owner says that is the best coal in this part of the country. I get it very cheap and am not going to change it, the old boilers burned that coal well enough and you must make yours do it. Expert begs for a little delay, ships a car-load of that same coal to another locality, where there is the same kind of a boiler which has a record with a fair quality of bituminous coal of developing 50 per cent. above its rated capacity on a grate surface which has been cut down from 60 feet to 48 square feet. At this new location the expert finds the boilers still giving satisfaction, and proceeds to test the boiler with the car load of coal from the other place. Within half an hour the engines stop for want of steam, the fire is drawn and the grate surface restored to its original 60 square feet and the coal tried again, and now by careful firing and frequent cleaning of fires enough steam is raised to keep the engine running, and the test shows that this boiler, which had a record of developing 50 per cent. above its rated power with contracted grate surface, develops 15 per cent. below its rated power with the enlarged grate surface. The problem is now solved, and the expert reports that for this peculiar coal the standard proportions of heating to grate surface must not be followed. To secure the best results the grate surface should be at least double, and the coal burn slowly to prevent its clinkering and to provide abundant chance for the air to get through the grates to burn the coal. Mill owner who complained of his 240 pounds horse-power boiler is enlarging his mill and orders 400 more horse-power of boilers on the same type on a guarantee of their giving satisfaction. The new boilers are built in precisely the same way as the old, but the grate surface is made larger, and entire satisfaction results. In this case also the horse-power refers to the amount of work which can be got out of a boiler and not to the amount of coal burned, to produce that work; the coal in fact was so cheap, that it was a matter of trifling importance how much coal was used compared to the damage which resulted in frequent stoppages of the mill for want of steam.

CASE D.—Mill owner wants about 400 horse-power of boilers; what's the price? Answer, $7,500. Why, that is exactly double the price for which I can get them of the same type that I am now using. Salesman investigates, finds that he is using two flue boilers each of 400 square feet of heating surface and four of them would easily produce 400 horse-power, and mill owner is nearly correct in saying he can get them at half the price of the others. The

salesman says no doubt you are getting 400 horse-power out of these boilers of yours, but they ought not to be rated at over 50 horse-power each, or 200 horse-power. Now, if my employers would allow me to sell you our boilers and set them the same as yours are set with a grate surface equal to $\frac{1}{18}$ of the heating surface I would sell you one of our 200 horse-power boilers and guarantee to develop 400 horse-power with it, but my employers insist on giving 12 square feet of heating surface for each horse-power, and give only 1 square foot of grate surface for every 40 of heating surface. By this means their boilers may come higher in first cost per horse-power actually developed, but it pays the purchaser in the long-run to buy them, in the saving of fuel which they accomplish. The result of the talk is that the new boilers are purchased at the price named and they are then tested. The test shows that they develop 500 horse-power, or one horse-power from every nine and a half square feet of heating surface: the old boilers are then tested and found to develop 400 horse-power, or a horse-power for every four square feet of heating surface. Thus the new boilers filled their guarantee and do twenty-five per cent. better, while the old boilers do one hundred per cent. better than their rating. Thus far the question of fuel consumption has not been mentioned, simply because it has nothing to do with the horse-power question. But for commercial reasons and to show the mill owner that he had better pay $19 a horse-power for the new boilers, rather than $10 a horse-power for the old ones, the fuel consumption is figured up showing the saving of thirty-five per cent. of fuel in favor of the new boilers, or enough to pay their whole cost in eighteen months, the works running night and day.

CASE E.—Mill owner buys the boiler rated 150 horse-power. Shortly afterward reports it is not more than 100. Expert goes to investigate, and while approaching the boiler-house, in company with the superintendent, says: "I see one cause of that trouble before I get into the boiler-house; your chimney is not large enough." "Why didn't your people tell us that when we bought the boiler?" "We had nothing to do with your chimney, we only sold you the boiler, and you put it to work at an old chimney which was too small for it." Tests were made, and the boiler shown to be developing about 110 horse-power. The coal was anthracite pea, rather dirty and very wet, as it was exposed to heavy rain out-doors.

A steam jet was put in the chimney temporarily and the power developed rose at once to about 150 horse-power. The grates were

then cleaned and anthracite egg coal substituted, and burned without the use of the steam jet. Nearly 200 horse-power was developed. The steam jet was then put on and the power raised to over 250 horse power. In this case the whole question was, whether the boiler would give the horse-power for which it was sold, the question of fuel economy was not considered at all. The result was entirely satisfactory to the purchaser. He built a new chimney, and there has been no complaint since.

CASE F.—Another 150 horse-power boiler, mill owner reports does not furnish steam enough to run the mill. How much coal are you using, he is asked. Not using coal at all, using shavings and refuse lumber. I supposed the old boiler I had to be of smaller capacity than yours, and had no trouble with it, and yours cannot be the horse-power it was sold for. Expert sent and finds a contracted flue between the boiler and chimney, secondly a hood over the chimney, which acted as an obstruction to the draft, rather than a help to it, and, thirdly, that the furnace under the boiler was not of sufficient size for burning the shavings properly, recommends change of these three faults in the order named. The contracted flue is enlarged and the hood taken off of the chimney, and satisfactory results ensue without making any change in the furnace. Here, again, the question of fuel economy had nothing to do with the problem; in fact, the fuel consumption was of no importance whatever. The character of the furnace under the boiler was such that the shavings were being distilled, and converted into gas and sent up the chimney in that form rather than being properly burned, so that the fuel consumption, under conditions of proper draft which thoroughly burned the fuel, might be even less than under poor draft which merely distilled the shavings into gas.

CASE G.—Another boiler fed with shavings and reported not to be developing its rated horse-power.

Expert finds that the blower carrying the shavings into the boiler furnace is so placed that it blows the shavings to the chimney before they get a chance to burn; the front of the boiler furnace is cold while the chimney is red hot. A change in the direction of the blower was the only one recommended. It was changed so as to blow the shavings down to the floor of the furnace, and cause them to rebound back into the furnace and burn there instead of burning in the chimney; the result of the change was entirely satisfactory, and the boiler was agreed to be furnishing more than its rated power.

CASE II.—A hypothetical case. A party writes to four boiler makers for s ecifications and estimates for a 100 horse-power boiler, of their regular makes. The specifications submitted may be tabulated for comparison as follows:

Maker.	Horse-power. Builder's rating.	Sq. ft. Heating Surface.	Sq. ft. Grate Surface.	Ratio Heating to Grate Surface.	Price.	Price per sq. ft. Heating Surface.	Price per H. P.
A	100	1,500	37.5	40 to 1	$1,500	$1.00	$15.00
B	100	1,200	30	40 to 1	1,260	1.05	12.60
C	100	1,200	40	30 to 1	1,320	1.10	13.20
D	100	1,000	40	25 to 1	1,200	1.20	12.00

Would-be purchaser is puzzled. A's boiler costs the most, but it is the cheapest per square foot of heating surface. D's costs least, but it is the dearest per square foot of heating surface. B's and C's boilers approximate to D's in cost, but to A's in cost per square foot of heating surface, while they differ widely in extent of grate surface. All the makers define a horse-power as a guaranteed evaporation of 30 pounds of water per hour, but all say their boilers will evaporate 25 or 50 per cent. more if driven. Would-be purchaser has heard that the question of horse-power of boilers is an unsettled one, and thinks he would like to see how the horse-power of these four boilers compares when some scientific rule is applied to it

He hears of two rules proposed, involving the rate of combustion of fuel under the boiler, and presented before this Society.* These are:

RULE 1.—H.P. = 35 pounds of water, from and at 212° F., when the rate of combustion *is not less than* 10 pounds of ordinary coal on each square foot of grate surface.

RULE 2.—H. P. = 40 pounds of water evaporated from and at 212° F., with a rate of combustion of *not more than* $\frac{4}{10}$ pounds of good ordinary coal per square foot of heating surface.

After learning further that one pound of "good ordinary coal" will evaporate 9 pounds of water, he applies these two rules to the specifications, with the following results:

	According to Rule 1.	According to Rule 2.
A's boiler, rated at 100 H. P.,	is *not less than* 96 H. P.	Is *not more than* 150 H. P.
B's " " 160	" " 77	" " 120
C's " " 100	" " 103	" " 120
D's " " 100	" " 103	" " 100

* Discussion of Report on a Standard Method of Conducting Steam Boiler Trials, by Prof. W. P. Trowbridge. Trans. A. S. M. E., Vol. VI., pp. 319, 320.

While he is cogitating over these curious results without getting any clearer idea of the relative horse-power of the four boilers, A, B, C, and D appear and urge their respective claims.

A is told, "Your boiler costs too much." A replies, "I am giving you more boiler than any of my rivals, 15 square feet of heating surface per horse-power. It is the cheapest per square foot of heating surface, and has the proper proportion of grate surface to give you both capacity and economy. It is the most economical of fuel, because it has the largest surface to absorb the heat.

B is told, "If A's boiler is 100 horse-power, yours is not. You don't give enough heating surface nor enough grate surface." B then "runs down" A's boiler, shows that a large portion of its heating surface is badly placed and therefore inefficient; that it consists of small tubes closely placed together, which will soon get covered with scale and cease to absorb heat. "A gives 40 square feet of grate, to be sure, but the gas passages are so contracted that his draft will be choked, and he cannot get any more coal burned, and therefore any more horse-power out of his 40 square feet of grate surface than I can out of my 30.

C is also told that his boiler is smaller than A's, and he replies as B did. Then he is told that his boiler is higher priced than B's. Yes, but he don't give you as much grate surface, and therefore the capacity of his boiler is limited. I can get 30 per cent. more horse power out of my boiler than B can out of his.

D is told that his boiler is entirely too small for 100 horse-power —only 10 square feet of heating surface per horse-power. "True, but I place every square foot where it does good service, and I can easily keep it all clean, so that it will do as much work as A's 15 square feet of heating surface per horse-power." "Well, your price is too high—$1.20 per square foot of heating surface, while A's price is only $1.00." "Certainly, for A's heating surface is only small tubes, while mine is large tubes and shell, costing more to build. I give you more grate, also, than A, and plenty of draft, which A don't give, and therefore I can guarantee to develop 50 horse-power more than A can, in case you need to drive your boiler at any time above its rating." "But your boiler will not be as economical of fuel as A's, because you have not enough surface to absorb the heat." "Well, there may be 1 or 2 per cent. difference, but what is that to you, when coal costs you so little, as you are near a coal mine, and you will not need to drive your boiler over 70 or 80 horse-power over one-tenth of the time?" D's boiler is

purchased, and is furnished with a large chimney. It is afterward tested. Results, 20 pounds of coal burned per square foot of grate per hour, or 800 pounds per hour. Water evaporation only 6 pounds per pound of coal=4,800 pounds, or 160 horse-power, according to the customary definition. As the rate of combustion per square foot of heating surface is $\frac{8}{10}$ pounds, or double the *maximum* allowed in Rule 2, that rule cannot be applied to the test, or if it is applied on a "pro rata" basis, the horse power might be called only 60. As the rate of combustion is double the *minimum* of Rule 1, that rule will apply, and the horse-power developed called 137. Now, shall this boiler be rated at 160, 60 or 137 horse-power? Had we not better leave the rating to D himself, who calls it a 100 horse-power boiler, rather than to attempt to lay down a law for him, which he would not follow if he could, and could not if he would?

These cases it is hoped are sufficient to show that the term horse-power is properly used as a rate of work done, or as an approximate measure which is expected to be done for the purpose of selling or advertising a boiler, and in either case the rate of combustion, or the fuel consumed, is not an element of the problem.

In numerous cases of complaint concerning contracts for delivering boilers with which the writer has had experience during the past three years, he does not remember a single case where the term horse-power has had any relation to the fuel consumption, and in most cases the use of the term in this way would have created confusion of the worst kind. In many cases a contract for selling boilers does contain a guarantee of fuel economy; but that is one thing and horse-power guarantee is another. In this case the contract says, we agree to furnish you a 100 horse-power boiler, the horse-power being defined as an evaporation of 30 pounds of water per hour, and we further agree that when the boiler is being run at a rate not exceeding 120 horse-power it will evaporate 9 pounds of water from and at 212° for each pound of anthracite egg coal. In this case the duty of the boiler is considered a separate matter from that of the power, just as was recognized by John Bourne forty years ago.

Prof. Thurston.—I am not quite sure whether this discussion is on the horse-power unit for boilers, or the method of measuring the power of boilers. When the matter of the unit of boiler power came up in connection with the work of the committee on trials of steam boilers, that committee could do no less than attack the latter

question, so far as lay in their province; but it did not lie within their province to determine a method of commercially rating boilers. It was their duty only to find a standard method of testing boilers, to determine efficiency and economy. In doing that, it was necessary for them to find a *unit of measure*, not a unit of the power by which the boiler should be rated, but the power actually delivered through the boiler; it is not necessary for me to go over the arguments pro and con. They were given very clearly, I think, in the report of the committee. The fact is, that that committee had no business, at that time, with the measuring of the capacity of boilers, with a view to determine how they should be rated as sold in the market. It seems to me to be evident at once that there are two distinct matters to be considered; first, what shall be the unit by which boiler power shall be measured? Secondly, under what conditions may that unit be applied, in order that we may be able to say that a boiler has a certain power, or has not a certain power. The first of these tasks—of determining the unit—fell within the province of the committee. They could not carry out the work assigned them without performing it, and the result was practically to say that, as we say 33,000 foot-pounds of work per minute, or 1,980,000 foot-pounds per hour, represent a horse-power for an engine, so, substantially, 33,300 thermal units of energy in the form of heat shall be the measure of a unit of boiler power, and those were the figures at which, substantially, the committee arrived (33-305), and they are in accord with Mr. Babcock on that point. They took a number which was, substantially, the same as that previously arrived at by the committee on steam boiler tests at the Centennial Exhibition. I think the difference in the two problems has been brought out in Mr. Kent's reply very well; and if I may be allowed to say so, without any attempt to bring up the humorous side of the matter, I would say that, to attempt the process of measurement which is proposed in the paper that has been read to-night, is very much like attempting to provide that a steam engine shall be measured up in units of 33,000 foot-pounds of energy delivered per minute, the engine working at 100 pounds of steam, cutting off at one quarter stroke, non-condensing, piston speed 500 feet a minute; the steam itself to be, substantially, dry, and the management to be good; the engineer being always sober and never sleeping on watch.

What remains to be done, undoubtedly, is to determine under what conditions it is fair to apply the measurement and to deduce

the rated power; that did not come within the province of the committee; but if the Society were to choose to refer the new question to that committee, I have no doubt they could settle it if it is desirable to settle it. I have never made the motion to refer that question to the committee on tests of boilers, simply because I do not feel certain that it is desirable at present to give a definite statement of the conditions under which the unit now established shall be applied. It would be fair to add that a great deal which is stated in the paper read to-night is indisputably exactly correct, and I presume every engineer present who is familiar with this subject will agree with nineteen-twentieths of what is there stated.

Mr. Chas. E. Emery.—The feeling that a standard for the power of boilers should also contain some provisions to secure reasonable economy is entirely natural. Further thought on the subject will show that it is as impossible to do this as it would be to provide that the cost of the power in all steam engines should be the same, or at least equal to a common standard. Even an uneconomical rotary engine may be the best for a portable fire engine, in which it is desirable to get as much power as possible with as little dead weight. So also in respect to the boiler for a fire engine, economy of fuel is not of the slightest consequence. Large power with small weight is all that is required, as fires are of limited duration. Similar considerations prevail to some extent in portable boilers using waste products for fuel, and in steam yachts making short trips, etc. Boilers used in fuel-producing districts also can be of cheaper construction, and therefore be less proportioned for economy than in other cases. Paramount to this, however, the mercantile consideration comes in. Purchasers demand low prices, sellers provide for them. It is the same with boilers as with engines, and no formula can be made which will eliminate this part of the problem. The power of the boiler must be confined to its steam-producing capacity. In order, however, that a given boiler may produce a given power under average circumstances, the report of the committee on boiler tests states that "the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained." This covers the whole ground. Of course this same boiler will develop different powers with different conditions of draft, and different fuel, etc.; but, in making a trial, the boiler, if forced, with the particular height and size of chimney in use when the trial is made, should be able to develop one-third

more than its rated power, and the actual power developed, as well as that which can be obtained with easy firing, can be readily ascertained by comparing the results of trials by the standard given in the report of the committee.

Mr. Samuel Webber.—I agree fully with Professors Trowbridge and Richards as to the necessity of fixing some limiting point for the rate of combustion as compared with the amount of heating surface.

A large number of experiments, which I have made at different times, to ascertain the relative economy of various forms of boilers and different kinds of fuel in the production of steam, for such purposes as bleaching, dyeing, and heating, have all gone to show that a slow rate of combustion would convert the largest amount of water into steam at a moderate pressure, say 30 pounds per inch; but these experiments would also have shown a much larger quantity of water than 30 pounds per horse-power.

If we assume what may be considered as fair average practice, an evaporation of 8 pounds of water per pound of fuel, and a consumption of 3.75 pounds of fuel per hour per horse-power, we reach this figure of 30 pounds per hour per horse-power; but, to enable us to make this assumption, we should know something about the area of grate surface, and area, height and draught of chimney.

So far as I now see my way to any fixed term for the rating of boilers, it is only to the simple one of 15 square feet of heating surface per horse-power for a tubular boiler, of the ordinary underfired horizontal construction, which experience shows will develop a horse-power of steam under fair working conditions.

These conditions, however, remain to be defined, and it seems to me that the statement should include the area of grate surface, and the rate of combustion per hour.

Without attempting to give the exact proportions, it seems to me that a form somewhat like this is wanted:

1 horse-power of boiler to be = to 15 square feet of heating surface, with a ratio to grate surface of 30 to 1, and a combustion per hour of 6 pounds of coal per square foot of grate.

I merely throw out these suggestions for future consideration.

CXCVII.

CRYSTALLIZATION OF WROUGHT IRON.

BY WILLIAM HILL, COLLINSVILLE, CONN.

THE writer would present before the Society a specimen of crystallization of wrought iron, which apparently took place under the influence of continued repetition of violent shocks. The specimen exhibited is from the lower end of a hammer bar of a

FIG. 71. FIG. 72. FIG. 73.

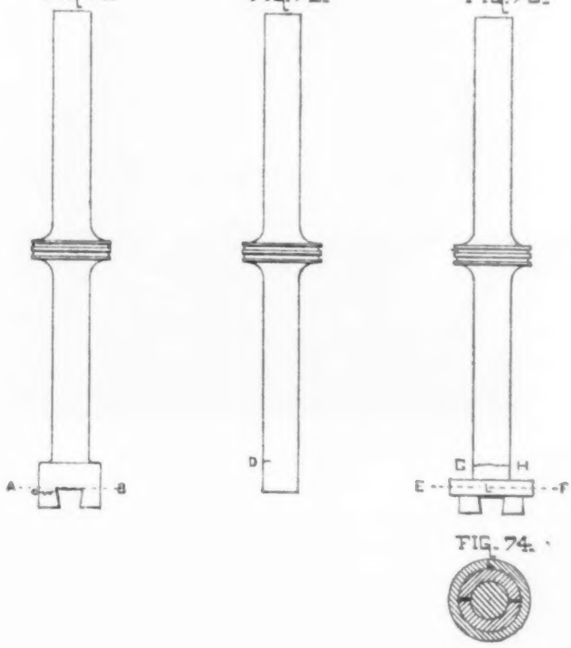


FIG. 74.

Sellers 2,000 lb. steam hammer. The bar was originally made as shown in Fig. 71, but after having been in use about five and a half years one of the lips broke off at C, Fig. 71. The bar was then cut off at A B, Fig. 71, and the head turned down to a size

uniform with that of the bar itself, as shown by Fig. 72. When the head was turned down a small crack was observed at D, Fig. 72. After turning off the head, a false head in two halves was made and put on, as shown by Figs. 73 and 74 (Fig. 74 is a horizontal section through Fig. 73 at E F).

The head was held in place by the ring L (Figs. 73 and 74), which was shrunk on, causing the two parts of the head to grip the bar tightly. A few months after the new head was put on, the hammer bar suddenly broke at G H, Fig. 73, the same point where

FIG. 75.

the crack had been previously observed (D, Fig. 72). It will be noticed that the break occurred at the point where the original forging was enlarged to make the head. At the time the break took place the forging had been in use five years, nine months and ten days, during which time it had been in use night and day almost continually, and had probably hammered about 13,500 tons of 200 lb. blooms, made from scrap iron.

By an inspection of the fracture reproduced in Fig. 75 by a photographic process, it will be seen that the forging is more crystalline than pig iron. It is difficult to determine whether the

forging was crystalline when made, or not, but it does not seem possible that it could have stood constant use for nearly six years before breaking, if it had originally been in its present condition.

The specimen is brought before the Society that the members may see in it a specimen which tends strongly to show that wrought iron can crystallize while cold, under long-continued repetition of shocks.

DISCUSSION.

Mr. Hutton.—I must confess complicity in the bringing of this paper and sample fracture before the Society, because of a desire to call out a discussion on the subject which it brings up.

I know of two theories or hypotheses about the structure of irons and structural alloys. No doubt there are many modifications of both held by the members present, and it will add interest to hear them and put them on record.

The first theory is that iron (both cast and wrought) is, to a degree, of a *colloid* structure like jelly or glue or resin. The phenomena which support this notion are those seen in the cold-flow of metals, under pressure or rolling. Hot wrought iron, as drop or die-forged, is obviously of this structure, and to hold that it retains the same qualities when cold only necessitates that there should be so much more force exerted to compel it to change its shape. The theory is, perhaps, the extreme reaction from the theory that a wrought-iron rod was a fascicle or bundle of continuous fibers running from end to end of it. In building up a crank or other forging on this theory, the object of the designer was to lay the fibers so as to have the strain come along their length. The jelly theory represents the other extreme. It has to be held, however, with limitations, for there must be time allowed for change of shape, which will differ with each form of the metal and the amount of change. The holders of this theory would explain the appearances on the fracture of this hammer-head by saying that these facets were the cleavage planes produced by shocks and compression, exactly as wax or lead under pressure stratifies at right angles to the pressure on them.

The second theory or hypothesis is the one which I am more inclined to hold, and may be called the crystalline theory. By this notion, a bar is made up of crystals of iron, interlacing and overlapping on the ends, and made to grip each other by their hold on their sides. When a bar is slowly broken by direct ten-

sion or flexure, these crystals stretch out so that on a ragged fracture we see the light reflected from their sides and the fibrous *appearance* is developed. Sudden fracture does not allow these crystals to stretch (or the metal to flow), and so we see their ends only, and the appearance of the break is called crystalline. High carbon steels whose crystals will not stretch without parting cannot be brought to show a fibrous break. The reduction of area at the point of fracture of a machine specimen is the result of the drawing out of the crystals at that point by the strain. The tensile strength of a bar is the hold which these crystals have upon each other by their sides, and the greater the hammer treatment or the "work" of the rolls in the reduction, the stronger will be the bar, because the crystals, being fined in their grain, get a better hold to resist pulling stress, and there are more of them per square inch of area. Or, another view would be that the greater work on the bar in hammer and rolls had elongated the crystals more when hot so that they were more likely to resist further pull when cold. This is confirmed by the fact that small sections are stronger per square inch of section than larger ones. By this theory, overheating of a bar, or heating without hammering, simply allows the elongated crystals to shorten back again to their octahedral form; the structure goes back more nearly to that of cast iron, and has no greater strength. Similarly, cold hammering, or hammering after the bar is cooled too far, tends to loosen the hold which the crystals have upon each other sidewise, and the forging has checks on its surface, or is rotten under strain.

On this theory, vibration either lengthwise or transverse has a tendency to loosen the hold of the crystals on each other. Each crystal alternately pulled and relaxed tends to work on its neighbors, exactly as they do in a piece of wire which is bent backwards and forwards for the purpose of breaking. This loosening or disaggregating process will always start at a place where the crystals are free at their ends, as at an abrupt change of section or a flaw in the iron, and the strength being gradually impaired by this action, the bar suddenly breaks at a strain perhaps much below what it was formerly able to withstand and has withstood when new. The sudden break allows no drawing out of the crystals which still hold and the fracture is called a crystalline fracture. Or the fracture may be partly fibrous in appearance and partly crystalline, because the break took place so as to admit of the stretching of certain crystals, but not of all. I used to think that

the relaxing of the sidewise hold of crystals on each other by vibration, allowed the crystals to shorten back from the condition of extension which the forge treatment had caused, and thus develop the larger crystalline faces visible on the break. The difficulty with that view is that it would require an increase of cross sectional area at the fracture to have the crystals lose their deformation lengthwise, and so far as I know that enlargement has not been observed. This enlargement would be called for by the analogy of the reduction of area at the extension of bars undergoing test in a machine. In a section which shows large crystals, as in the one before the Society, it seems necessary to hold that the larger section of the bar admitted of relatively less mechanical treatment to make a fine grain, and the slower cooling of the large mass from its plastic state when at a welding heat favored the formation of large crystals, as has been so universally observed in castings. I confess it is difficult for me to accept the bald theory that a bar can become crystalline in the solid by passing from some different structure. I do not believe in the fibrous structure of iron, unless he who uses that term means by it what I do when I speak of the bar as made up of separate crystals, which elongate under strain and fracture. There seems to me to be great force in the position that true crystallization of a substance can only take place from a state of either solution, fusion or sublimation.

There is another series of phenomena, explicable in part at any rate by this second theory. I refer to what has been called the fatigue of metals under shock, blows and strain. It would seem that nearly all the effects referred to by Colonel Webber and Professor Egleston, in a discussion on the Physical Properties of Metals and Alloys,* can be explained by assuming that a blow, by disturbing the initial arrangement of a crystalline structure, produces a change which can be made manifest by etching. The beginnings of fracture at "gag-marks," on a straightened rail, would be but results to follow rationally from a local disaggregation of the crystals at that point where the pressure had come, and vibration would do the rest on this theory. A gag or stamp pressure would produce its disturbing effect for some distance into the bar or rail, thus confirming previous observations in this direction. It may be also that the complementary observations of the restoration of their original properties to overstrained

* Transactions A. S. M. E., Vol. V., page 154.

metals, by giving them a prolonged rest, is to be explained by the notion that rest gives the crystals of the body a chance to seize each other again by the surfaces which vibration has separated, and thus to get back their first tenacity, just as lead surfaces attach themselves to each other.

I am aware that this discussion may be classed among speculations necessitating what Tyndall calls "the scientific use of the imagination." Nor do I suppose this crystalline theory, as so stated above, is entirely new, but I have never seen it definitely stated anywhere, nor has attention been called to how well it can be made to agree with a great many observed facts. Besides the confirmations above noted, it explains that unique and surprising case where an alloy increased in diameter under light cold hammering! The crystals becoming disaggregated necessarily required more room as they became less compacted. The spongy center so often found in steel taps and reamers, resulting from the fact that the round bar from which they are finished was worked out under the hammer into an octagon, is a phenomenon of the same class.

While primary reference is made in the above remarks to wrought iron, the same principles, with the proper limitations, will apply to steel and to some of the other metals and alloys, and it would be useful to hear further opinions in this matter, and to have suggestions made for tests confirmatory or to oppose to any theory which has been or may be proposed.

R. H. Thurston.—The subject of Mr. Hill's paper is of very great interest and of equal importance. The facts are among the most common and familiar in engineering practice; but the precise character of the change and the method of its occurrence are not so well determined. I do not think that I can add anything really new relating to the matter. It has been one that has attracted my attention, as it has that of every one who has had much to do, practically, with the handling of machinery subject to jar and repeated strain. I am not at all sure that this is a real crystallization, although some well-known writers and admitted authorities so consider it. It may be a form of granulation, not crystallization, due to repeated strains exceeding the elastic limit. There is no question that, under some circumstances, iron and steel can take on the crystalline condition. This is well known, and the kind of crystallization has been well settled. Both iron and steel crystallize in the "cubical system" of the

mineralogists, and in the related forms. This change from their usual amorphous or fibrous condition may be brought about, certainly, in either of two ways: by solution and slow deposition; or by heating up to the bright red heat, and holding that temperature for considerable periods of time. It is in the former way that the chemists, as Miller and others, have studied the morphological structure of these metals; and it is in the latter way that the crystallization observed by the engineer, and so often destroying his best work in wrought iron, is very frequently produced. It is caused by the prolonged heating, at high temperature, so generally needed in the building up of a large shaft, as for a marine engine, by "fagoting;" and such shafts, so made, are seldom perfectly sound, in consequence of this action; while, in many cases, they are entirely ruined, and break on the trial trip. The same is true of steel, when drawn down in such work. I examined the steel shaft of the *Dolphin*, the United States steamer recently made famous by the unfortunate misunderstanding between Mr. Roach and the Navy Department, which has led us to so strongly sympathize with that enterprising builder of late. I found the section fractured, exhibiting most excellent structure near the surface; but it was very strongly crystalline, apparently, near the center. It had, I judge, been too heavy a job for the hammer available for forging it; the crystals had been produced while heating it, and had not been broken up in forging, in consequence of the fact, I have no doubt, that the effect of the blows could not reach the core. The tenacity of the outer part of the shaft was evidently fully up to standard; but the center had lost its strength very greatly. Steel requires a heavier hammer than iron, and I have little doubt that the same hammer could make a shaft of iron for that boat which would be sound and safe.

I received some years ago from the Navy Department some beautiful specimens illustrating this action, and I remember that I published illustrations of them, among other places, in my "Text-book of the Materials of Construction," in the chapter on "Conditions affecting Strength," and also in my "Materials of Engineering," Vol. 2, "Iron and Steel," under the same heading. One of these cases was the following: A hammer-head, made of steel, for a blow-pipe set, was left to anneal in a furnace, overnight, and presumably was at a red heat for hours. The next day, when taken out and cooled off, a slight tap with a hammer, ac-

cidentally given, broke it, and it exhibited such a perfect and beautiful crystallization, throughout the area fractured, that I had it enlarged and engraved, under the microscope, for the *Scientific American*, and also published it in the books to which I have just referred. It was the most perfect and beautifully complete crystallization, I think, that I ever saw. With these illustrations, I published also several obtained by the now familiar process of etching a cut surface to bring out the texture of the metal, as done by Dr. Sorby.

Whether such crystallization can be produced by jar, as in the action of the steam hammer upon its rod, illustrated in the paper before us, I am not so sure ; although I have a strong impression that it sometimes may. Where the stresses are insufficient to carry the particles beyond the elastic range, I presume it cannot occur ; and this seems to be indicated by the experiments of Fairbairn, and of Wöhler and Spangenberg. They found that, within a certain limit, millions of changes of form produced no apparent effect in change of structure ; while, beyond that limit, the number of deformations required to produce rupture rapidly decreased as their magnitude increased. What is the character of the fracture, when such breakage takes place, is the question in dispute. The fact of a change from the fibrous condition observed in the slow fracture, by a single stress, at ordinary temperatures, of good iron, to a different and either granular or crystalline fracture, is one of the most familiar facts, as I have already said, in engineering. But it is not settled whether such difference due to method of fracture is granulation or crystallization. This is the question still to be settled by properly conducted investigation. The fact that the same difference is produced in a number of ways is, perhaps, an evidence that it is granulation. If the same bar has piece after piece broken off it, by different methods of fracture, different kinds of breaks will be seen ; and it is easy to produce one or another kind at will. If good fibrous iron is broken slowly, it exhibits its characteristic fibrous structure ; if broken by a sharp, quick blow, the fracture is of the other kind. If broken at high or moderate temperature, the break may be perfectly fibrous ; while, if broken at zero, in precisely the same way in every other respect, the break will be like that of a stone or of a casting.

But it is not of so much consequence, perhaps, what we shall finally call this kind of fracture. The important fact is that it

may be produced wherever or whenever iron or steel is not handled aright, or properly proportioned to its work; the conditions producing it are well understood, and the well-informed and painstaking engineer will never be troubled by this kind of break in any ordinary situations. Some time ago, Mr. Wm. Metcalf stated that he had found the best material for hammer-rods to be a steel containing about 0.80 per cent. carbon. This corresponds very closely with my own work in the determination of qualities having maximum "elastic resilience," or shock-resisting power within the elastic limit.

It is perfectly obvious that, in cases in which the piece must necessarily meet with heavy blows, and especially with sharp, quick blows, the quality to be sought in any metal to be adopted in the construction is this of "elastic resilience." This quality is possessed in highest degree by steel of the character which I have just specified. The expedient to be adopted by the designing engineer, in such cases, is so to design and proportion the machine, of which this is a part, as to avoid, as far as possible, the production of such destructive action in its operation, to reduce the frequency and violence of shock to a minimum, to cause it to affect the most resilient parts of the structure and to distribute the stresses and strains, to dilute the shock, just as much as possible.

Prof. Egleston.—I have had some occasion to study these phenomena, and although this subject is somewhat obscure, perhaps some of the results of the researches I have been able to make at various times may throw some light upon it. We are very apt to think of iron and steel as a homogeneous material. It is, however, anything but homogeneous. If a mass of metal is built up of separate pieces and welded together, it is generally supposed that the piece made will be a homogeneous mass resulting from the welding by interpenetration of the different parts. I had occasion in 1868, as the civil member of the Board appointed by the United States to examine into the use of iron for fortifications, to make some of the investigations on the quality of the iron used in conjunction with brick and stone in the fortresses of the United States, with a view to strengthening them.

We found, in examining weld surfaces, that in a square foot there would not be one-third of it where there was any interpenetration. Over all the rest of the surface there was simply juxtaposition, with a thin coat of oxide of iron between. It is, I believe, not generally understood that in welding by interpene-

tration the most minute trace of oxide will prevent anything more than juxtaposition of the surfaces. In order to make up my mind exactly what was the effect of it, I had prepared by the Innerberger Steel Works, of Austria, in the year 1873, a series of steel specimens which were made by welding 24 ingots together. These ingots were reduced by hammering to one-fourth of their original size, and a section of the ingot so prepared was afterward polished. It appeared perfectly homogeneous, yet every individual ingot, as well as every deformation of the surface of each ingot, could be distinctly seen when the polished surface was etched with weak acid. Instead of being a homogeneous mass, it was simply a bundle of individual ingots, each one perfectly preserved, with all its good or bad qualities, without alteration except as to a little change of shape. Yet such ingots were exhibited and received a prize at the World's Fair at Vienna, in 1873, as the best samples of homogeneous steel.

There is another cause of a lack of homogeneity which occurs in many irons and steels, and that is blow-holes. I had occasion once to examine some 12 or 15 pieces of steel which appeared to be perfectly homogeneous; but on examining them with the microscope, I found them full of blow-holes, so regularly arranged around the section of the piece in the order they were compelled to move by the power exerted to force the bar into the shape into which it was manufactured, that it had every appearance of a rather bad quality of so-called fibrous iron. Yet the article passed current as homogeneous steel. If one of such blow-holes should come to the surface, no matter how minute it is, it will eventually propagate itself through as a crack. I also found that whenever iron which was supposed to be more or less homogeneous was submitted cold to shock, there was a tendency for the uncombined carbon to combine with the iron. If this was continued long enough, the crystals became large and the piece broke. When large masses of iron and steel are kept liquid for some time, there is a tendency for the metalloids contained in the iron to undergo what we call liquation, and settle from top to bottom and from sides to the center. It has been known for a long time that any alloy which contains one or two metals will also liquate when kept melted for any length of time. The components will separate according to gravity. It has been recently discovered that, under pressure, they will do the same cold. This phenomenon of liquation is all the more prominent as the mass of metal is larger. The

assumption has been made that small steel ingots will not liquate, while large ingots always will. To test this matter, I had several very small ingots made, and on analyzing the top and the bottom of each ingot, I found they were not of the same composition nor exactly of the same quality.

When liquid iron or steel used for castings which will weigh anywhere from 10 to 15, 25 or a 100 tons is cast, the liquation may take place to such an extent that there may be a difference of 0.25 per cent. in the carbon, which in some cases changes the metal entirely. The metal of all very large castings should be stirred before pouring. If they are to be of great thickness they should be made hollow and so cooled from both the inside and outside at the same rate. If this stirring was not done in making fine gold and silver, there would be a difference of several thousandths of fineness between the top and bottom of the melt. In castings generally, if the metal is cooled suddenly, the iron is white, and we say that the carbon is combined. If the same iron is cooled very slowly, it will be gray, and we say that part of the iron is combined and part uncombined. This method of expression is justified, because if a piece of hard white iron or tempered steel is dissolved in nitric acid, there will be no residue. If a piece of gray cast iron or untempered steel is treated in the same way, there will be a residue of carbon remaining. The combining of the carbon means simply that a crystallization has taken place. The more a uniformly fine-grained iron is shocked when cold, the larger the crystalline faces become. These faces are sometimes so large that a solution of continuity between the crystals takes place, and the metal breaks. I have a steel rail which was broken on the Northern Railway of France in 1872, where the fracture commenced in the foot, ran up through the web, then ran five inches and a half along the web, then horizontally along under the head, then through the head. That crack started in a blow-hole, but the fracture was caused by the crystallization produced by the constant shock of the wheels of the train falling from the top of the end of one rail to the rail below, or rising to the one above it, though the distance was less than one-eighth of an inch. The force of the blow depended, of course, on the weight on each wheel. When crystallization is being caused by shock, owing to the combination of the carbon, this tendency can be counteracted if the iron is treated before actual rupture takes place, either by heating or else by rest.*

* Trans. Am. Inst. Min. Engs., Vol. VIII., p. 398.

Now supposing that an ingot which is a very large one is being made (and I speak of this in this connection because it has recently happened in a large works in casting very large guns), and it is allowed to remain for a considerable length of time in a liquid condition. It was found in the special case to which I refer that the material on the outside cooled, while the material on the inside did not cool. There were consequently two sets of strains. The material was cold on the outside, and could not liquate; it was more or less liquid on the inside, and did liquate and become of a different composition there from the outside. Besides this, there was a set of strains on the outside different from those set up in the interior of the ingot. At the same time there was a tendency to crystallization, and the material was therefore not homogeneous. It is only necessary to keep the metal liquid a certain length of time, when there will be produced a double set of causes why that ingot should not be strong. If a recently cast but solid ingot remain for some time in its own heat—and this is the principal advantage derived from the soaking pits—then the metal will become uniform inside and out. This was the very reason why the large casting that I spoke of failed, that it was not heat-soaked, that is, of uniform temperature inside and out. If such an unsoaked ingot is worked, a series of strains is produced which tend to make the material crystalline and the metal is consequently brittle. No large piece of steel which has once been heated up should ever be allowed to get entirely cold; or if that cannot be prevented, it should be covered with coal and made to cool so slowly that it will be annealed throughout, that is, all parts will have cooled down alike, so that there will be no internal strains. If in making any part of a structure out of iron or steel, it has been necessary to bring any part up to a welding or forging temperature, the whole piece should be brought to the same temperature and slowly cooled, or, in other words, annealed. If this had always been done there would have been less loss of life and property from the giving away of bridges.

I may say, in this connection, that I recently had occasion to examine how much surface was in actual contact where a careful shrinkage joint is made, and found it to be only about one-third of it. In some cases it is much less. Many failures to make and separate shrinkage-joints are owing to improper soaking of the metals.

I have laid out and have commenced to make a number of in-

vestigations on these subjects, some of which I had hoped to have completed before this; but in the multitude of my other duties, the want of sufficient assistance and the various calls on my time have prevented my completing them. So far as I have been able to complete them, however, they show that equal pressures, equal temperature, and equal cooling are necessary in all parts of a large ingot to make reliable steel, and that pressure, not shock, should be used in producing their form. I have made a great many analyses over a long period of time in the study of these phenomena, and I am satisfied that jar is the great cause of fracture in most cases where the ingot is solid to commence with, and is caused by the combination of the carbon and the producing of large crystalline faces which are hard and smooth, and slip on each other with great ease, which is all the greater if the pieces have varying internal strains, have been welded and not annealed, or if the piece has been so worked in the fire that the temperature of the interior and exterior were not the same. The rupture will be all the quicker if in addition to other causes a blow-hole ever so minute comes to the surface. All of these are causes of subsequent rupture.

Mr. Hawkins.—I would like to say a word in reference to something in Prof. Thurston's remarks on this subject, where he said: "If the same bar has piece after piece broken off it by different methods of fracture, different kinds of break will be seen, and it is easy to produce one or another kind at will. If good fibrous iron is broken slowly, it exhibits its characteristic fibrous structure; if broken by a sharp, quick blow, the fracture is of the other kind. If broken at high or moderate temperature, the break may be perfectly fibrous; while if broken at zero, in precisely the same way in every other respect, the break will be like that of a stone or of a casting." I call to mind an experiment I made some years ago on a bar. The fracture showed a decidedly fibrous texture, except at a certain portion of it, perhaps occupying one-fourth of the area of the fracture, where the fracture was distinctly crystalline and very markedly so, and the limits of the crystalline portion were as distinctly marked as could be, and could be seen perfectly without any magnifying-glass. It occurred to me that possibly this crystalline portion would extend through the entire length of the bar; but I found, after cutting and breaking the bar into short pieces in the same manner that the first fracture was made—namely, by nicking them around with a blacksmith's cold chisel and breaking them

over the anvil with a sledge—that at every place there was a distinctly different fracture. In some places it was wholly fibrous, in other places of the mixed character, showing that this crystalline texture did not run through the bar, but existed in it in pockets. It was a rolled bar. This would go to show that the test given by Prof. Thurston would not be a reliable one, because from the same bar we would find different results under the same conditions, and the variations shown in his experiments might come from causes other than the stated changes in the conditions under which the fractures were made. I would like to say also, in reply to Mr. Hutton's remarks, that he seems willing to upset the generally accepted idea that rolled iron is generally and actually fibrous. I think we should not throw aside that view of the matter too suddenly, if indeed at all. Probably most engineers present know that, if a round rolled bar be turned and polished, and afterward treated with acid, it will show the distinct fibers running lengthwise on the surface of the bar. Then again, if we take a plate, such as nail plate, where it is rolled very much more in one direction than in the other, we all know that a narrow strip cut off crosswise of the plate, will have very much less strength than if cut from the plate in the direction of its length. So that I think we cannot throw aside the fact that we do have a fibrous structure in such bars that gives them a very decided value for strength in one direction over that in another.

Mr. Durfee.—One of the greatest difficulties we have in considering a matter of this kind is this: the way in which a mass of wrought iron is built up does not seem to be well understood, and the difference of its structure from that of a homogeneous material is not fully comprehended.

The term wrought iron is popularly supposed to designate a metal; but it is really the name of a mechanical admixture which, at its best, consists of clusters of crystals (which may be described as compound crystals) of pure iron separated from each other, as the result of the manipulative processes employed, by films or threads of an unavoidable impurity, called "cinder." In the manufacture of wrought iron, we first deprive the pig iron of carbon and other impurities by the puddling process.

This process may be briefly described as consisting of four distinct operations, viz.:

1st. The melting of the pig iron.

2d. The "boiling" of the melted metal in a bath of liquid cin-

der (composed mainly of silicate of protoxide of iron) until the iron begins to solidify in the form of small granules, or crystals, which can be seen moving amid the boiling cinder, like white-hot peas in a red-hot soup. When the iron begins thus to granulate, or crystallize, it is said to be "coming to nature."

3d. The collection by the puddler of these granules, or crystals, into distinct masses, which are called "balls;" these contain much cinder.

4th. The "squeezing," or "hammering," of these balls, while still at a welding heat, into more solid masses, which are called "blooms." These contain much less cinder and other impurities than the balls, but are far from being uniform in structure.

The balls above named may be described as white-hot sponges of iron saturated with liquid cinder, which fills all their accidental and irregular cavities. When the balls are hammered or squeezed for the purpose of expelling this cinder, and welding the granules or crystals of iron into a homogeneous mass, the operation is never wholly successful; for the cinder, as the metal cools, quickly assumes a pasty consistency, and flows with difficulty, and all that portion of it inclosed in the interior cavities of the ball is simply flattened out, or elongated. Hence it will be seen that the bloom is composed of a compacted mass of granules, or crystals of iron, separated from each other by films of cinder of very irregular thickness.

When speaking of crystals of iron, I mean minute ultimate units of that metal, which are bounded by well-defined planes, whose intersections always form salient angles. A number of these crystals may cohere and form an aggregation, having bounding planes similar in their outline and relative arrangement to those of any single crystal; such aggregations vary in size, and are often regarded as single crystals, and spoken of as such; just as we speak of crystals of galena and calc-spar, when, as a matter of fact, the ultimate crystal of each of these substances remains undiscovered, and as undiscoverable as the North Pole.

These large, or compound crystals of wrought iron are, in themselves, practically homogeneous; that is to say, the ultimate crystals of which they are composed are not separated and kept apart by any foreign substance, but are as nearly as possible in actual contact, as the law of cohesion, in obedience to which they are formed, will admit.

Now let us see how a bloom—which may, with propriety, be

described as the crudest form of a mass of wrought iron—differs in its structure from that of the homogeneous compound crystals of which it is built up.

Such a mass of wrought iron is an aggregation of an indefinite number of such compound crystals as have been described, separated from each other by films or threads of cinder of varying thickness; but which, notwithstanding, are mutually attracted with a greater or less degree of force, the minimum value of which is the measure of the cohesive strength of the mass.

When a properly heated bloom, or other similarly constituted mass of wrought iron, is subjected to the action of hammers or rolls, the contained cinder endeavors to escape from its entangling alliance with the crystals of the iron, and in so doing each particle thereof moves in the line of least resistance, which line is always located in a plane at right angles to the direction of the force acting upon the metal; that is to say, if the bloom is rolled or forged into a rod or bar, the metal will be acted upon in two directions at right angles with each other,* and its individual compound crystals will be compressed in directions normal to the exterior surfaces of the bar, and at the same time elongated in the direction of its length, thus forcing the ends of adjacent crystals towards each other, and compelling the intervening cinder to move at right angles with the axis of the bar, and unite with the films or threads of cinder which have become established in parallel lines of least resistance along the flanks of the crystals, and at right angles to the direction of the force acting upon the bar.

Fig. 133 is intended to illustrate on an exaggerated scale this arrangement of elongated compound crystals of iron with intervening films or threads of cinder, the light spaces representing the iron crystals and the dark spaces the cinder, the force of compression acting on the bar in the direction of the arrow.

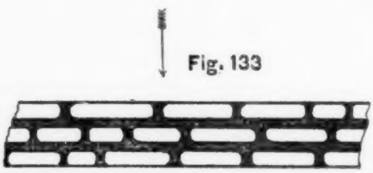


Fig. 133

Fig. 134 illustrates a method of showing by experiment the structural difference between a bar of wrought iron and one of a homogeneous material, such as low steel. In this figure, let *A*

* In forging a bar it is the usual practice to turn it about its axis through an angle of 90° between each blow, or series of blows; and in rolling a bar, it is usually turned through the same angle between each "pass" through the rolls.

be a vertical section of a cylinder provided with an accurately fitted plunger, P. The space B below this plunger we will suppose to be filled with bird shot, or preferably, fragments of lead of irregular dimensions, having their surfaces covered with a coating of oxide of lead. If sufficient force is now applied to the plunger P, the lead will be forced out of the hole in the lower end of the cylinder in the form of a rod, C, and every fragment of the lead will have become more or less elongated, but will be prevented from actual metallic contact with adjacent fragments by a film or thread of oxide of lead.

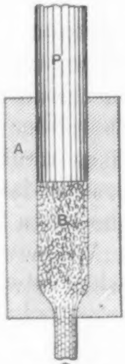


Fig. 134

In this experiment, the elongated fragments of lead correspond to the elongated compound crystal of wrought iron before named, and the oxide of lead occupies the same relative position in the rod of lead, as the cinder does in a bar of wrought iron. If now, in place of the fragments of lead, we place in the space B a solid mass of that metal, then, on applying adequate pressure to the plunger P, there will be forced through the hole in the bottom of the cylinder a rod of lead, whose structural difference from that of the former rod made from the oxide-covered fragments, is closely allied to that subsisting between a bar of low steel and one made cf the cinder-coated compound crystals of wrought iron.

The direct consequence of the elongation of the compound crystals and the effort of the cinder to escape in the direction of least resistance while the bloom is being forged or rolled as before described, is the establishment of that structural aggregation in the bar known as "fiber," which is one of the most conspicuous qualities of wrought iron, and one not found in any other form of ferruginous materials. The forging or rolling of a bloom into a rod or bar, involves, as before stated, the alternate operation of two forces acting in planes at right angles to each other, and necessarily results in the production of a "fibrous" or "stringy" arrangement of the cinder and elongated compound crystals, which together constitute the bar, and when any of these films or threads of cinder are so large as to be visible to the unassisted eye, they are called "sand seams" or "cinder cracks." If the compound crystals are nearly pure iron, the bar can be readily bent cold without fracture, and, if forcibly pulled asunder, its fibrous texture is at once evident to the eye; but in case the compound crystals have chemi-

cally combined with them some substance—such as phosphorus or silicon—which tends to diminish the cohesive attraction between the ultimate crystals of which they are composed, as well as the mutual attraction of the compound crystals for each other, then the bar cannot be easily bent cold without rupture, and is said to have a "crystalline fracture;" but, notwithstanding this appearance, the mechanical structure of the bar is the same as before; that is to say, the cinder and elongated compound crystals are still arranged in lines parallel with the axis of the bar, though it is quite probable that the length of the compound crystals may be much less than those in a bar such as the one first described.

Whenever a bloom is subjected to a force of compression which always acts perpendicular to the same plane—as is the case when it is rolled into a sheet or plate—the compound crystals and accompanying cinder are each flattened and extended parallel with that plane; and the resulting sheet or plate has more of a laminated than a fibrous structure; being built up of a number of leaves or strata of iron separated from each other by films of cinder; which, if at any point unduly thick, cause defects in the plate that are called "blisters."

We now come to a question which has been asked many times, and which is in effect repeated by the paper before us, viz.: Can a bar of wrought iron which is originally of a pronounced fibrous structure, be ruptured so as to exhibit a crystalline fracture?

I answer yes; in two ways, viz.:

1st. By a sudden application of a force of extension; commonly called a "jerk."

2d. By a prolonged repetition of a force of compression; sometimes called a "jar."

The first method of rupture has already been well described by Prof. Hutton. Briefly, it may be said to consist of a transverse division of the compound crystals of the bar, as distinguished from a sliding of their interlocking flanks upon each other, as is the case when the rupture presents a fibrous appearance. I have often seen crystalline fractures produced in truly fibrous iron. In the manufacture of iron rails (now a nearly extinct industry) it was always considered desirable that they should be of a hard and crystalline texture as to their tops or "heads," but soft and fibrous in their bottoms or "flanges;" but, however perfectly this distribution of metal was made, it was always possible to break such a rail so as to show a

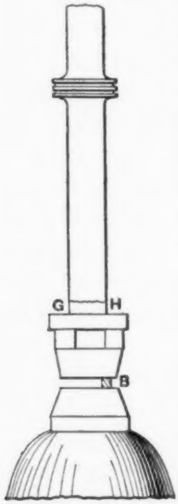
crystalline fracture in its flange; this was accomplished by making a slight nick across the flange and placing the rail (flange down) in the straightening press on supports placed a short distance on either side of the nick, and then putting in the gag "*heavy*," just over it; the result was almost always a crystalline fracture in the flange;—in short, the elongated compound crystals were "jerked" asunder. But, if the points supporting the rail were placed further apart, and the rail has an opportunity to yield slightly between them, then, if the gag was put in "*light*" a number of times in succession, the fracture of the flange would be sure to exhibit a fibrous texture; due to the fact that sufficient time had been given to break up the films of cinder along the flanks of the compound crystals and destroy their transverse cohesion, thus permitting them to slide apart and exhibit the appearance of disrupted fibers.

The section of a hammer bar before us is a good illustration of the second method of producing a crystalline fracture in fibrous iron, as it is the result of repeated action of a percussive force of compression.

As has been before stated, there exists in a bar of fibrous iron films of cinder, between the ends of its elongated compound crystals (as shown exaggerated in Fig. 133). These cannot, from the nature of their formative process, possibly be of uniform thickness, which, considered in connection with the fact that the greatest force of the percussive action per unit of area of any cross section of the hammer bar, is exerted upon a section made by a plane cutting the bar at right angles immediately above its head A B (Fig. 71), justifies the conclusion that at or near this plane fracture would be most likely to occur. It is also evident that the percussive action of the hammer would have more destructive effect upon thick than upon thin films of cinder, while at the same time the force of cohesion between the ends of adjacent compound crystals will be diminished in some inverse proportion to the thickness of the film of cinder between them. It therefore seems exceedingly probable that the fracture due to the continued percussion will take place in the plane above named, or in one very near to it, in which the cinder films chance to be of greater thickness. The particular point in the circumference of such a hammer bar where the imminent fracture first appears, is often determined by the manual peculiarity of the "hammer-man;" a left-handed man will incline to throw his work to the left, and a man who is right-handed will be

likely to use the right side of the anvil more than the left, in which case, as is evinced by Fig. 135, the work, B, will tend to produce a tensile strain at the point G, and therefore we should expect the initial manifestation of the fracture to be found at that point, and that it would gradually extend toward H, until, as a consequence of continued percussive compression, the bar was "jarred" asunder; the separation taking place through the films of cinder between the ends of the elongated compound crystals of the bar, thus exposing these ends and exhibiting what is called a crystalline fracture, as in the case under consideration. As long as workmen are right and left-handed there will be more or less of this one-sided work done; and, in fact, certain kinds of work demanded of steam-hammers necessarily involve this objectionable feature. I have in mind a hammer (having a bar similar to but much larger than the one described) which was used for "punching" and "bicking" tire blooms, at which all of the "bicking" was done on the right side of the anvil, and, after some years' use, a crack manifested itself on the left side of the bar in exactly the same relative position as that at D (Fig. 72) in the bar under consideration.

Fig. 135



Mr. Sanderson.—It is about two years ago that I inspected a large number of iron axles. The effect of concussion was a little different on the iron from the direct blow of the piston-rod on the hammer. I frequently carried a test after I had fulfilled the requirements called for by specification to the breaking point. Before commencing I tried to follow up a plan to find out something about the crystalline nature of iron. I found invariably under the points where the blow had been struck, a thin bar of bright crystalline iron about one-eighth of an inch thick; underneath that was a thin layer of granular iron, and underneath that was every kind of iron under the sun. I had hoped to find that in bending axles up and down sometimes as much as 13 inches, that the center of the axle would remain fibrous, and that the outside would be changed. But it didn't follow out my idea at all. Prof. Thurston, in his discussion, says such crystals can be produced by jar. I am not so sure of that. I think the matter of rest was mentioned in the secretary's discussion. The question came up once as to

whether the axle, which got very warm after 22 blows, would stand the test as well if it was allowed to cool off. I had two axles running exactly the same way which broke at the 24th or 25th blow; the third one I stopped at the 22d, and let it cool off. At the next blow after cooling it flew apart like broken crockery. The second axle, after receiving 24 or 25 blows, had a certain amount of granular fracture, mixed irregularly with crystals. This third axle was purely crystalline. It was probably an hour and a half cooling off, yet it stood 22 blows before it broke.

The President.—Did you ever make the experiment of annealing the axle under the process of testing it in this way?

Mr. Sanderson.—No, sir; I intended to do that after I went back, but I was changed off to other work.

Mr. Harrison.—I would like to ask where that ingot was cast which was referred to by Prof. Egleston.

Prof. Egleston.—I do not care to say, because it was a failure. No publication of the matter has been made.

Mr. Wm. O. Webber.—I would like to confirm what Mr. Sanderson has said about the matter of rest after dropping axles. I made a great many experiments some four or five years ago testing axles in that manner, and found that, *invariably after letting the axle cool off*, after a dozen or more drops, it would break on the first blow; that is, with iron axles. But on steel axles, of which I tried a great many, I found it made no difference; at least I never could break one on the first two or three blows after letting the axle get entirely cooled off.

The President.—I look upon this question as being one of very great importance to mechanical engineers, and it involves in it some other questions which are of equal importance, and to which the mechanical engineers of this country will have to give their attention very soon. One of the questions growing out of this, is the influence which will be exerted upon steel, cast in moulds or ingots, by its being hammered. We have had some notable instances of hammered steel shafts breaking, and I have noticed references here, in which the fracture of steel structures has been mentioned. It is important for the Society to investigate, and ascertain, if possible, to what extent a steel casting or a steel ingot will be improved or damaged by its being afterward heated and hammered. There are certainly some curious things which have happened in connection with this, and it is a very important question, because some of our largest structures are now made of

steel castings, or steel ingots. Personally, I am inclined to think (perhaps it is a very bold thing to say), that we have had no instance in this country where a first-class steel casting has been benefited—I mean in a large structure, of course—by its being afterward hammered. There is something very peculiar in the behavior of steel in this respect, as compared with wrought iron. There is a tendency, as is known to every one here, in hammering any metal, to produce a flow of the metal on the outside first. This is particularly true of steel as compared with wrought iron. Every blow which is struck upon a steel bar seems to move the outside of it most, no matter whether it is a large bar or a small one. This will occur to many of you who have undertaken to sharpen a cold chisel; after you have hammered it down it is often cupped on the end, and you have to cut the point off. One would suppose that an article as thin as a cold chisel, that the penetration of the blow would drive the center along with the outside, but practice shows that the outside of a steel forging or bar will move while the center remains stationary. Or, what is worse, in a large bar, or shaft, it often does not remain stationary, but actually pulls apart, as has been shown in many instances, when the forging has been cut asunder, lengthwise, or has afterward been broken. This is important for you to consider now, and in the future, whether we do not damage large masses of steel by undertaking to hammer them on the outside, especially after they have been allowed to become cold and again reheated.

Mr. Durfee.—Your remark about steel drawing on the outside calls to mind a fact which has unfortunately come to my notice on several occasions. If a large, cold ingot is put into a furnace and suddenly heated, it will expand much faster on its outside than at and near its axis; and the result will be a pulling apart of the center of the ingot as at A, Fig. 136. This breach of central continuity may in some cases have a diameter equal to half that of the ingot.

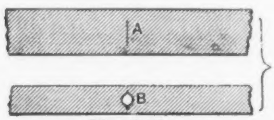


Fig. 136

An ingot thus internally fractured, if hammered or rolled down to a smaller section, will have a cavity developed in the center of its mass, as shown at B, Fig. 136, and unless you are fortunate enough to work upon the ingot until you discover the existence of this cavity, serious difficulty may result from its use as a part of any mechanism. Many of the so-called "blow-holes" which are discovered in broken steel

forgings and shafting, etc., are only cavities resulting as above described from improper heating of the ingot from which they were made, and this accident which I have described occurs a great deal oftener than people who forge iron and steel are disposed to admit or even believe.

Prof. Egleston.—The matter which has been just brought up by the President is perhaps the most important matter in the whole metallurgy of steel. It wants a discussion of several days, and not a discussion confined to minutes. The first way in which the difficulty can be gotten over is to have no center to your steel. Take the center out; then the structure would be a great deal stronger, and have a great deal less dead weight. The other way is not to hammer it; and I may say here that in large masses hammering is certainly the worst thing to do. It would almost be better to sprinkle sulphur over your work, or do almost anything else to it than to hammer it with very heavy hammers, particularly if in large masses the outside and inside are not of the same temperature. If they have been soaked, that is, allowed to remain in a furnace until both the outside and the inside are at exactly the same temperature, then perhaps they may be hammered with safety, but there will always be danger in hammering. The difficulty may be overcome either by taking out the center or by pressing. There is in no human engineering a disposition of the material in any way comparable to that which is seen in a rye straw. Why not imitate it? I have seen a shaft 56 feet long with a hole 18 inches through the center pressed up so true that when $\frac{1}{16}$ of an inch was taken off the outside, it was ready for use. The ingot was cast hollow, and it was pressed out on a mandrel, and that is I think what will have to be done with the large steel ingots in the very near future. I have no hesitation about it. I am glad to say that I have seen two or three 80-ton hammers in the scrap heap. The abandonment of the hammer and the use of the press for structural steel is what I think we shall all have to come to.

I have recently had occasion to examine a forged crank pin, made with great care from the best open hearth steel. It was rough turned to $16\frac{3}{8}$ inches. To ascertain its quality in the center an inch and a half hole was bored through it. This hole revealed such a number of cracks and cavities that the hole was increased to four inches, in the hopes of cutting them out. Defects of considerable size were still found. The pin was then sawn into, where single horizontal cracks, 10 inches in diameter and $\frac{3}{8}$ inch wide

were found, and inclined ones $7\frac{1}{2}$ inches long, in which there were cavities $\frac{1}{2}$ an inch wide, to say nothing of defects of minor importance. None of these could have been revealed but for the forethought of examining the center of the piece. If it had been used without this examination it would have produced great disaster. If it had been cast hollow and forged on a mandrel they would never have occurred.*

With regard to fibrous iron, there is no such thing. It is an appearance, not a quality. Etching with acid does not prove the fibrous structure, since all iron contains a considerable amount of slag easily soluble in acids. This in a rolled bar will be distributed more or less uniformly in the direction in which the bar has been rolled, and when acted on by acid will be eaten out in more or less parallel layers from the outside. When the action is continued, this appearance of parallelism disappears as it is only superficial. If the same iron is submitted in a tube to a current of chlorine gas the whole of the iron will be dissolved out and a mass of exactly the same shape as the iron will be left behind, which is exceedingly light and porous, which is slag. If this is examined it will be seen to have a sort of pseudo-laminated structure running through its mass, which brings long strings of it to the outside of the iron, giving the pseudo-fibrous appearance to the piece when it is etched. It is the eating out of the slag which gives the fibrous appearance. If the end of any fractured bar which has the pseudo-fibrous structure is examined with a glass, each so-called fiber will be seen to be the face of a crystal. It is the drawing out of the ends of these crystals which produces the change of color in the mass which gives the pseudo-fibrous appearance. If the surface was highly magnified there would be no fibrous appearance. I have had occasion elsewhere to call attention to another of these metallurgical optical delusions,† and there are many of them.

Mr. Kent.—In corroboration of what has been said by Prof. Egleston, I would mention an instance told me by Mr. Zimmermann, of Pittsburgh. He said that at one time he had occasion to inspect a lot of steel for bridges, which was cast in small pieces, which were hammered into smaller billets and rolled into a test piece. The effect of the hammering of these billets was such as to make all the difference between acceptance and rejection. In

* School of Mines Quarterly, Vol. VI., p. 216.

† Trans. Am. Inst. Mining Engineers, Vol. XII., p. 385.

regard to the stretch of steel on the outside and breaking in the middle, I have seen repeated instances of it in a certain lot of very bad steel which was heated in a furnace and passed through cogging rolls. It pulled apart in the middle and stretched on the outside, so that you could see daylight right through it. I have had repeated instances of breaking in the middle without breaking the edges in testing boiler plate in a testing-machine, in which the strain was applied by means of screws. Once in testing a piece of round steel and measuring the stretch very carefully with an electric contact micrometer and plotting the results, I found a sudden change in the readings of the micrometer. Continuing the tests I found the modulus of elasticity was lower. I made the statement then that the piece was broken in the center, although there was no external evidence of the break. We discontinued the test, I think, at midnight, and broke it the next day, and found plainly a flaw in the middle of the fracture. I reported that test to the American Institute of Mining Engineers some six years ago.

Mr. Sanderson.—I would like to mention one little thing more. I noticed when testing mild steel axles that, after the first blow, the deflection would decrease and the axle stiffen up, then give out gradually until it broke. Now is any change going on in the axle during that stiffening process? I would like some member to tell me something about that if he can.

Mr. Durfee.—There is one property of the structure of metals, which has not been mentioned here, and that is the increase of strength due to properly manipulating them. I have no doubt at all that that observed increase of the tensile strength is caused in the case of iron by the reduction of thickness of the film of cinder which separates the particles of iron from each other, so that the attraction of cohesion (whatever that may be) has less space to act through. It is well to remember, when we talk about less space in a matter of this kind, that we are dealing almost with the infinitesimal.

The President.—It is very well known that re-working wrought iron, which means as far as possible the exclusion of the cinder from it, or, in other words, its refinement, always adds to its tensile strength. The question is whether, as Mr. Durfee suggests, this cinder is attenuated more, and made much less in thickness in being drawn, so that the fibers of the iron come closer together. Of course it adds, as we all know, to the tensile strength of the material. This also is a question, gentlemen, which is going to meet you in the near future.

Mr. Stephen Nicholson.—In connection with this subject of hammering steel I have noticed, in the working of small pieces (say from $2 \times \frac{1}{2}$ inches and under), first, that steel may be hammered in such a way that, after a number of blows, ranging, perhaps, from 12 to more, it becomes so crystallized and brittle, as to break entirely. Secondly, and in opposition to this, I have seen under a hammer a forger take a cold piece of steel and hammer it to white heat, draw it out and shape it nicely, and the steel worked very well—as well as when heated in the ordinary way. I mention these as two directly opposite results obtained both from hammering.

I am sorry to say, Mr. President, that in your mention about the sharpening of the cold chisel, I have observed, perhaps every day, that the steel works very differently. Instead of forging out hollow, or cupping, it turns out rounded. I have observed, further, that steel worked above a certain temperature, say, perhaps, a bright red heat, will lengthen out and shape itself with a great deal of ease, but that below that heat very poor results can be obtained in changing the dimensions of the steel. The surfaces can be made flat or equalized, but very little effect can be had in forming or drawing.

The President.—Those of you who have observed the action of a large ingot or bloom under the hammer, will have seen, I think, that it is nearly always cupped at the end. Perhaps in the particular instance to which the gentleman alludes I am not correct.

Prof. Sweet.—I would like to remind the members of the fact that the most of the Stubs' wire which we buy at the present time is made by hammering rods of steel from an inch and a quarter perhaps down to $\frac{1}{16}$ of an inch, or less, cold, the rod being reduced about $\frac{1}{16}$ at each operation, and every reduction makes it stronger. It seems to me this is one argument which offsets the one that steel is ruined by being hammered. My own impression is that you are mistaken, Mr. President, about steel ingots always cupping at the end. I believe that if the hammer is large enough in hammering an ingot three or four inches in diameter, the ends of the ingot will round out. They use that as a gauge to tell whether the hammer is as large as it ought to be for a certain ingot or not. If the hammer is too small the end is sure to be cupped.

Mr. Hill.—We make small steel ingots about 4 inches in diameter. They are then hammered out under a steam hammer, and the ends always round out.

Mr. Durfee.—There is another article that is manufactured by

cold hammering, and that is sewing-machine needles. All sewing-machine needles are now reduced from the wire. The wire is cut off at a proper length, and put into a mechanism that subjects it to a very rapid, but very light hammering, and the result is that the metal is reduced in diameter and elongated, and the needle resulting is really better than that made by the old system.

Mr. Stetson.—I wish to ask whether the Stubs' steel wire was made in this country which was made by the hammering process? I am led to believe that the Stubs' wire which is usually brought to this country is drawn through draw-plates, and I guess that the Stubs' steel wire which is made in this country is made by hammering.

There is one set of men who never will be converted to the idea that iron does not granulate, and those men are hammersmiths. Wherever you fasten iron heads on the wooden helves of trip-hammers, after a while the bolts will break and appear to be crystallized, and that crystallization depends very much on the torsional strain of the nut. I have two workmen, one of whom seems to break the bolts every day, and the other not nearly as often. One fastens them with a long wrench tight, and the other leaves them as slack as possible, and this matter of the fatigue of the metal seems to come in there in the bolt of the man who keeps that portion over-strained. That set of men will always believe that iron does change its nature from fibrous to crystalline structure.

Prof. Egleston.—We have been discussing the effects of hammering steel in large ingots when the temperature of the inside and outside is not the same. The hammering of steel wire is a different matter. It is done on small sizes at very great velocity of blow all around the diameter of the wire, and has exactly the same effect of compression as the draw plate. It gives and should give a close grained and high quality of steel wire, much better than that which is drawn, but this is a very different thing from hammering a large ingot the interior of which is at several temperatures, all of them different from that of the outside. Such hammering weakens large pieces of steel and leaves them with so many interior strains and so deteriorates the material that it cannot be depended on. All large masses of steel, after they have been wrought up to a given temperature shown by color on the outside, should be left so long in the furnace that it will be certain that the heat is exactly the same on the inside and outside, and then they may be properly and safely worked.

Mr. Oberlin Smith.—In dealing with this question I think we ought to consider whether these troubles would occur in large ingots if the hammers used with them were as large and heavy, in proportion to the ingots, as are the hammers that pound out the little bars of tool steel and the sewing-machine needles, leaving blowholes out of the question. Would these cracks in the interior be started if these large hammers were immense enough to bear the same proportion to the large ingots as the small hammers do to the small bars.

Mr. Crouthers.—An experiment was made not a great while ago which would seem to corroborate Prof. Egleston's idea. The test which I speak of was made on a buggy-axle, the cross-section of which was an inch. It was suspended between centers, and 16 blows of a certain weight of drop broke it by reversing it each time of its drop. The mate of this axle was taken and put through a process of conversion. We coated that axle with a quarter of an inch of steel, and instead of requiring 16 blows to break it, it required 24. We welded the first axle together again, and covered it with another coat of steel an eighth of an inch thick. That we hardened as hard as fire and water could make it. That required 20 blows of the hammer to break it. We went through a series of experiments of different kinds with railroad iron, and with a car axle, with pieces of the commonest kind of iron we could pick out anywhere, showing them to be a rank of crystals—a whole regiment of them. By covering them with this process of steel covering, we drove all those crystals out of existence. We coated them with a coat of steel from a thirty-second of an inch to half an inch thick. Each time we found less of the impurities in them. Dissatisfied with the results there, we tried another brand, and got about the same effect there, increasing the torsional strain 40 per cent. and the tensile strain 60 per cent.; coating them and hardening them, and then bending them into any shape we might desire, and still they were hard enough to cut ordinary steel.

The President.—Please explain the mode of coating.

Mr. Crouthers.—I beg your pardon, but as yet I have to decline. I have also some cutter knives which were forged, hardened and tempered, and twisted after. If it is going to ruin steel to hammer it, we will have to use iron. We can hammer iron, we know, and improve it by hammering, and then turn round and make steel of it.

Mr. Harrison.—I would like to ask Mr. Durfee in regard to the

hammering of those needles—whether it is done with one operation, and also about the carbon.

Mr. Durfee.—I have seen great quantities of it done; the reduction is a gradual one. The end of the wire is inserted between dies having conical cavities in their faces, and gradually travels in, and as it goes, the metal is gradually reduced. It is left with a perfectly smooth polished surface.

Mr. Harrison.—How hard is the steel?

Mr. Durfee.—I cannot say just what the percentage of carbon is.

Mr. Stephen Nicholson.—A gentleman inquired about the fine American wire used in place of Stubs'. I have seen the wire made, which probably ranks highest in the market. It was made by a Pittsburgh firm, and is made under a similar process to that described by Mr. Durfee. I think the same machines which are used in making the needle are patented and used in both places. The percentage of carbon which can be treated I do not think is material. I have seen wire which was perhaps three-eighths of an inch in diameter tapered from three-eighths down to a sixteenth of an inch. Another sort of wire which in this country sells in place of Stubs' wire is drawn, and so far as I have seen, it works equally as well as that hammered, cold or otherwise. I do not think that any of the fine wire sold to-day in America is made hot. It is all finished cold.

Mr. Durfee.—This so-called crystallization of iron as the result of prolonged use, I think is a mistake altogether. I think that the crystals existed there just as we see them at the time the metal was put into the form in which it was fractured.

A number of processions of these crystals were, by the processes of manufacture, formed parallel with the axis of the bar, and these were separated from each other by films or threads of cinder, and the individual compound crystals in each procession were likewise separated by films of cinder; in other words, the bar had a structure similar to that resulting from the compression of the fragments of oxide-covered lead before named.

Now, after a bar subjected to sudden "jerks" or the "jars" of percussive compression has been shaken up, in some cross section in it having a slightly thicker film of cinder between the ends of adjacent crystals of iron, that film of cinder finally gets broken up and the cohesion is destroyed, and the result is what we see there in that fragment of a hammer bar. Those crystals existed in that bar, I haven't the slightest doubt, when it was made, but

they were separated from the rest of the bar by a film of cinder, and the result of the shock was to break that up and so rupture the bar.

In speaking of films of cinder I am talking of very thin substances, but I have no doubt but that these existed when the bar was forged, and separated as I have supposed, the ends of the elongated compound crystals, which are those we now see in the fragment on the table.

The President.—Is it not true that iron crystallized in the manner which you speak of may be made very much tougher by reheating? If that is so, how do you account for it?

Mr. Durfee.—I account for it in this way, that in the original heating and shaping of the forging its crystals had a comparatively thick film of cinder between them, but when it is reworked those crystals are driven up into closer order, some of the cinder is expelled, and what remained is very much reduced in thickness and the cohesive attraction between the crystals having less space to act through, acts with augmented intensity.

Mr. Bayles.—Interesting and instructive as this discussion has been, it seems to me to have touched very lightly and only incidentally the question presented in the paper of Mr. Hill. The facts to which he calls attention are interesting, but not surprising, similar phenomena having been encountered in the experience of every engineer who has carefully observed the behavior of iron under various conditions of shock and stress. The fracture described presents a crystalline appearance. Mr. Hill considers it more distinctly crystalline than pig iron. The accuracy of this comparison will, I think, be found to depend upon what kind of pig iron is taken as the standard. The object of Mr. Hill's paper, as I understand it, was to invite a discussion of the question whether the shocks of use had produced in the metal structural changes of the kind necessary for the transformation of fibers into crystals. My opinion on this subject was formed long ago, and has since been strengthened by observation and experiment. Among the first questions which I was called upon to discuss under conditions imposing a professional responsibility, was this very question of the crystallization of wrought iron. While seeking evidence on the subject I was invited by the manager of a rolling mill in Pennsylvania to witness some simple experiments, and from these I learned a great deal. In the blacksmith shop attached to the mill a number of test pieces of high quality mer-

chant bar were tested. These pieces were nicked and turned over the horn of the anvil. They developed a fracture more like that of seasoned hickory wood than anything else to which I can compare it. The metal tore open with long, silky, fibrous fracture, showing a quality as good as had ever been attained in iron making up to that time. Six inches further along the same bar a second nick was made, and, without any apparent difference in the manner of striking it, the iron was broken short off, showing a structure so apparently crystalline that one might imagine it was anything but good wrought iron. I found upon investigation that this was simply a blacksmith's trick, and that such results could be produced at pleasure, the character of the fracture depending entirely upon the manner in which the metal was struck after nicking. At first I was somewhat skeptical on this point, deeming it probable that the shocks and stresses of tearing the fiber apart in the first test had produced certain structural changes which accounted for the appearance of the fracture occasioned in breaking it short off. This, however, was disproved in a very few minutes, it being as easy for the blacksmith to produce the crystalline fracture first and fibrous fracture afterward, as to reverse the order. These facts have been touched upon by some of the gentlemen who have already spoken, but they do not seem to have given the fact that a piece of new iron can show both a fibrous and crystalline structure within a space of two or three inches, as much weight as it seems to me entitled to as evidence with regard to the cold crystallization of iron.

From such study of iron structures as I have had opportunity of making, I have reached the conclusion that talking about the crystallization of iron is much the same as talking about the crystallization of sugar and salt. It cannot pass from the plastic to the solid state in any other way than by crystallization. Whatever we may be able to do to it in the way of shop manipulation, we cannot give it a structure other than crystalline. In rolling iron important structural changes are produced; crystals are more or less distorted and are so displaced that they form, with the aid of the cinder, what we commonly call the fibrous structure. The crystals remain, however, and I have never seen a piece of iron polished and etched with weak acid in which a well-defined crystalline structure was not distinctly visible. If, therefore, we find in iron which is broken, an apparently crystalline structure, there is no occasion for surprise. The question is not whether it has

become crystalline as the result of shocks or stresses, but what changes have been produced by these means which render the metal more brittle than when first wrought into form. This is a question quite separate and distinct from that presented at present for discussion. No fact has ever been brought to my notice which has seemed to me to warrant the conclusion that any crystals had developed in cold iron which were not there before it had cooled.

CXCVIII.

EXPERIMENTS ON THE TRANSMISSION OF POWER BY GEARING.

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THE idea of determining by experiment the relative values of different forms of gearing as measured by the economy with which they transmitted power originated with Mr. J. Sellers Bancroft in the spring of 1883, and soon after the following investigation was begun at his instance and carried out under his direction. The question implied was one of continual recurrence upon which diverse opinions were found to exist, and these were so frequently based upon general impressions or loose observations, in which some important fact or condition was wanting, that the necessity was felt for more definite and reliable information, as a guide in the construction of high class machinery. To obtain this in such a manner as to leave no room for doubt, it was proposed that the gearing itself should be made to bear testimony, and the writer accordingly undertook to prepare the design for an apparatus which should thoroughly test the efficiency of the more common forms of gearing, namely, worm, spiral and spur.

With what success our efforts were attended in the design and execution of the work will appear in the sequel, but at the outset we imagined our apparatus to be well-nigh perfect and almost incapable of showing anything but the truth.

The principal requirements in the problem presented, were a dynamometer to measure the power received by the worm or pinion shaft and a brake to measure the power delivered to the wheel shaft. These two elements in their proper relation to the gearing to be tested, constituted the apparatus shown in plan by Fig. 87, and in elevation by Figs. 88 and 89.

The chief difficulty in the perfection of the scheme lay in the

construction of a dynamometer which should not have the errors and defects common to such machines.

We required in the first place that it should be readable at any

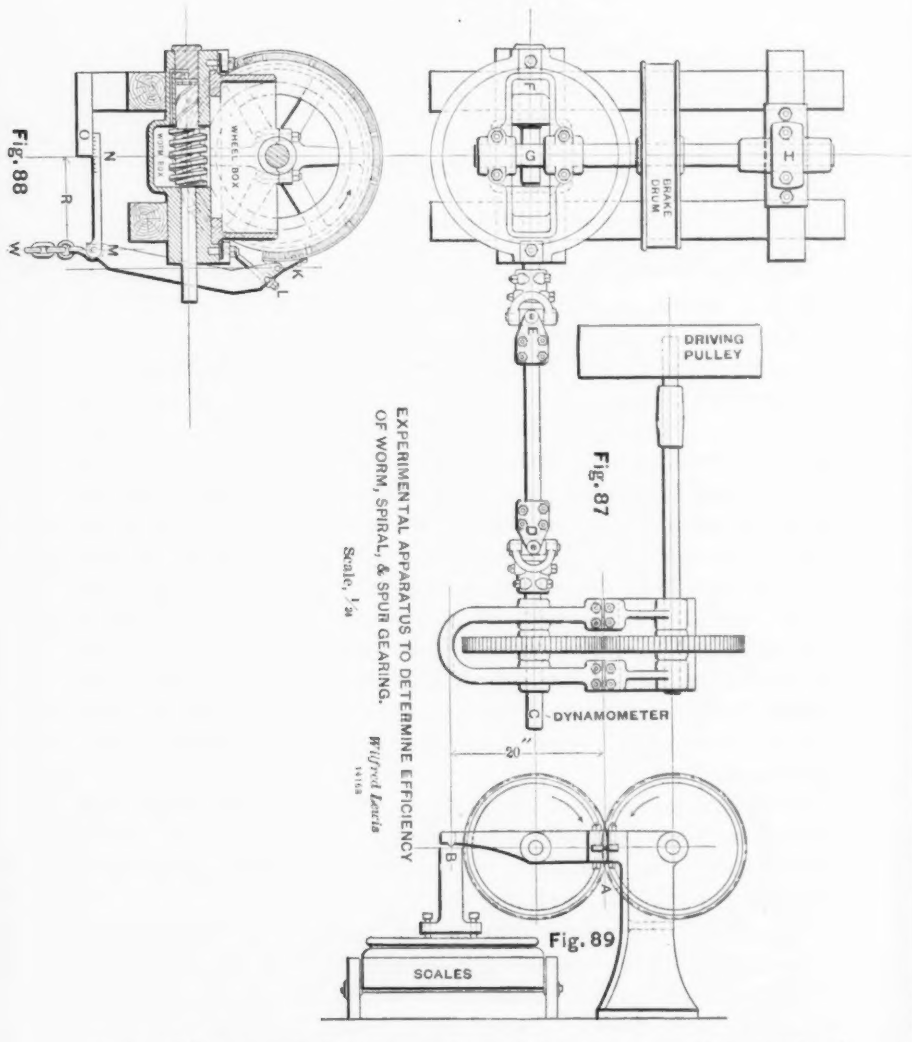

Fig. 88

Fig. 87

Fig. 89

EXPERIMENTAL APPARATUS TO DETERMINE EFFICIENCY OF WORM, SPIRAL, & SPUR GEARING.

Scale, 1/24

time, with as much delicacy and precision as an ordinary pair of scales, and, in the second place, that its own resistance should, if possible, be eliminated from its readings, without the necessity of making guess-work corrections.

An experimental dynamometer similar to the one represented was built at the suggestion of Mr. Bancroft, and tested with encouraging results; but, instead of the universal joints, he used simply a straight shaft and depended upon its flexibility, and instead of the frame hinged at A, two separate frames were used and the shafts held at a fixed distance apart by means of links.

From a practical point of view, this might have been considered a success, but, theoretically, it was subject to an error arising from the friction of the driven shaft in its bearings. When properly lubricated this friction would be small, but from carelessness in oiling, it might well be worth considering, and the arrangement shown in Fig. 87 was adopted as an improvement. Here it will be seen that the frame which carries the driven wheel is supported by a flexible joint at the line of contact A, and again at B, by portable platform scales.

From this construction it follows that no matter how great the friction in the journals of the shaft C D may be, there will be no pressure at the point B, except what results from torsion in the shaft D E.*

To demonstrate this point, let us imagine the driven wheel firmly clamped to its frame, so that for all intents and purposes it may be considered as part of the lever A B. Now it is evident that a downward force acting on this lever at any point to the right or left of A, will produce at B a reduction or increase of pressure proportional to the distance of the force applied from A. And it is likewise apparent, when a force acts directly in line with A, that no portion of it can be communicated to B. When this principle is clearly understood, it can readily be seen that journal friction is nothing more than a partial clamping of the wheel to its frame P. A thorough test of the accuracy of this construction was made by attaching to the shaft C a brake in connection with the frame A B. When the wheels were set in motion this brake represented a magnified case of journal friction, and no matter how tightly it was drawn, the reading of the scales remained unaltered.

As a still further refinement of this dynamometer, the attempt was made to avoid the use of gear teeth altogether and drive by the contact of flat-faced wheels, but the great pressure required to drive caused the journals to heat rapidly, and the idea

* This principle is the same which was used by Mr. Tatham in his belt dynamometer.

was soon abandoned. Otherwise the method was satisfactory, and the truth of the principle just demonstrated was repeatedly shown by the fact that the reading of the scales could not be altered by any change in the clamping pressure on these discs.

To measure the power transmitted it was important to know the effective length of the lever arm A B, and this was carefully tested in the following manner and found to agree with actual measurement.

The universal joints were disconnected, the driving pulley was blocked and a weighted lever attached to the shaft C; the scales were then balanced, and the weight was moved out upon the lever until the reading of the scales was increased by an amount equal to the weight moved. The distance through which the weight had to move was then taken to be the effective length of the lever arm of the dynamometer.

The brake used to measure the power transmitted was designed to maintain an approximately constant load of any desired amount. It consisted of a drum or pulley encircled by a Napier brake of rather novel construction, in which the load itself was made to adjust automatically the tension on the strap. By this means there were no extraneous forces involved and no corrections to be made, and the error usually arising from the use of an auxiliary tightening lever was avoided.

By reference to the drawing it will be seen that, for any given load suspended at the point M, the tension on the strap at L depends upon the horizontal distance of M from K, and that the moment of resistance is measured by the horizontal distance R of the point M from the center of the drum.

As a precaution against excessive heating during the progress of experiments, this brake drum was provided with deep internal flanges forming a trough through which water could be made to pass by means of a siphon.

The gearing to be tested is represented on the drawing as a worm and wheel, but the housings which carry their journals are adjustable, so that the brake shaft can be set obliquely to the worm shaft for spiral gearing or parallel to it for spur gearing.

The worm box was made very large, to insure an abundance of oil and dissipate as fast as possible the heat of friction. It also acted as a reservoir for the lubrication of the journal and step-bearings, and as a bath in which a thermometer could be kept to note the temperature from time to time.

Power was received by a belt to the driving pulley from an in-

dependent engine, the speed of which was under the control of the experimenter. In preparing for experiments, the shafts C D and E F were set in line, or nearly so, and the edge of the block O adjusted to be directly under the center of the drum shaft. Then, with a known weight attached to the chain from M, the worm box filled with lard oil and the apparatus set in motion, the experiments were ready to begin.

By means of the nut L, the position of the weight was adjusted until the graduated bar M N, resting upon O and M, was brought to a horizontal position. Then the revolutions per minute were counted, the temperature noted, and the reading of the dynamometer observed, together with the distance R on the bar M N.

In commencing to make the experiments, several unexpected difficulties were encountered, which for a time vitiated the results to an unknown extent. The experiments thus affected, however, were afterward repeated so as to exclude any errors which might have crept in unobserved.

At moderate speeds everything ran smoothly, but as the speed was increased, the step bearing for the worm shaft began to give trouble. This bearing was formed by the contact of two hardened steel discs carefully ground, and although well lubricated by a circulation of oil, the danger of cutting at high speed was to be constantly apprehended.

The delay caused in this way was very annoying, and after a number of futile efforts to lengthen the life of the bearing, the experiment was finally made of introducing a loose washer of hard brass between the hardened steel faces.

This remedy proved to be effectual, and no further evidence of cutting at this point was detected for any combination of speed and pressure.

The brake strap was also at first another source of annoyance. It was made as shown by the drawing of a flat iron strap lined with hard-wood blocks on end grain, and for a time it worked well, although not quite as steadily and smoothly as could be desired.

There appeared to be considerable variation in the friction of these blocks against the drum, and the weight suspended was in consequence thrown into oscillations, which had to be checked before the distance R could be properly measured.

To check these oscillations it was simply necessary to press the bar M N against its bearing, until they were absorbed by the friction thereby produced, and so long as a slight amount of oscilla-

tion remained, it was considered as evidence that this friction did not affect the mean radius to be determined. Still there were variations in the amplitude of the vibrations which could not be altogether controlled in this way, and for the purpose of improving its action the brake strap was lubricated with a mixture of tallow and oil, which at once produced the most alarming results. The load was thrown into the most violent agitation, and the apparatus shaken in a manner which threatened to break it down. For this strange phenomenon no satisfactory explanation could at first be given. The apparatus was strengthened and braced in various ways, sometimes with an improvement in its general working, but oftener without avail. The vibrations were at times very violent, and again entirely absent under apparently the same conditions. Sometimes they increased in violence with the speed, and sometimes they diminished, and finally disappeared.

At or above a speed of 180 revolutions per minute of worm-shaft which corresponded to a surface velocity of about 60 feet per minute at brake surface, these vibrations seldom occurred; but as the wheel slowed down they would almost invariably appear before coming to a full stop. Once they occurred at a speed of 348 revolutions after the wheel had become hot enough to melt the tallow, and their recurrence at this speed was prevented by keeping the wheel cool with water.

At another time, when running at a speed of 70 revolutions with the same load, the vibrations continued for several hours, and were finally checked by allowing the wheel to warm up. The speed was then reduced to 3 revolutions, at which it ran without shake or jar, but five minutes later, upon starting up, the vibrations were as severe as ever and could not be made to disappear, even at a speed of 70 revolutions.

The wooden blocks were then scraped and washed with benzine to remove all grease, with, it was thought, some improvement, although at slow speeds the vibrations still continued.

In general, it was found that slow speeds and heavy weights had the greatest tendency to produce vibrations, and that the heavier the load the greater the speed necessary to check them. At slow speeds their amplitude was greatest and their number the least, while as the speed increased they became shorter and more rapid, producing a higher and higher tone until they finally ceased.

The phenomena presented were so strangely contradictory that

for a time they seemed to defy explanation, and seriously to threaten the success of the undertaking.

The only plausible theory upon which they could be accounted for appeared to be, that the frictional surfaces were in an unstable condition, caused by the difference between friction of rest and friction of motion.

With most substances the friction of motion is less than the friction of rest, and it was argued, that by reason of the friction of rest, the brake strap would be carried beyond the point where it belonged, to be in equilibrium with the friction of motion, and when sliding occurred, it would fall back and grip the wheel with such force as to stop all sliding and again produce friction of rest.

Having failed to secure a satisfactory brake surface of wood, it was decided to try another material, and from the view of the case just presented, leather suggested itself as the most suitable substance, on account of its peculiar frictional properties, which make it an exception to the general rule, that friction of motion is less than friction of rest.

Accordingly, each block of wood was covered with a leather face, and all further difficulties of this kind were effectually checked.

During the long series of experiments which were afterward made, a slight tremor was sometimes noticed when the wheel became hot enough to dry up the leather, but at such times the original condition was easily restored by the application of a moderate amount of belt grease.

Too much of the lubricant was found to be injurious, and no more was needed than could be absorbed by the leather. It was also noticed, that on account of the increase in friction of the leather over the wooden surface, the brake was more easily adjusted, and that the weight suspended from it hung, in consequence, farther from the center of the drum shaft.

It could hardly be expected that a weight suspended in this manner from a rotating wheel would remain motionless, and very naturally it was found that oscillations of gradually increasing amplitude were to be constantly contended with. The disturbing cause, however, was so slight that the friction produced on the guide O, by a weight of one pound resting upon the graduated bar M N, was found to be sufficient to reduce the variation in its readings to $\frac{1}{16}$ inch. The apparatus was now thought to be entirely satisfactory and the experiments were begun anew.

The readings of the graduated bar were taken to the nearest

one-sixteenth of an inch, and they were considered accurate within that limit, which, in a radius of 14 inches, gives a probable error of less than .005. During the progress of an experiment this radius was subject to gradual changes, which were generally accompanied by corresponding changes in the reading of the dynamometer.

The readings of the graduated bar and dynamometer were, however, always taken at the same instant, and as a matter of possible importance the temperature of the oil in the worm box was also recorded by a thermometer constantly immersed.

The dynamometer was sensitive to a variation of half a pound, and the readings for average cases were probably taken within .01 of their true amount, the error in observation being greater for light loads and less for heavy ones.

It was not suspected until the experiments were about to be concluded, that the dynamometer itself was liable to any error at all, and it was then discovered, when too late, that it had a constant error of about two per cent., which was either positive or negative according to the alignment of the universal joints.

It will be explained further on how this error was detected and the causes which produced it, but in view of the great variations found in experiments made under similar conditions, it was not thought necessary to attempt to make any correction. Indeed, this could not have been done without a repetition of the whole work, and the experiments are accordingly presented as subject to an error of two per cent.

The probable errors in observation are not included, because it is not to be supposed that the average of so large a number of experiments could be much affected by errors in observation, unless these errors were necessarily in one direction, as in the case just cited.

The merits and defects of the apparatus have been thus reviewed at length, partly for their own intrinsic worth, but chiefly as matters of the first importance to a clear appreciation of the real value of the experiments.

It is intended to give not merely the bare results, but also the facts upon which a judgment or criticism of them can be formed, and this is our apology for what may seem like unnecessary detail.

Having overcome the difficulties thus far enumerated the experiments themselves were continued.

Weights were suspended from the point M, ranging from 256 to 4,000 lbs., and the worm shaft was run under these loads at speeds ranging from 3 to 880 revolutions per minute. A great variety of conditions were thus obtained, and covered by about 800 experiments.

These conditions involved four variables, namely, speed, pressure, temperature, and state or nature of the rubbing surfaces.

The speed and pressure were the primary conditions adjusted by the operator, and the temperature and state of surfaces were secondary, and dependent upon the duration of the experiment as well as upon their primaries. The importance of the latter variable was not discovered until about half of the experiments were completed, and then it could be judged of only by its effects.

The experiments have been divided into series, each representing a special set of gearing tested.

The first series was made upon a double-thread worm, 4-inch diameter, gearing with a worm wheel of 39 teeth, $1\frac{1}{2}$-inch pitch, the thrust of the worm being taken on its annular surface instead of upon the step bearing used in other series. The remaining series upon this form of gearing were made upon worms having cast and cut teeth of single and double threads, all of the same pitch and diameter, gearing with the same diameter of wheel. Four series were made with spiral pinions 4-inch diameter, $1\frac{1}{2}$-inch pitch, having respectively 1, 2, 4 and 6 teeth, gearing with a spur wheel of 39 teeth, and one series with a spur pinion of 12 teeth, $1\frac{1}{2}$-inch pitch, gearing with the same wheel.

In every case the material used was cast iron, and to facilitate comparisons the wheels and pinions were all made as near as possible to the same dimensions.

From the great mass of data obtained in this way, it became necessary to deduce some general conclusions by which the efficiency of any system of gearing could be determined.

The foundation for this work was taken to be the efficiency of the apparatus used. This was computed for every experiment by two persons independently, and their results were compared and corrected by a third, so that the possibility of errors in calculation should be reduced to a minimum.

The method of computation was very simple, but the labor involved by so many experiments was very great. The efficiency of the apparatus was in each case determined by dividing the moment of the dynamometer into the moment of resistance.

The moment of resistance was measured by the product of the weight suspended at M into the distance R, plus an additional amount for the moment of the brake itself, and the moment of the dynamometer was measured by the product of its record into the distance AB times the ratio of the gearing used.

The efficiency was at once seen to depend principally upon the speed, and within limits, the higher the speed the greater the efficiency. But there were other disturbing elements including temperature, pressure and state of surfaces, the combined effects of which produced many exceptions to this general rule.

At very high speeds, the rubbing surfaces were more liable to cut, and beyond certain points which could not be definitely determined, the efficiency appeared to diminish.

In different cases, variations of temperature and pressure were accompanied by such contradictory results that no generalizations could be made concerning them.

As already stated, the condition of the rubbing surfaces could be judged of only by its effect. At certain speeds and pressures the efficiency would slowly increase to its maximum, while at others it would suddenly diminish and indicate the destructive action known as cutting.

Upon examination this destructive condition did not always become apparent to the eye, and in some cases the apparatus was taken apart and cleaned, without making any decided improvement in efficiency, but it was finally discovered that in such cases it was necessary to restore the surfaces to their best condition by running for some time at a moderate speed and pressure. From this it appeared that the order in which the experiments were made, had an important bearing upon the results obtained, and it also furnished a satisfactory explanation for the contradictory appearance of many experiments which were otherwise made under apparently the same conditions.

Throughout one-half of these series no attention was paid to the order of the experiments, their sequence being guided entirely by convenience. The main object at first was to discover the efficiencies corresponding to variations in speed, pressure and temperature, and to determine a definite limit beyond which the speed of worm gearing could not be carried to advantage.

The injurious effects of high speeds upon succeeding experiments was not at first noticed, and a number of series are thus

intermixed with exceptional cases arising from this cause, while in others the destructive conditions appear to form a separate group.

Having found the limiting speeds and pressures for worm gearing, care was taken in the remaining series to avoid them as far as possible, and by this means to secure some very good and harmonicus results.

In order to present the experiments in a convenient shape for generalization and practical use, the attempt was made to tabulate them with reference to the speed as the most important variable. But the tabular method proved to be inadequate, unwieldy, and difficult of application to other cases, and in its place was substituted a graphical method, by which the efficiency and attendant conditions in each series could be seen in a much more comprehensive and instructive manner.

The results of each series herewith presented have been carefully plotted to scale, making abscissas proportional to the logarithms of the revolutions per minute and ordinates proportional to the efficiency of the apparatus.

This peculiar method of constructing the abscissas was adopted because it was seen that increments in efficiency were more nearly proportional to the ratio of the speeds than to their actual differences.

Different symbols are used to denote different pressures on teeth, and each symbol is numbered to show the order in which the experiments were made and the temperature of the oil in the worm box.

These symbols are connected so as to form lines of efficiency corresponding to the various pressures used, and the average of all these lines is taken as the average for the whole series.

The first series of experiments graphically represented in Fig. 90 was made upon a double-thread worm 4 inches diameter, gearing into a worm wheel of 39 teeth 1½-inch pitch.

There are 114 experiments which cover a range of speed from 3 to 790 revolutions per minute, and loads up to 4,000 lbs. upon brake wheel.

The worm and wheel had cast teeth, and the thrust of the worm was taken upon the worm itself instead of upon the shaft step, as in the subsequent series.

Of these 114 experiments the first 38 have been omitted as preparatory on account of difficulties already stated, and the diagram

begins with experiment 39, when the leather lagging on brake strap was first used.

The first 62 experiments were made with the disc dynamometer, and from there on, gear wheels were used to diminish journal friction.

The diagrams herewith presented give the essential results obtained, and it will only be necessary in connection with them to notice some peculiarities.

Considerable variation in the efficiency of the apparatus at any given speed will be observed on all diagrams, but more especially

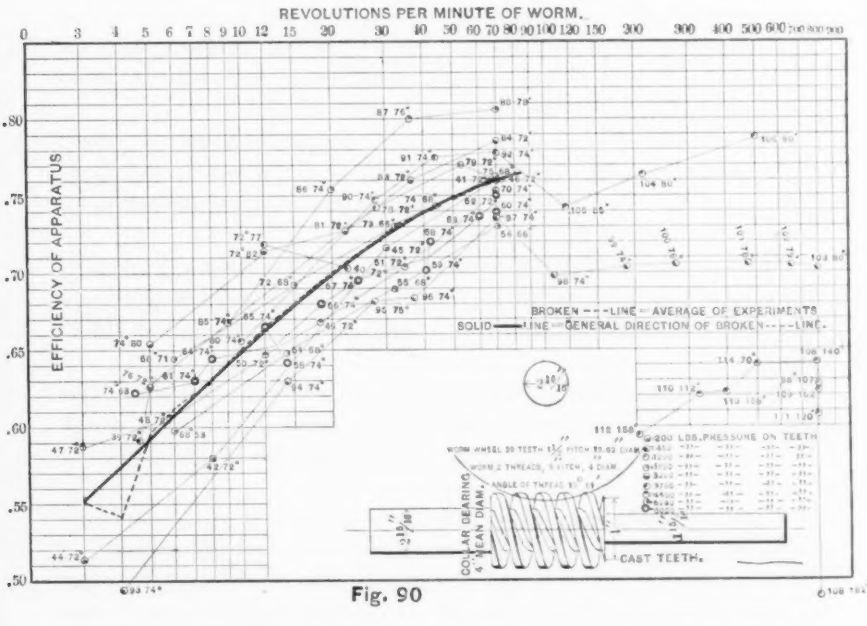

Fig. 90

upon those for worm gearing, where, as already stated, no attention was paid to the progressive order of the speeds and loads.

In Fig. 90 the bulk of the experiments were made between the limits of 3 and 70 revolutions per minute, for, in order to obtain the higher speeds it was necessary to make a change of pulleys, and, fortunately, this was not done until the experiments on the lower speeds were completed.

Between the limits just mentioned the general direction of the lines of efficiency is strongly marked, and the group forms a band of nearly uniform width, extending over about 10 per cent. of

variation. At the higher speeds the results are very much scattered and give unmistakable evidence of cutting.

This series was unexpectedly brought to a close by the breaking of the worm teeth, which were found to have been entirely ground away. The wheel, however, was in good condition, the wear having been just sufficient to polish the surface of the teeth.

The second series of experiments was made upon a single thread cast worm and wheel, the thrust being taken on the step at end of worm shaft. In this series, also, the experiments appear to form themselves into separate groups, and the same phenome-

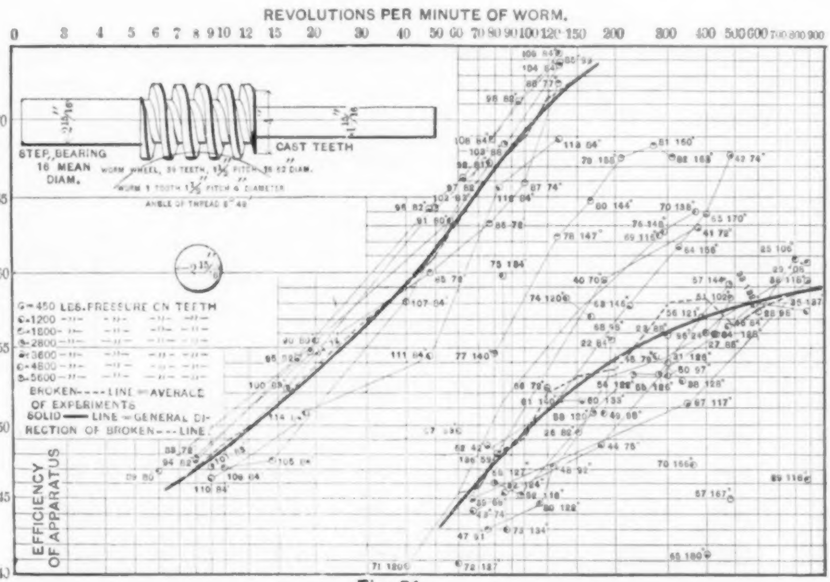

Fig. 91

non will be noticed on Figs. 92, 93 and 98, and in all cases where the destructive conditions depending upon speed, pressure and temperature have not been carefully avoided.

So long as the condition of the rubbing surfaces is unimpaired, the efficiency appears to increase with the speed, and even after cutting has begun, the same general tendency will be observed, although starting from a lower point on the diagram.

The principal object in this series was to determine the limiting speeds and pressures at which the worm could be run without danger of cutting. These speeds were also found to depend upon

the temperature and duration of the experiment, and they were, consequently, not clearly defined; but in general it was noticed that at slow speeds the greatest efficiency was found under the heaviest pressures, at moderate speeds under moderate pressures, and at high speeds under light pressures. This seemed to point toward a limiting product of speed and pressure, beyond which the heat of friction became generated so rapidly as to impair the condition of the surfaces in contact and produce cutting.

The following table shows the conditions under which cutting was found to take place within periods of ten minutes. In all cases the precise time at which cutting occurred was marked by a sudden rise in the reading of the dynamometer, and from the reading at that time the final efficiencies were computed. In the last experiment no cutting took place, as shown by the constant efficiency of .677.

SPEEDS AND PRESSURES LIABLE TO PRODUCE CUTTING.

Velocity of Sliding in ft. per min.	Pressure on Teeth.	Temperature.		Efficiency.		Duration of run in minutes.	Ft. lbs. per min. consumed in friction before cutting began.
		Initial.	Final.	Initial.	Final.		
800	1785	106°	140°	.609	.387	6	117,600
880	1780	118°	132°	.607	.462	3	129,300
880	1205	137°	150°	.575	.360	3	97,000
800	448	118°	133°	.594	.445	10	29,400
480	2822	144°	167°	.591	.450	7	117,800
400	3481	170°	180°	.639	.415	3	98,300
360	4837	138°	166°	.641	.473	6	122,400
306	5558	163°	186°	.677	.677	10	102,000

It appears from this table that the danger of cutting does not depend entirely upon the amount of frictional work, and it is not easy to understand why this should be the case, but the fact remains, and we are forced to conclude that very high speeds should be avoided, even under light pressures, and that the best working conditions are to be looked for at or below 300 revolutions per minute, which corresponds to a surface velocity of sliding of about 300 feet per minute. A great deal, of course, depends

upon the rapidity with which the heat of friction can be conducted away.

For the apparatus used it was determined that when one horse-power was consumed in friction, the worm box remained at a uniform temperature of about 50° above the surrounding air, and, assuming the rate of cooling to be proportional to the difference in temperature, it seems improbable that more than two horse-power could be continuously consumed in friction without overheating the lubricant.

In plotting the diagrams care has been taken to exclude from

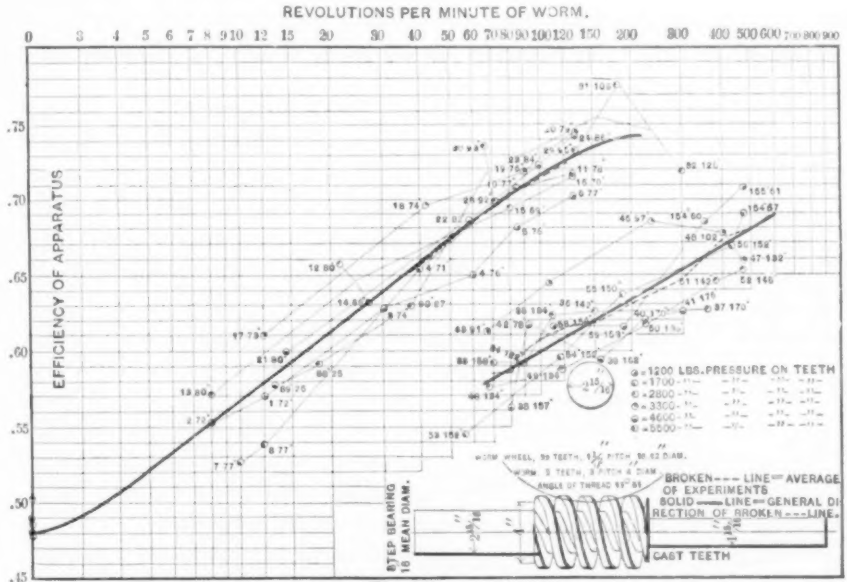

Fig. 92

the general result the worst and most evident cases of cutting, such as those just given in the table.

The third series, represented in Fig. 92, was made on gearing similar to that used in the first series, with the exception that the thrust of the worm was taken upon the step at end of worm shaft instead of upon the annular end of the worm itself. At the conclusion of this series the step bearing was found to be in good condition, the rubbing surfaces having worn to an annular bearing of, say from one to three inches diameter.

The worm shaft and its bearing at large end, however, were

badly cut, and, judging from the record of the experiments, the cutting must have occurred near the middle of the series, thereby reducing the efficiency of the latter half of the experiments, and furnishing an explanation for the discrepancies between this and the first series. In this series the limiting pressure for a speed of 300 revolutions appeared to be about 4,500 lbs. for a run of five minutes.

A pressure of 5,600 lbs. at a speed of 280 revolutions per minute and temperature of 190° produced cutting in three minutes, but whether on the teeth or in the journals it was never possible to determine without taking the apparatus apart at the sacrifice of considerable time. But this cutting would naturally be supposed to take place sooner on the teeth where the intensity of pressure was greater.

One more series, the 9th in order, represented in Fig. 98, completes the experiments upon worm gearing. This was made upon a worm and wheel similar in every respect to those used in the second series, except that the teeth were cut instead of being cast. The speeds, however, were kept below the limits found by previous experiments to produce cutting, and the temperatures were consequently moderate, ranging from 48° to 116° in extreme cases. In this way the rubbing surfaces were kept in good condition throughout, and the results are altogether more satisfactory than any yet recorded.

The break in the line of efficiency at the highest speeds is not due in this series to their injurious effect, but to the fact that the precaution of running the apparatus for some time before commencing to note experiments was accidentally neglected.

It had been previously observed that under moderate speeds and loads, the efficiency of the apparatus would continue to improve until a maximum limit appeared to be reached, and, in order to obtain observations under the best working conditions, a rule had become established not to begin noting down experiments until after the apparatus had been warmed up once or twice in running.

For all speeds below 100 revolutions, however, the curve shown on Fig. 98 may be accepted as very near the truth, and, by comparing this with Fig. 91, it seems probable that with both sets of gearing in their best working condition, these lines of efficiency would practically coincide.

From the experiments made to determine the limiting speed

and pressure at which worm gearing can be run successfully, we conclude that when the gearing is loaded to its working strength, it is not safe to exceed a velocity of sliding of 300 feet per minute, and that, in general, the best working conditions are obtained at or about a velocity of 200 feet per minute. The experiments upon spiral gearing begin with Fig. 93, which shows the efficiency of

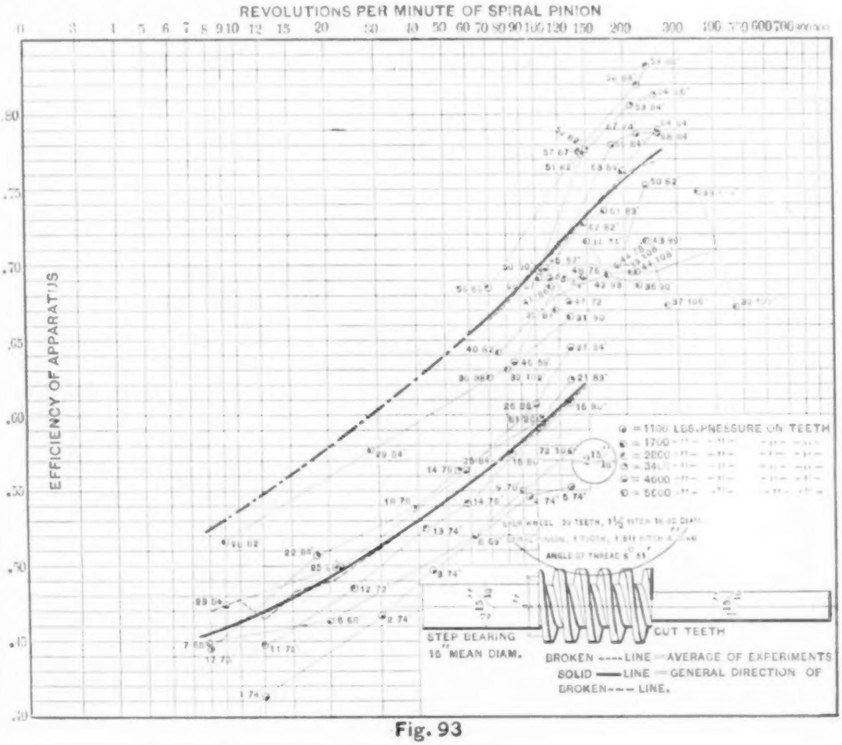

Fig. 93

a one-toothed pinion gearing with a spur wheel of 39 teeth $1\frac{1}{2}$-inch pitch.

In these experiments, the shaft G H was set around at an angle of 6° 51′, so that the teeth of the spur wheel should gear properly with the pinion. The results obtained show a constant improvement in the condition of the rubbing surfaces throughout the series, and clearly indicate the necessity just mentioned, of running the apparatus for some time before taking observations. Here the slow speeds were taken first before the teeth were worn to a good bearing, while in the ninth series the circumstances

were reversed. In both series the condition of the surfaces underwent considerable change, and the results are to that extent not as satisfactory as could be desired. Upon repeating some experiments at the conclusion of this series, it was noticed that the efficiency had improved nearly 10 per cent., and the broken line on the diagram was drawn to show the probable position of the curve, had the precautions alluded to been observed. Theoretically, the efficiency of the spiral gear should have been a trifle better than that of worm, on account of a slight diminution in the

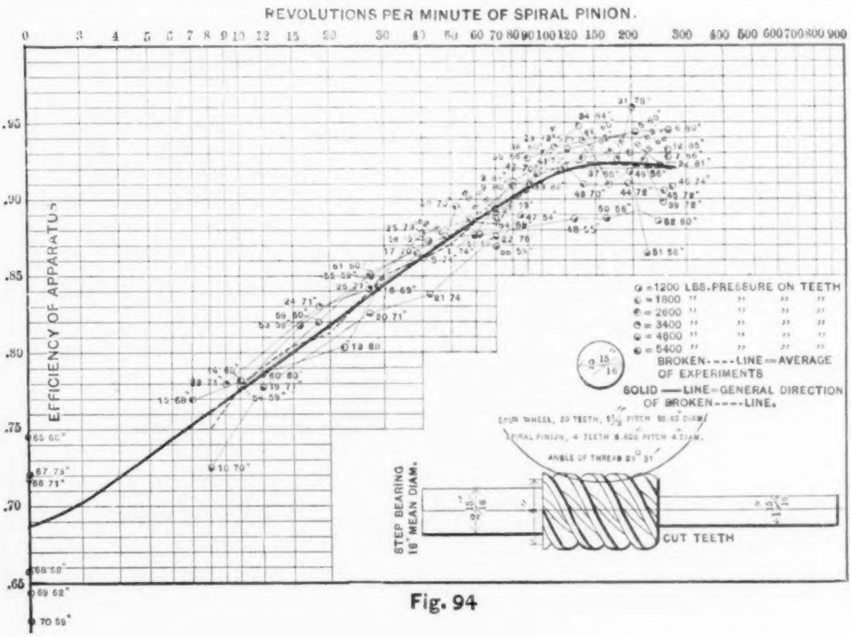

Fig. 94

velocity of sliding, and had this series been repeated, it would undoubtedly have so appeared. The teeth used in this gearing were accurately cut to cycloidal shapes, and in all cases their action in rolling contact was practically perfect, while with the worm gearing the shape was rather indefinite, presumably evolute, but the patterns for the wheels were made by a cutter, like the worm itself, and the two were necessarily obliged to fit.

The fifth series was made upon a spiral pinion of 4 teeth, with the shaft G H set around at an angle of 28° 31′. In this, as well as the following series of experiments upon spiral pinions, the

apparatus was run at a moderate speed and load, until the rubbing surfaces were thought to be in their best working condition, before any records were taken, and as a result, the diagrams will be found to be more definite and conclusive than anything heretofore shown. The speeds ran just high enough to show that the limit had been nearly reached, but no well-defined case of cutting occurred, and the experiments may be regarded as having been taken under the best working conditions.

The sixth series was made upon a spiral pinion of 2 teeth, with

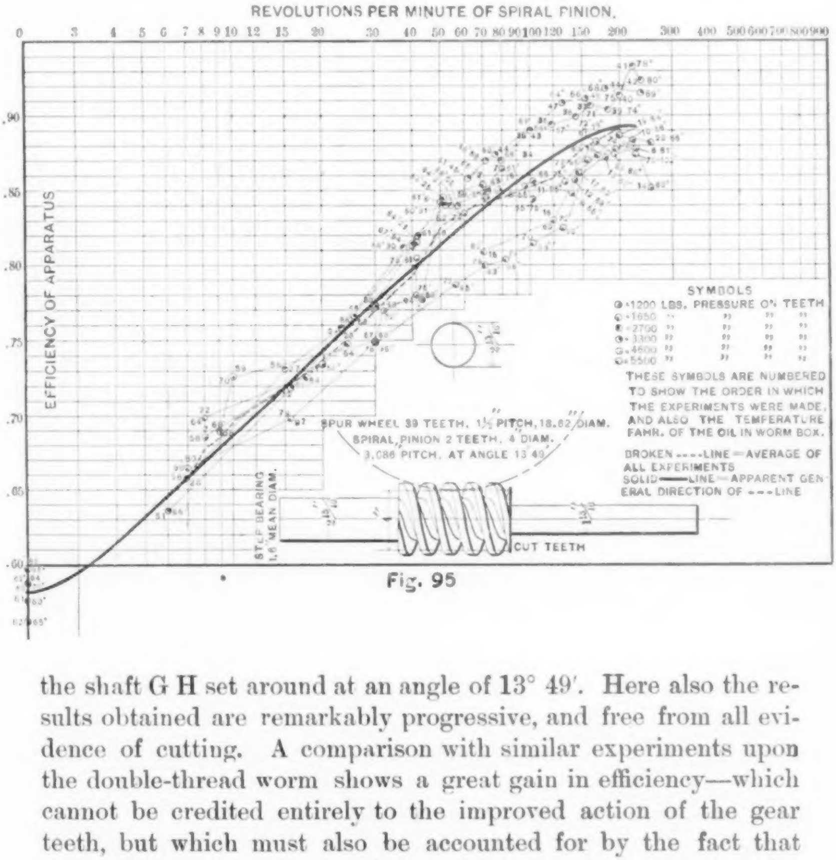

Fig. 95

the shaft G H set around at an angle of 13° 49′. Here also the results obtained are remarkably progressive, and free from all evidence of cutting. A comparison with similar experiments upon the double-thread worm shows a great gain in efficiency—which cannot be credited entirely to the improved action of the gear teeth, but which must also be accounted for by the fact that destructive speeds were avoided, and that the apparatus was maintained throughout in good working condition.

The seventh series, upon a spiral pinion of 6 teeth, with the shaft G H set at an angle of 45° 44′, also gives very good results,

and these three series, Figs. 94, 95, and 96, might be taken as a basis upon which to determine the laws of friction for spiral gearing in general.

To make a complete analysis of these results, it would be necessary to eliminate the friction of worm shaft and drum shaft from that of the teeth themselves, and determine the coefficient of friction for a number of different velocities of sliding.

An effort was made to do this by running these shafts under known journal pressures, but the results obtained were of such an indefinite and contradictory character that the task seemed hope-

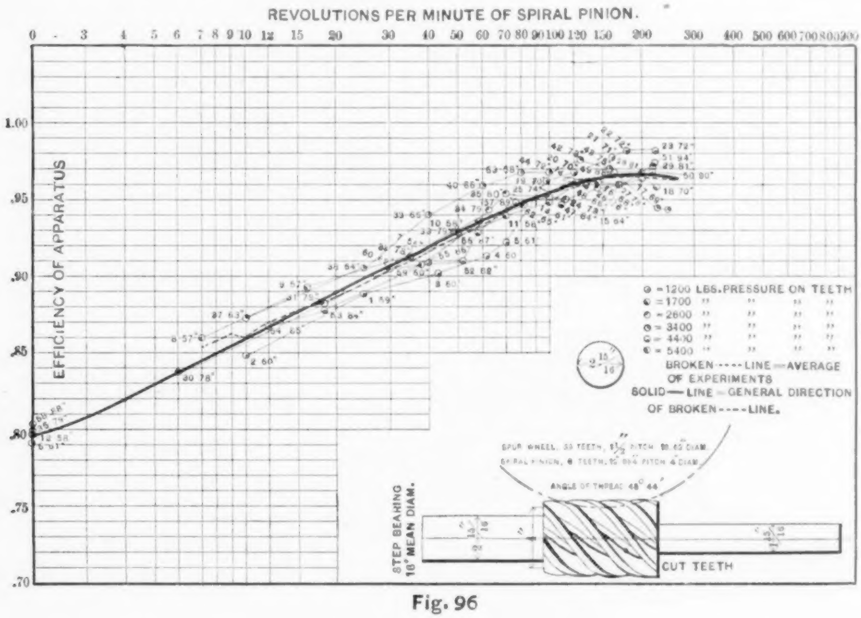

Fig. 96

less, and it was not until after the completion of the eighth series that a method was adopted by which the efficiency of any set of gearing could be approximately determined. For practical convenience the efficiency of the whole apparatus may be considered as equal to that of any other similar piece of gearing, and, as a matter of fact, the relative dimensions of the various parts do not differ a great deal from those used in these experiments.

The wheels might be larger or smaller, but the diameters of spiral pinions and worms generally bear about the same relation to their shafts, and it is in these parts that the principal work of

friction is consumed. When the angle of a worm or pinion and its speed are given, it should be possible, from these experiments, to fix at once with tolerable accuracy the efficiency of the gearing to which it belongs.

This can now be done for the angles experimented upon, but we shall presently show how by the method alluded to, the efficiency may be determined for any angle whatever.

The eighth series (Fig. 97) was made upon a cut spur pinion of 12 teeth $1\frac{1}{2}$-inch pitch, with the shaft G H set parallel to the pinion shaft. In this series an error, already mentioned, was discovered

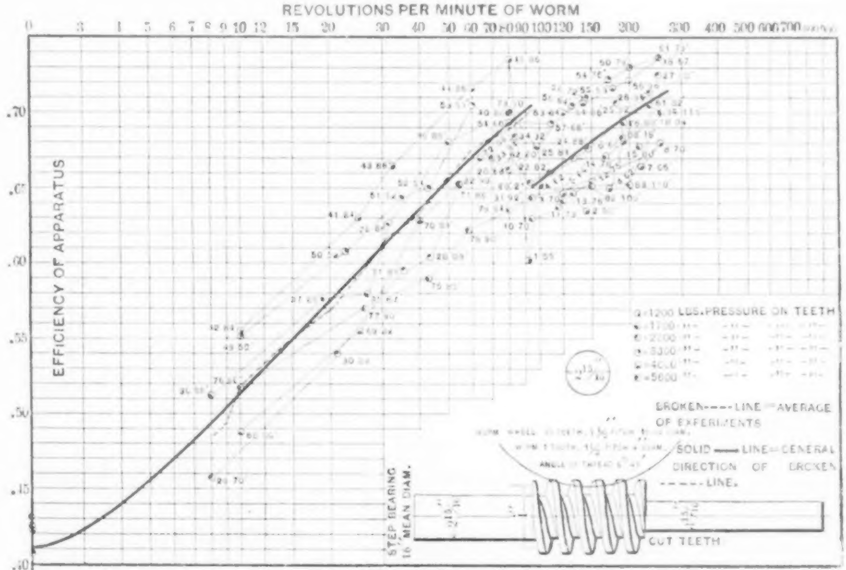

Fig. 98

in the readings of the dynamometer. The efficiencies, as calculated from the record of experiments, were in some cases found to exceed 100 per cent., and these results were repeatedly confirmed by experiments.

Numerous efforts were made to locate the error without success, until finally it was remembered that the accuracy of the instrument was predicated upon the assumed flexibility of the universal joints. It was also thought probable that however carefully adjusted the shafts F E and C D might be, they would no doubt be somewhat out of line, and that consequently the universal joints

would be forced to swivel slightly under pressure. Should the error be due to this cause it was expected that by moving the shaft F E parallel to itself for a short distance to either side of the center line of C D, the efficiency would appear to be alternately above and below its true value by an amount due to the stiffness of these joints.

The shaft E F was accordingly moved about 1 inch from the center line in either direction and a number of experiments were made in each position, the result of which showed a total varia-

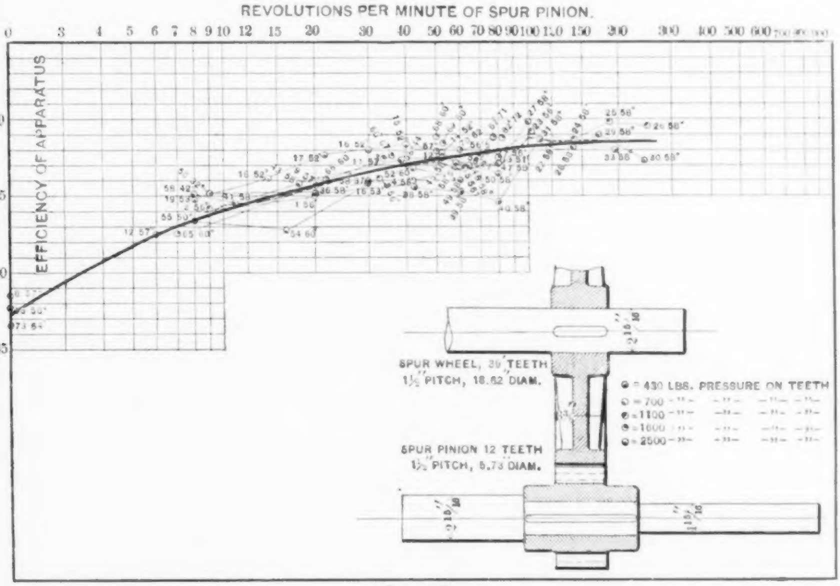

Fig. 97

tion of about .04 in the average efficiencies as determined for each position and speed.

For a speed of 20 revolutions the efficiency varied from .944 to .993 according to the position of E F, and the true value was accordingly estimated at .968, the arithmetical mean. Similarly the efficiencies at a speed of 80 revolutions, varied from .966 to 1.016, giving .991 for the true value at this speed.

The eighth series, as plotted upon Fig. 97, represents the mean of two sets of experiments conducted in this way, and the errors due to the stiffness of the universal joints are consequently eliminated. It should be observed in regard to this error, that its

actual amount is proportional to the efficiency itself, or, in other words, that the error is 2 per cent. of the efficiency and not a actual difference of 2 per cent. more or less, and for this reason it may be said there was less necessity for its correction in any other series than there was in this. An approximate analysis of the loss in effect presents fewer difficulties in this series than in any other, but still it is necessary to make certain hypothetical assumptions which cannot as yet be substantiated. This loss in effect may be considered as composed of three parts, the friction of two journals and the intervening gear teeth.

If we assume the loss in each case to be proportional to the product of the pressure and amount of sliding, we have the pressure on pinion shaft equal to the pressure on teeth, and the pressure on drum shaft about 50 per cent. greater, owing to the resultant from load suspended.

The amount of sliding per revolution of pinion shaft is about 7.5 on pinion shaft, 3 inches on teeth, and 3 inches on drum shaft, and the product of these numbers times the pressure in each case gives the relation of 7.5, 3, and 4.5, the sum of which is 15. Of this total effect the friction of the gear teeth consumes .2.

Therefore, it appears on this assumption, that the principal loss in effect with spur gearing is from journal friction.

The experiments which were made to determine the friction of the worm shaft under various loads and speeds, showed for a pressure of 800 pounds, coefficients ranging from .15 at the start to .006 at speeds between 110 and 240 revolutions per minute, and for a pressure of 3,440 pounds, coefficients ranging from .12 at the start, to .033 at a speed of 240 revolutions per minute.

This irregularity under different loads made, as already observed, so much difficulty in determining the loss in effect from journal friction, that the attempt to eliminate it was at first abandoned, but, after the completion of the eighth series it was thought that, inasmuch as the principal loss was there due to this cause, this series itself might be made the basis upon which to eliminate the effect of journal friction in others.

We will therefore assume for this series that the loss in effect from journal friction is .8 of the total amount of loss. The loss from this source in other series has been roughly estimated to be nearly the same, and proceeding from this stand-point we can find the probable coefficients of friction for the pinions used in the fifth, sixth, and seventh series.

The efficiency of the apparatus, E, plus L, the loss in effect from journal friction gives the efficiency of the spiral pinion and its step. From this we can find the coefficient of friction by assuming the coefficient for step friction to be the same as for the teeth themselves.

Let α = angular pitch of spiral pinion.
φ = coefficient of friction.
n = ratio of mean diameter of step to pitch diameter of pinion = .4.

Then it can readily be shown that

$$E + L = \frac{1}{1 + (1 + n)\,\varphi \cot \alpha} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

whence

$$\varphi = \frac{1 - (E + L)}{(E + L)(1 + n) \cot \alpha} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From these formulæ a number of coefficients have been calculated, and their values given in the following table, from which it will be seen that the coefficients agree as closely as could be expected, and thereby indicate to a certain extent an harmonious relation between the curves employed in their determination.

Revs. of Pinion.	E 8th Series.	L	E 5th Series 4 Teeth.	E 6th Series 2 Teeth.	E 7th Series 6 Teeth.	ϕ 5th Series 4 Teeth.	ϕ 6th Series 2 Teeth.	ϕ 7th Series 6 Teeth.	Average Value of ϕ.
3.	.90	.08	.70	.59	.81	.105	.086	.094	.095
5	.92	.064	.73	.63	.83	.097	.078	.089	.088
10	.94	.048	.774	.685	.86	.081	.064	.076	.074
20	.956	.035	.817	.741	.89	.065	.050	.061	.059
50	.973	.022	.876	.813	.93	.042	.035	.038	.038
100	.982	.014	.912	.862	.955	.030	.025	.024	.026
200	.984	.013	.923	.892	.967	.026	.018	.015	.020

The discrepancies are really not so great as they appear when we consider the variations in the experiments themselves, and the practical impossibility of obtaining very accurate results.

The average of these coefficients, it was thought, would represent, as nearly as possible, the best general results of the whole course of experiments; but upon still further comparison with the

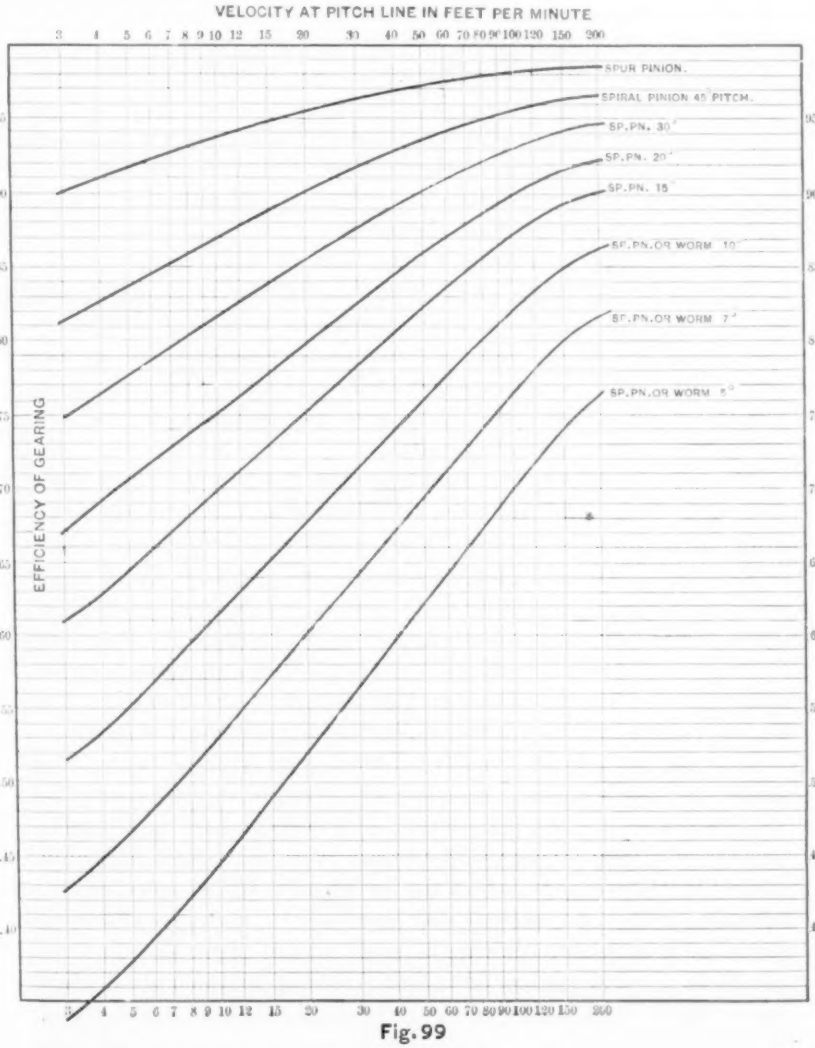

Fig. 99

best parts of the other series, it was concluded that the sixth series itself formed a better standard from which to deduce hypothetical curves of efficiency for other pinions not included in the

experiments. In this series the probable error in these coefficients was less than in either of the others, because the pinion and step consumed a larger proportion of the total loss in effect, and had the series upon the single tooth pinion been made with equal care the error would have become still less.

We are well aware that the method just employed for the determination of these coefficients is open to criticism, but when judged by the character of the data with which we had to deal, we believe it to be as refined as the conditions would permit, and however faulty it may be in theory, it is evident that by reversing the process we can reproduce from these coefficients the original curves from which they were deduced.

The effort has been to find some law, by analysis if possible, or by trial, if necessary, which would cover the best portions of all the experiments. In this we have failed to obtain sufficiently accurate data for a complete analysis of the problem, and we have reluctantly been obliged to feel our way by the aid of assumptions whose merits were largely founded upon the results which they afterward brought about.

We have assumed, for instance, that the loss in effect from journal friction is the same for all series, that the coefficients for different portions of the apparatus are alike, and that the velocities of sliding for the different pinions were equal, to all of which exceptions might very properly be taken, but the results obtained upon these assumptions have agreed so well with each other that, for the present, we are obliged to accept them as correct, at the same time hoping that there may be enough interest awakened in the subject to lead to a better understanding of it in the future.

In order to put the results of these experiments in the best shape for practical use, a number of curves have been computed and plotted in Fig. 99, from which it is easy to find by interpolation the efficiency corresponding to any angle and speed of pinion.

To illustrate the practical use of Fig. 99, let us consider the following train of gearing and proceed to find its efficiency. Given a spur pinion driving a spur wheel, upon the shaft of which is a spiral pinion 30° angle, driving another spur wheel upon whose shaft is a worm 7° angle driving a worm wheel. When the speed of the first shaft is known, it is easy to find from the dimensions and ratio of the gearing, the velocity at pitch-line of each pinion or worm. We will assume 200 feet per minute for the spur pinion, 50 feet per minute for the spiral

pinion and 10 feet per minute for the worm, and by reference to Fig. 99 we find .985 for the efficiency of the spur pinion, .902 for that of the spiral pinion and .53 for that of the worm.

The efficiency of this train of gearing is, therefore, $.985 \times .902 \times .53 = .471$. For speeds above 200 feet per minute, it is recommended to estimate upon the efficiency at that speed.

From the coefficients as determined for the sixth series, $E + L$ can be found by substitution in the formula.

$$E + L = \frac{1}{1 + 1.4\ \phi \cot \alpha}$$

and the value of E is then easily determined by subtracting the value of L, as just given in the table.

The curves determined for 5°, 7° and 10° spiral pinions may be used equally well for worms, and they have been so marked. If we let E = efficiency of a spiral pinion, and E_1 = efficiency of a worm having the same angular pitch, then it can be demonstrated that

$$\frac{E}{E_1} = \frac{\cot \alpha}{\cot \alpha - \phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

as far as the friction of the teeth alone is concerned, and for small angles this relation is so near unity that the difference is practically inappreciable.

Theoretically, for worms of 20° pitch, the difference in efficiency for the teeth alone should not exceed .03, but the side thrust on worm wheel would probably increase the friction considerably if larger angles were used.

Additional experiments, not shown on the diagrams, were made to determine the efficiency of spur gearing, in which the reduction corresponded more nearly with that of the single and double-thread worms. For this purpose the driving gear of an old 48-inch lathe which had been for many years in use, and consequently represented the best attainable conditions, was called into requisition. This lathe was provided with double-back gears which gave, respectively, reductions of 38.4 and 10.2 to 1. The counter-shaft was driven by the dynamometer, and the power transmitted was measured by the brake, already described, carried between the centers. The efficiency of the lathe was measured for every speed under different loads, the average results of the

test showing that for a reduction of 10.2 to 1 the efficiency was .952, and for a reduction of 38.4 to 1 the efficiency was .935. The power required to run the lathe empty was, of course, excluded in the calculations, otherwise the efficiencies would have had a different value for every load, becoming almost *nil* for light loads and approaching the figures given for heavy ones such as the lathe was expected to carry.

These experiments showed conclusively the advantage of spur gearing over all other kinds in point of efficiency and durability, for not only is there less friction, and consequently less wear, but the wear in spur gearing is distributed over a great deal more surface.

In conclusion it may be said, that the whole subject of friction is dependent upon such a variety of changeable conditions that it is almost impossible to separate and determine the effect of each independent variable apart from others which attend it. The four variables—speed, pressure, temperature, and condition of surface—are susceptible of infinite combinations, and even if it were possible to measure the effect of each independently, it is quite probable that the effect of any given combination would not follow from that of each component factor.

To give some idea of the extent of these experiments and the time and care spent in arriving at the results herewith presented, it may be stated that the experiments began on the 8th of August, 1883, and were not completed until January 14, 1884, that fully six months were consumed in the design and construction of the apparatus, and as many more in working up the records obtained.

That they were not more completely successful is due to the fact that we attempted too much at first, and only learned by experience the influences which operated to impair the results.

DISCUSSION.

Mr. H. R. Towne.—The relative efficiency of worm and spur gearing has heretofore been largely a matter of speculation and theory, authorities differing widely in regard to it, and no reliable information being given in the text books as to the true efficiency of either under the conditions of ordinary practice. The experiments now under review go further to supply this deficiency than anything previously published, and are of corresponding interest and importance.

The practical value of all such information is increased by stating the results obtained in a summarized form, convenient for reference, and so far condensed as to admit of the whole being comprehended at one time, and with reference to the relationship of each of the several deductions to the others. Such a summary of the experiments presented by Mr. Lewis seems to afford the following deductions and indications:

1. That spur gearing is decidedly the most efficient mode of transmitting power by positive gears.

2. That the efficiency of a pair of good cut spur wheels, having a velocity ratio of 1 to $3\frac{1}{3}$, ranges from 86 to 99 per cent. under average conditions.*

3. That with spiral gearing the efficiency is increased with the angle of the thread until the spiral pinion becomes a spur pinion, when the axes of the pinion and its wheel become parallel.

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* Referring to this deduction, Mr. Lewis comments as follows:

"Here I would prefer to see the velocity ratio eliminated, although I recognize it as a variable, which should, strictly speaking, be considered.

"The diagram (Fig. 99) is constructed without reference to anything but velocity at the pitch line of teeth, so as not to complicate the question more than is necessary for an approximate determination. As a result of this, I believe that the errors arising from neglecting the velocity ratio, size of journals, etc., have a tendency to neutralize each other. For instance, supposing two cases of a wheel and pinion in each of which the same pinion is used. Let the wheel in one case be three times, and in the other six times the diameter of the pinion. For the same velocity of teeth, the pinion in each case makes the same number of revolutions, and the friction on its journals is the same. The smaller wheel, how ever, runs twice as fast as the larger, and there is consequently twice as much sliding in the journals in its case, but, on the other hand, the coefficient of friction for the smaller wheel is much less on account of higher velocity.

"By the same reasoning large journals would compensate partially, in the reduction of the coefficient of friction, for the increase in the amount of sliding. This feature is not proved, of course, but such a tendency no doubt exists, and is further augmented by the fact that, in general, the larger the wheel, the larger are its journals. It may be stated, as a general principle, that the higher the number of teeth gearing together and the smaller the journals, consistent with strength and stiffness, the greater the efficiency.

"The limits imposed upon all proportions of gearing by considerations other than that of efficiency do not, I think, admit of such great variations as to require in ordinary practice a consideration of the question of velocity ratio. Taking any pair of shafts connected by gearing, I think it will be admitted that the efficiency will increase with an increase in the size of either one of the gears, whether the velocity ratio becomes greater or less. I would suggest, therefore, that as the velocity ratio cannot be used as a guide or limit, it may properly be omitted from the statement covered by your second deduction from the experiments reported by me.

4. That with worm gearing the efficiency corresponds at small angles (10° and less) very closely to that of spiral gearing. At greater angles the efficiency of worm gearing does not increase so rapidly as that of spiral gearing, and, unlike the latter, the possible angle of the thread is limited.*

5. That the efficiency of spiral and worm gearing (of cast iron, machine cut or worn smooth) ranges from 35 to 90 per cent., according to the conditions of speed, angle, pressure and quality of surfaces.

6. That with each kind of gears there exists a certain point of maximum efficiency, depending chiefly, under average conditions, upon the velocity of the rubbing surfaces.

7. That very high velocities develop a tendency of the rubbing surfaces to cut, and that this difficulty then becomes a limiting condition.

8. That with worm gearing, a large part of the applied power being lost in end-thrust on the worm shaft, it becomes correspondingly important to adopt a form of end-bearing which will diminish this loss as much as possible.

9. That the range of variation from the mean line of efficiency in the Sellers experiments rarely exceeds 5 per cent. in either direction, in the case of worm gearing, and is diminished to about 3 per cent. in the case of worms of high angles and spur gearing, so long as no cutting occurred, but that the variation became much greater and very irregular as soon as cutting began.

10. That in general, at slow speeds, the greatest efficiency is found under the heaviest pressures; at moderate speeds under moderate pressures; and at high speeds under light pressures. This seems to indicate a limiting point for the product of speed and pressure at which the heat of friction is so rapidly developed as to impair either the condition of the surfaces in contact, or their ubrication, and cause cutting.

* On this latter point Mr. Lewis suggests as follows:

"The limiting angle for the maximum efficiency of worm gearing is to be determined in a manner similar to that for screws, as fully explained in a paper which I prepared some years ago, and which may be found at page 73 of the Journal of the Franklin Institute for February, 1880. The formula particularly referred to is No. 6, on page 76, from which it will be seen that the angle for greatest efficiency is probably limited to something approximating to 45°. Although this angle has been often used on screws with good results, I should hesitate to recommend it for worms on account of the heavy side thrust upon the worm wheel, and I am inclined to think that 30° is the more nearly correct figure for the limit for maximum efficiency of worms."

11. That the liability to cutting depends upon the speed of sliding, being also affected by the quality of lubrication, the intensity of pressure, and the period of duration.

12. That with worms and spiral gears, time or duration of action is a limiting element, a good pair of gears being capable of efficient action for five or ten minutes, or more, but failure from cutting will result after the proper time is exceeded. (See table on page 286.)

13. That the maximum efficiency is attained at or below a velocity of 300 feet per minute of the sliding or rubbing surfaces, and that while it is possible to exceed this limit temporarily with good results, it is somewhat hazardous to do so. For continuous work 200 feet per minute is probably the highest velocity of rubbing surfaces which can safely be adopted without danger of cutting.

14. That in spur gearing the chief loss from friction is in the journals, which should therefore be carefully designed; that the same is true in the case of worm gearing, but that it is still more important in the latter to provide for the reduction of friction due to the end-thrust on the worm shaft.

15. That the coefficient of friction (φ) of the spiral pinion and the worm, including step friction, ranges from 2 to $9\frac{1}{2}$ per cent. (See table on page 296.)

The importance of the above determinations admits of no question, and will be apparent to any one who has ever given consideration to the subject to which they relate. The application of them, and of their summarized results, as shows in the tables on pages 286 and 296, and in the diagrams of efficiency (Fig. 99), will be of the greatest service in adapting gears of any kind to any particular use, and this is especially true in the case of worm gearing—wherein the limiting conditions have been but little understood heretofore.

As a further contribution on this subject, I will now describe the results obtained from a series of experiments made at the instance of the writer for The Yale & Towne Manufacturing Company, by Professor R. H. Thurston, at the Stevens Institute of Technology, in 1883-4.

In this case the gears experimented with consisted of a cast-iron worm-wheel of $15\frac{15}{16}$ inch pitch diameter, with 50 machine-cut teeth, $2\frac{1}{2}$ inch face; driven by a double-threaded cast-iron worm, machine finished, of $6\frac{1}{16}$ inch pitch diameter, and 4 inches long on the thread. The velocity ratio was 25 to 1. These gears were set in a

suitable frame, and were driven by power transmitted by shafting and belting through a transmitting dynamometer carefully standardized. The power transmitted from the gearing was taken off and measured by a Prony brake, also carefully adjusted. The tests were made with much care, and by competent observers, and were sufficiently numerous to give assurance of reliable results. The range of speeds covered by the experiments extended from 41 to 339 revolutions of the worm per minute, and the variations in the amount of power transmitted to the gearing ranged from .2 to 4.14 horse power. In each case, apparently, the experiments were carried up to and beyond the point of maximum efficiency, which latter was, of course, the determination aimed at in the investigation. The range of speeds above referred to gave velocities of the rubbing surfaces ranging from 65 to 538 feet per minute. The latter speed afforded clear indications that the point of maximum efficiency had been reached and passed, its location, under the conditions of the tests, being apparently at or about the point of 243 feet per minute of the rubbing surfaces (which is obtained at a speed of 140 revolutions of the worm per minute), a result strikingly corroborative of the work of Mr. Lewis.

Appended hereto are two tables, No. 1 giving the dynamometer readings, speeds and efficiency of the gears under varying conditions, and No. 2 showing a summary of the same, with the coefficient of friction as deduced therefrom.

By examining these tables it will be seen that the ratio of power absorbed or lost in the gearing decreases with increasing velocity up to a maximum at the point where the velocity of the rubbing surfaces is 243 feet per minute. As the speed is further increased a decrease in efficiency occurs, thus indicating clearly the speed at which the gears experimented with should be driven in order to attain the highest efficiency in amount of power transmitted. Obviously the precise velocity of maximum effect will vary with the degree of pressure on the rubbing surfaces, and also, to a less extent, with the kind of lubricant used. In the present case the maximum obtained was with the highest pressure consistent with the general strength and rigidity of the apparatus, and with thorough lubrication with good sperm oil. It thus represents conditions which are probably the best ordinarily attainable, and the maximum efficiency thus indicated, while closely approachable, will rarely, if ever, be exceeded in ordinary practice.

The results of the tests made by Professor Thurston, as above

described, showed a lower efficiency of transmission from the gears than was expected, and this led to an investigation as to the causes of loss. Chief among the latter was evidently the absorption of power caused by friction due to the end-thrust of the worm against its bearing in the frame. To diminish the loss from this cause, the apparatus was thereupon provided with two forms of thrust-bearing for the worm-shaft, so arranged than either could be readily applied and tested independently of the other.

As originally constructed the end-thrust of the worm was taken directly from its hub upon a corresponding face or collar of the cast-iron box contained in the frame or housing, the width of this surface of contact being one inch, and its mean radius from the center of the shaft or worm $1\frac{1}{4}$ inch. The two modified arrangements consisted:

1. Of what is usually known as the "button thrust-bearing, or step," in which the projecting end of the worm shaft is capped with a thin disc of hardened steel, the exposed face of which is slightly convex, and behind this is placed an adjusting set screw with its hardened end abutting against the disc on the end of the shaft. In this case the area of contact of the rubbing surfaces is the minimum which, which the metals used, will resist crushing under the thrust received. The radius of rotation is obviously also reduced to a minimum, and is very small. The whole bearing was kept well oiled.

2. Of an adaptation of the well-known roller thrust-bearing, consisting, in this case, of 12 chilled cast-iron coned rollers of $\frac{9}{16}$ inch mean diameter, contained within a brass cage having a separate pocket for each cone, the cones traveling, at a mean radius of $1\frac{3}{8}$ inch from the axis of the shaft, between two steel collars or rings, one bearing against the hub of the worm, and the other against the face of the frame bearing, the faces of these rings being coned to the shape of the rollers. The centrifugal thrust of the cones was resisted by a wrought-iron ring surrounding the cage, the ends of the cones being convex.

The apparatus being thus modified, further tests were made, the results of which, in the case of the button thrust-bearing, are shown by table No. 3, and in the case of the roller thrust-bearing by table No. 4. A comparison of these with the preceding tables at once discloses the fact that the efficiency of the gearing is materially increased by both forms of thrust-bearing, a maximum of over 60 per cent. being obtained with the roller thrust-bearing, as compared

with a maximum of 43 per cent. with the original bearing. The efficiency of the improved apparatus is thus 50 per cent. greater than that of the original arrangement, showing conclusively that a well designed thrust-bearing is a most important feature in worm gearing for the transmission of power. Appended to this are diagramatic plottings of the several tests above described (Figs. 137 and 138), an examination of which will show a curious divergence in the curve of efficiency of the button and of the roller thrust-bearings. This discrepancy or divergence led to some apprehension of an error in the tests, and to remove doubt on this point the experiments were repeated, but with no change in the

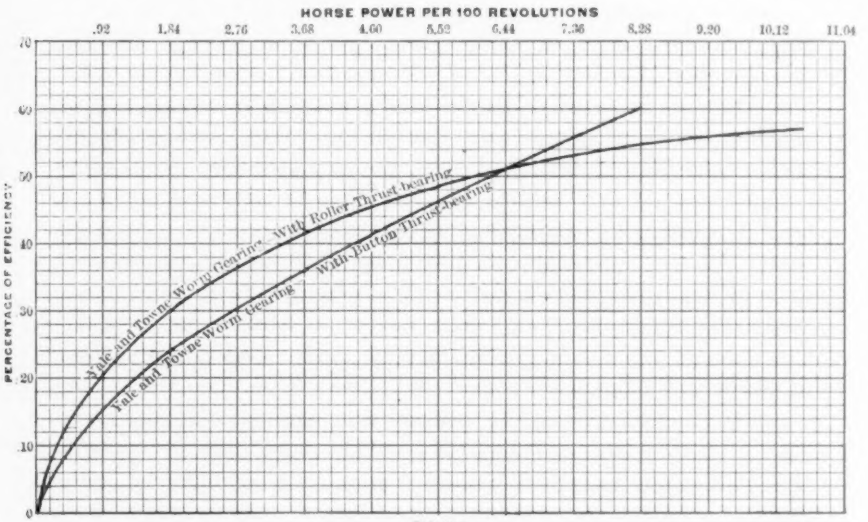


FIG. 137

results. It is probable, therefore, that under high pressures the roller bearing is subject to deformation, or other alteration from strain, which decreases its efficiency, whereas the button bearing continues to gain in efficiency up to the point where cutting of the surfaces will begin. The general results of Professor Thurston's tests, as above referred to, are very clearly exhibited in the above diagrams, Figs. 137 and 138.

The results of the tests thus reported are strikingly confirmative of those made by Mr. Lewis, and all of the deductions which I have stated as to be derived from his work apply almost equally to that of Professor Thurston. The chief importance of the latter, aside from its general confirmation of the former, is the clear indi-

cation it gives of the importance of providing for the end-thrust of the worm much more carefully than has ordinarily been done. The experiments show that by a very simple provision of this kind the efficiency of the worm can be increased 50 per cent. Thus constructed, its absolute efficiency may be said to range from 50 per cent. to 60 per cent. of the power received, as compared to an efficiency of 75 per cent. to 95 per cent. in the case of the best cut spur gearing. Worm gearing thus becomes a permissible mode of transmitting power in many cases where a large velocity ratio is desired within a small space, where it is necessary to connect two shafts whose axes are at right angles, and especially where the duty on the gearing is intermittent and the duration of maximum stress limited to short periods of time. On the other hand, it is obvious

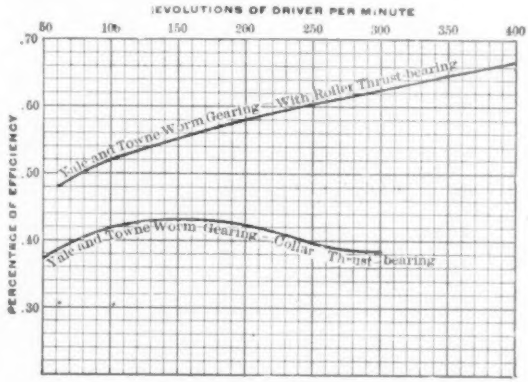


FIG. 138

that worm gearing cannot, under any circumstances or conditions, be as efficient a means of transmitting power as well made spur gearing, and that the loss of efficiency becomes very excessive when worm gearing is used at slow speeds. *These facts indicate clearly, therefore, that the use of worm gearing for transmitting power in machines moved by manual labor, and at slow speeds, should always be avoided, if possible.* In machines driven by steam, or other power, worm gearing is often a most convenient and useful substitute for spur gears, and, if properly designed and used, is not so much below the latter in efficiency as to cause any hesitancy in its employment. The proper methods of constructing and using worm gearing will certainly be better understood by a study of the investigations reported in the preceding pages.

TABLE NO. 1.

YALE AND TOWNE WORM GEARING. WITH COLLAR THRUST-BEARING.

Reading on Dyn.	Rev. of Dyn.	Rev. of Worm.	Rev. of Gear.	Pounds on 4-foot Arm.	Horse-power received.	Horse-power given out.	Effic.
½	70	50.7	2.03	59	.252	.0914	.362
1	70	50.7	2.03	116	.417	.176	.422
1½	72	52.1	2.08	166	.599	.254	.424
2	70	50.7	2.03	213	.747	.33	.441
2½	66	47.8	1.91	250	.860	.264	.423
3	67	48.5	1.94	290	1.032	.429	.415
½	57	41.6	1.23	58	.205	.055	.268
½	108	78.6	3.14	61	.388	.146	.376
½	135	97.8	3.91	65	.486	.194	.399
1	54	39.1	1.56	92	.322	.109	.338
1	97	70.6	2.82	100	.578	.215	.371
1	122	88.7	3.54	106	.727	.286	.393
2½	157	116	4.64	252	2.04	892	.437
2	157	116	4.64	212	1.67	.751	.449
1½	162	119	4.76	157	1.34	.570	.425
1	168	123	4.92	105	.995	.39	.391
1	162	117.7	4.7	119	.965	.427	.444
½	159	115.5	4.62	68	.572	.239	.417
1	197	143.1	5.72	121	1.174	.528	.449
½	212	153.9	6.15	71	.763	.333	.436
1	255	184	7.36	123	1.519	.691	.454
½	285	206.5	8.26	69	1.026	.435	.423
¾	218	165	6.64	91	1.04	.460	.435
¾	266	195	7.80	87	1.27	.517	.417
½	285	206	8.26	69	1.026	.435	.423
1½	333	243	9.72	143	2.76	1.09	.395
1⅝	370	281	11.24	155	3.26	1.36	.417
1¾	365	266	10.64	167	3.03	1.221	.403
1⅞	395	283	11.32	182	3.51	1.61	.458
1¼	393	280	11.20	120	2.81	1 05	.373
1	318	233	9.32	115	1.90	.85	.447
1¼	318	233	9.32	134	2.27	.985	.434
1	308	225	9.00	101	1.83	.717	.392
1	400	294	11.76	98	2.38	.912	.383
¾	304	274	10.96	85	1.79	.732	.409
1¼	464	339	13.56	119	3.31	1.27	.384
1½	413	303	12.12	139	3.44	1.33	.386
1⅝	465	337	13.48	168	4.14	1.77	.427
1$\frac{9}{16}$	442	323	12.92	152	3.58	1.52	.424
1½	453	331	13.24	137	2.77	1.41	.375
1½	434	317	12.68	130	3.61	1.3	.36
1	400	294	11.76	98	2.38	91	.382
1	436	826	13.04	96	2.60	1.03	.396
⅝	439	320	12.80	70	1.81	.72	.398

TABLE NO. 2.

YALE AND TOWNE WORM GEARING. WITH COLLAR THRUST BEARING.

Revolutions of Worm.	Velocity of Rubbing.	Horse-power received	Efficiency.	Coefficient of Friction.
45.5	72	.229	.347	.194
84	132.8	.545	.383	.162
88.7	140.3	.727	.393	.159
118	186.6	1.51	.425	.140
154	243.6	.993	.440	.132
195	308.5	1.27	.431	.136
226	357.5	1.957	.418	.144
275	435	2.81	.405	.152
285	450.8	2.83	.402	.154
315	498.3	2.96	.390	.160
333	526.8	3.45	.395	.158

TABLE NO. 3.

YALE AND TOWNE WORM GEARING. WITH BUTTON THRUST-BEARING.

Net W't in Pounds on Scale.	Rev's of Worm.	Rev's of Gear.	Reading of Dynam.	Rev's of Dynam.	Horse-power as per Dynam.	Horse-power as per Brake.	Efficiency.
1.20	128	5.12	.030	179	.247	.005	.020
1.25	175	7.00	010	242	.311	.008	.024
10.80	136	5.44	.092	188.3	.315	.054	.170
10.80	128	5.12	.030	180	.248	.058	.234*
16.52	165	6.60	.181	225.3	.472	.100	.212
16.54	196	7.84	.240	268.3	.636	.119	.171*
17.35	140	5.60	.203	192.3	.423	.092	.217
18.09	98	3.92	.204	134	.295	.068	.230
19.94	32	1.28	.226	44.7	.103	.023	.223
36.50	356	14.24	.393	462.7	1.432	.478	.334
37.66	175	7.00	.377	234	.706	.240	.339
39.06	310	12.4	.443	63	1.470	.444	.302*
39.07	136	5.44	.355	188.3	.549	.190	.346
39.25	103.7	4.188	.338	141.7	.402	.150	.373
42.61	108.3	4.33	.407	148.3	.468	.170	.363
42.96	34.7	1.388	.214	47	.106	.054	.509*
43.22	46.3	1.85	.443	63	.210	.076	.361
49.55	176	7.04	.280	242	.619	.290	.471*
55.40	196	7.84	.434	268.3	.882	.390	.442
64.18	128.3	5.08	.467	175	.602	.299	.496
84.77	129	5.16	.562	181.7	.707	.402	.568

* Doubtful.

TABLE NO. 4.

YALE AND TOWNE WORM GEARING. WITH ROLLER THRUST-BEARING.

Net W't in Pounds on Scale.	Rev's of Worm.	Rev's of Gear.	Reading of Dynam.	Rev's of Dynam.	Horse-power as per Dynam.	Horse-power as per Brake.	Efficiency.
20.50	168	6.72	.123	231	.2404	.1267	.301
36.92	155	6.20	.265	214	.5328	.2105	.395
37.25	168	6.72	.255	231	.5645	.2302	.408
41.21	100	4.00	.324	138	.3820	.1516	.397
43.08	170	6.80	.310	233	.631	.2695	.427
49.40	170	6.80	.347	233	.6705	.3089	.460
49.89	197	7.88	.365	279	.826	.3616	.438
52.70	214	8.56	.459	297	1.011	.4149	.410
55.07	139	5.56	.433	194	.537	.2876	.535*
58.88	85	3.40	.503	118	.426	.1841	.432
60.23	182	7.28	.452	253	.8534	.4039	.473
65.36	319	12.76	.459	441	1.502	.764	.509
65.72	168	6.72	.503	231	.8348	.4061	.486
65.90	168	6.72	.507	231	.8410	.4069	.484
68.06	174	6.96	.491	238	.8466	.4386	.518
105.00	121	4.84	.869	167	.892	.4673	.524
110.59	320	12.80	.936	442	2.500	1.6048	.642*
115.07	156	6.24	.891	215	1.171	.6603	.5644
115.34	354	14.60	.793	488	2.509	1.5019	.598
119.26	260	10.40	.853	359	1.8905	1.1406	.603
121.48	217	8.68	.912	299	1.6578	.9666	.583

* Doubtful.

CXCIX.

STANDARD PIPE AND PIPE THREADS.

BY GEORGE M. BOND, HARTFORD, CONN.

THE value of a system of pipe thread sizes which would be interchangeable, not only for the product of one particular pipe manufacturer but for all, has long been urged, and no doubt is fully recognized and appreciated, even if not actually adopted and practically carried out.

It was mainly through the efforts of the late Robert Briggs, C.E., that engineering principles were brought to bear upon this subject, and in proposing formulæ and tables for the dimensions of pipe and pipe threads, he endeavored at that time carefully to carry out these principles practically, in the manufacture of pipe and pipe fittings.

Since that time changes have been going on in the methods of manufacture, and the quantity manufactured being greatly increased, the demands of the trade now require pipe in such sizes and quantities as may have taxed the older methods and facilities to the utmost; so that, owing to the gradual change in size from what probably all manufacturers originally started, and the consequent variation in the sizes of dies used to cut the threads, the actual condition of the interchangeability of pipe threads is, at the present time, far from being satisfactory to consumers who find it necessary to buy their pipe in the open market.

In a letter received by the author from Mr. Briggs, dated March 29, 1882, reference was made to a paper he was soon to publish before the Institution of Civil Engineers, London, and in which he claimed as his the standard list of pipe dimensions as prepared by him when Superintendent of the Pascal Iron Works in 1862 and before.

The taper of the thread was then established as 1 in 32 each side, or $\frac{3}{4}$ inch per foot in diameter, up to and including 8-inch pipe.

The length of that part of the thread which was perfect at top and bottom (the angle of the thread being 60 degrees), and which

he termed the "complete thread," be expressed generally by the formula,

$$\text{Comp. T} = (4.8 + 0.8\,D)\,P$$

D being the outside diameter of the pipe, and P the pitch of the thread, equal to $\frac{1}{N}$, N being the number of threads per inch.

A list of these values is given later on in this connection.

The length of perfect thread, expressed by the formula, is followed by two threads imperfect at the top, the bottom only being complete, and finally, the remaining four threads, which are imperfect at both top and bottom, vanish at the surface of the pipe at this point.

In the illustration here shown (Fig. 105), and which is taken from a sketch contained in Mr. Briggs's letter, the section of one side of 2½-inch pipe indicates this varying character of the thread, and also the angle and the taper.

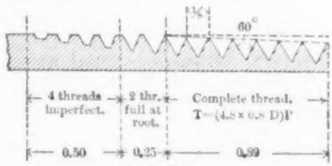


Fig. 105

Conditions for actual size 2½ inch Pipe Thread.
Taper ¾ inch per foot, or 1 in 32.
For whole length of all threads add 6 threads.

TABLE OF VALUES FOR COMPLETE THREAD DEDUCED FROM ABOVE FORMULA, WITH LENGTH OF ADDITIONAL 2 THREADS AS ABOVE.

Nominal Size of Pipe.	Actual Inside diam.	Actual Outside diam.	Length of Comp. Thread.	Length of Comp. Thread + 2 Threads.	Pitch of Thread = $\frac{1}{N}$.
⅛	0.270	0.405	0.19	0.264	27
¼	0.364	0.540	0.29	0.402	18
⅜	0.494	0.675	0.30	0.408	18
½	0.623	0.840	0.39	0.534	14
¾	0.824	1.050	0.40	0.546	14
1	1.048	1.315	0.51	0.683	11½
1¼	1.380	1.660	0.54	0.707	11½
1½	1.611	1.900	0.55	0.721	11½
2	2.067	2.375	0.58	0.757	11½
2½	2.468	2.875	0.89	1.138	8
3	3.067	3.500	0.95	1.200	8
3½	3.548	4.000	1.00	1.250	8
4	4.026*	4.500	1.05	1.300	8
4½	4.508	5.000	1.10	1.350	8
5	5.045	5.563	1.16	1.406	8
6	6.065	6.625	1.26	1.513	8

* 4.07, in Haswell's *Engineers' and Mechanics' Pocket Book*, 35th edition.

On the basis of these dimensions, and using the formula of Mr. Briggs for length of thread, the company with which the writer is connected, during the year 1882 made a set of thread gauges for all sizes of pipe from $\frac{1}{8}$ inch to 4 inches inclusive, using every possible precaution to avoid error, and with the greatest care taken to conform to the figures given for the outside diameter of the thread in the published list issued by Messrs. Morris, Tasker & Co. (Limited), from which these dimensions were taken, and in the angle, pitch, and taper of the thread.

In the course of time it became evident that pipe threads cut by the dies in general use would not enter the external or ring gauges, which were each adjusted so as to be a perfect fit on the standard reference gauge or plug, and which represented the data given in the list mentioned for each size pipe.

As an instance of this variation from a definite standard, one

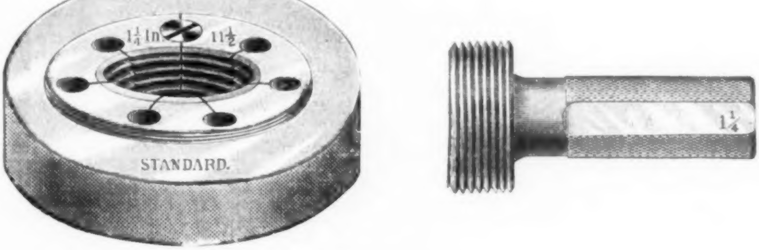


FIG. 106.

STANDARD PIPE-THREAD GAUGES.—EXTERNAL AND INTERNAL.

case may be cited in which the range of sizes, compared with these gauges, covered pipe threads from $\frac{3}{4}$ inch to 4 inches inclusive, and dies from $2\frac{1}{2}$ to 4 inches inclusive. In nearly every size there was shown a marked increase in the diameter of thread cut by the dies used by the manufacturers who furnished the pipe referred to.

The samples were sent to us for comparison by a corporation doing a large business in pipe and pipe fittings, the pipe they used being obtained direct from the manufacturers.

The gauges used were the external or ring gauges before referred to, and these were so adjusted to the standard plug that the end of the latter was exactly flush with the face of the ring.

Fig. 106 represents the style of gauge used. In the ring, provision is made for adjustment by having the inner ring split in several places, and a fine pitch thread (30 per inch) cut in the outer edge of the inner ring, which is slightly tapering in the thread A taper binding screw firmly holds this inner ring in its position by expanding it to a fit in the receiving thread of the outer ring.

The samples to be tested were short pieces of pipe, two of each nominal size, with thread cut on one end only.

They were marked No. 1 and No. 2; those marked No. 1 being from one prominent manufacturer, and No. 2 from another.

The following table will show the results of the comparison:

Size of Pipe.	Sample Number.	Relative Size as Compared with Gauge.
$\frac{3}{4}$	1	Entered $\frac{1}{3}$ thickness of gauge.
$\frac{3}{4}$	2	" $\frac{1}{2}$ " " "
1	1	" depth of one thread only.
1	2	" " " " " "
$1\frac{1}{4}$	1	" $\frac{7}{8}$ thickness of gauge.
$1\frac{1}{4}$	2	" $\frac{7}{8}$ " " "
$1\frac{1}{2}$	1	" $\frac{3}{4}$ " " "
1	2	" $\frac{2}{3}$ " " "
2	1	" within one thread of being flush with face of gauge.
2	2	" $\frac{3}{4}$ thickness of gauge.
$2\frac{1}{2}$	1	" within one thread of being flush with face of gauge.
$2\frac{1}{2}$	2	" $\frac{1}{2}$ thickness of gauge.
3	1	" within $1\frac{1}{2}$ threads of being flush.
3	2	" $\frac{2}{3}$ thickness of gauge.
$3\frac{1}{2}$	1	" $\frac{1}{2}$ " " "
$3\frac{1}{2}$	2	" less than $\frac{1}{3}$ thickness of gauge.
4	1	" $\frac{2}{3}$ thickness of gauge.
4	2	" $\frac{1}{2}$ " " "

In connection with this test of pipe threads, solid dies for $2\frac{1}{2}$, 3, $3\frac{1}{2}$ and 4-inch pipe were compared with the internal or plug gauges.

The $2\frac{1}{2}$-inch die permitted the gauge to enter and the end to pass $\frac{1}{16}$ inch beyond the face of the die, comparing favorably with $2\frac{1}{2}$-inch pipe, sample No. 1.

The 3-inch die had the same small variation, and shows it to be a little larger than the 3-inch pipe No. 1.

The $3\frac{1}{2}$-inch die was apparently correct, as the gauge entered flush with the face of the die when screwed in tightly, and hence did not compare favorably with $3\frac{1}{2}$-inch pipe, either No. 1 or 2. In the 4-inch die, however, the gauge passed $\frac{3}{16}$ inch beyond the face of the die, showing that it was more nearly the size of 4-inch pipe No. 1.

It will be seen from the above comparison, that with the exception of one or two sizes, there has been more or less increase of outside diameter of pipe from the figures originally adopted by the Pascal Iron Works, for the threads in every instance were full at the top and bottom, and certainly looked as if cut with dies which were in good condition. These dies were probably made to conform to the diameter of pipe as would be ordinarily found, and if made small enough to cut to the sizes represented by the gauges, would evidently weaken the pipe materially, besides imposing too much work upon them in removing the surplus metal.

Some manufacturers, in order to reduce the extra weight of pipe due to this increased outside diameter, have enlarged the inside diameter also, thus keeping the thickness of the pipe within a certain limit; in fact, even less than the original thickness, as the author has every reason to believe, making it practically impossible to cut the original standard thread on pipe as it is now manufactured.

It will be noticed from the comparison as shown in the table, that only the 1-inch and the $1\frac{1}{4}$-inch samples were alike, and while the $1\frac{1}{4}$ compared favorably, the 1 inch was so much larger than the standard that the pipe only entered the gauge one thread. The other sizes show more or less difference, and the comparison proves that the two samples of the same nominal size were not alike, and that the manufacturers evidently were not using the same standard for their dies.

This must certainly be a serious check to confidence in the interchangeability of pipe and fittings, especially by the parties having to rely on samples showing so much variation. This lack of confidence is keenly felt by those who handle pipe in large quantities, as they do not find it possible always to obtain their pipe from the same manufacturer, and even if this were possible, the chances are against its being uniform to a degree necessary in engineering operations of this kind, unless some united action

is taken by all manufacturers interested, and by their competing on the basis that each shall endeavor to use the *same* standard for reference in all pipe and pipe thread sizes.

One instance in which a non-interchangeability of pipe threads resulted in serious loss to the parties concerned, and which is authentic, may be mentioned here. A prominent corporation doing business not far from New York, undertook to execute a contract for fittings for a large factory in which extensive alterations in their water-supply system were to be made. The threads in the fittings were carefully cut to correspond with pipe which had been found to be quite uniform in size, and which was considered as fairly representing standard sizes in the thread. The fittings, when delivered, were found to be unsuitable for the threaded pipe already in the factory and with which they were to be connected. The consequence was the fittings could not be successfully applied, and were rejected.

In a case of repairs likely to be required in any large works where fittings are to be replaced or new joints made, the work is usually done at night, or possibly Sundays; it is then that this question of uncertainty in regard to practicable interchangeability becomes a serious matter. Often after working all night getting the old work out of the way, the new pipe or fitting is found to be too large, or perhaps too small, so that even with the advantages of the taper thread, and the proverbial resources and energy of the fitter, the joint cannot be made, or else some other change is necessary to preserve the original lengths of pipe and connections.

All that can otherwise be done is to replace the old fittings and try again at the next time of "shutting down," or by correcting the misfit at the cost of expensive delay. This is certainly not an ideal case, nor is it of rare occurrence.

In the introduction of their system of automatic sprinklers in cotton mills and other factories, the Providence Steam & Gas Pipe Company experience great inconvenience on account of this lack of uniformity, as it is often necessary to send from their works large quantities of pipe cut to short lengths, and which allows little variation in making up the connections. In this system for fire protection of mills, the greatest care is taken to know that every pipe and fitting is interchangeable. No chances are taken; but it often happens that new pipe must be screwed into old fittings, and then the trouble commences.

Dies will wear, and taps for fittings also wear, but all this variation is easily remedied, or at least can be held within reasonable limits, providing some definite standard is adopted and *followed*. A standard which shall be recognized, covering sizes for the outside and inside diameter within practicable limits, and also for the threads which the manufacturers cut upon the pipe thus drawn, can only become a reality and a success when all the larger companies agree to accept such a standard, and to faithfully inspect the pipe, using gauges which must in turn be carefully adjusted to standards reserved for this purpose only. A working gauge cannot possibly remain a standard always, for in this, as in work requiring a higher degree of nicety, such as arms and sewing-machine manufacture, ultimate standards of reference are absolutely necessary. These can be made and can be kept standard.

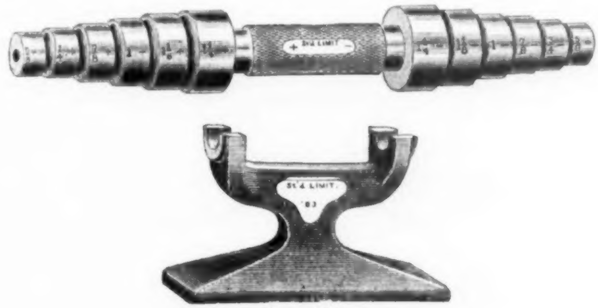


FIG. 107.

Gauges such as are represented in Figs. 107 and 108, and which are used for inspecting round iron intended for United States Standard bolts, with limiting variations above and below the standard size, were adopted by the Master Car-Builders' Association, June, 1883, and represent what can be done in gauging outside diameters within a practicable limit above or below the standard size.

Reference gauges like the set shown, serve to verify the working gauges, or to furnish standards to which worn gauges may be readily adjusted, and thus kept serviceable in spite of wear.

If round bar iron can be rolled within practicable limits, overcoming the "oversize" evil in this case, it seems evident that within limits that may be determined by the difficulties of pipe production to a standard size, the tendency toward larger outside diameters might be checked.

If the outside diameter can be secured within definite practicable limits, the inside diameter will then be determined by considerations of the proper thickness to withstand the internal stress, depending upon the purpose for which the pipe is to be used, with due regard for the weakening effect of the thread cut at the ends.

In sizes larger than 2 inches and including 6 inches diameter, the customary standard of 8 threads per inch seems rather too coarse for the average thickness of pipe as now made for ordinary steam and water pressures; it is certainly so in comparison with the pitch of the thread for the smaller sizes. In a recent conversation with Mr. J. H. Flagler, General Manager of the National Tube Works Company, he expressed himself in favor of the change for the larger sizes, if possible, to threads of a somewhat greater number per inch, and recommends, also, the

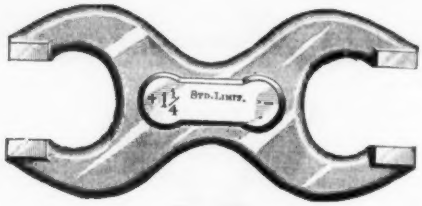


FIG. 108.

adoption of 11 per inch, instead of 11½, now used for pipe of 1 inch to 2 inches diameter inclusive.

This would correspond with the pitches of threads for all sizes from ½ inch to 2 inches inclusive as adopted by Whitworth, and which are used universally in England. It would certainly aid in simplifying the question of interchangeability in our own practice, and should American pipe find a market where English pipe is now exclusively used, any objection on this score could not be urged.

The great pipe lines conveying oil from the wells in Pennsylvania to the refineries in Pittsburgh, Philadelphia, Baltimore, Bayonne, and other points, are laid with pipe which is of special manufacture; the working pressure averages 1,200 lbs. per square inch, and hence the pipe is made of extra thickness, and lap-welded; it is 6 inches diameter for the three lines extending from Olean, N. Y., to Bayonne, N. J.—a distance of 300 miles—and the number of threads per inch cut on the ends of 18 feet lengths is 9,

being one thread more per inch or finer than is used for ordinary steam pipe of only $2\frac{1}{2}$ inches diameter. The ends and unions are made with a taper of $\frac{3}{4}$ inch per foot, and a special sleeve is used to connect the pipe, as the ordinary steam-pipe joints used on 4-inch pipe prior to 1877 would not resist the strain, and leakage caused considerable loss and inconvenience.*

Some discussion has arisen from the fact that while the taper of pipe threads is $\frac{3}{4}$ inch, the taps for fittings are often found to be 1 inch taper per foot. At first thought this might seem impracticable, but when we consider the relations of the fit under these conditions, the pipe being cut to a standard size, and the thread in the fitting also properly cut, we would find that this tends to produce a better joint, because the first "gripping" of the thread would be at the end of the pipe where it is the weakest, or where it would yield most readily, and in the fitting it would take place at its strongest part, which is inside, and would thus avoid the liability to split at the outside edge.

This gradual compression along the thread is more favorable to the conditions of a close fit for the entire length than if both had the same degree of taper, so that the practical difficulties, which are opposed to an extraordinary degree of nicety in pipe thread sizes, are in this way overcome, and good tight joints with the minimum amount of force required to make them so, are the result. The variation from the taper of the pipe in so short a length (about 1 inch for a 2-inch pipe) is almost inappreciable, but this apparently unimportant difference will aid in securing a steam tight joint, for reasons that it would seem are practically sound, and while the mathematical calculations as to the exact strains involved during this gradual "flow of metals" might easily be deduced, assuming a value for the coefficient of friction in this instance, it is hardly necessary to enter upon them here.

It might be well to include in the investigation of this subject, the most desirable standard for the diameter and threads of drawn brass pipe and the threads of brass fittings. This class of work, and the value of material involved, evidently warrants such a suggestion, not only on the broad principle of uniformity and interchangeability, but in order to establish and put on record the best possible conditions of size and thread which will cover the

* See *London Engineering*, July 31, 1885; article, "The Transportation of Petroleum."

range of the various diameters and thicknesses of brass pipe ordinarily manufactured.

Should the right sort of action be taken, in this question of practicable uniformity of pipe and fittings, by the manufacturers who have it in their power to shape the means to accomplish so desirable an end, the manufacturers of fittings or of pipe taps and pipe dies would certainly give to it their hearty support, by working even more closely to accepted gauges than may be possible to draw the pipe; for although the difficulties and differences as they now exist are less serious than formerly resulted from the lack of uniformity in bolts and nuts, and which is now happily settled for the purpose for which the Sellers, or United States Standard thread was intended; yet the value of a uniformity in all pipe sizes is of sufficient importance to be urged upon the attention of those who would be most benefited by its introduction, which includes manufacturers and consumers alike.

DISCUSSION.

Mr. George Schuhmann.—I would like to state that at the Reading Iron Works, which is the company with which I am connected, we have a complete set of standard gauges for pipe threads, which were made by Morris, Tasker & Co. while Mr. Briggs was their superintendent, and Mr. Briggs, after leaving there, was the consulting engineer of the Reading Iron Works up to the time of his death. We use those gauges, just as Mr. Bond recommends them to be used, for ultimate standards only. For reference in the thread cutting department as well as in the tool shop we use duplicate gauges, and we refer to the original standards only when the duplicates begin to wear.

The threads cut on our pipe are made to conform to those standards, the same as twenty years ago, and we have not increased the diameter. Of course we have had our complaints about our threads not corresponding with certain fittings, but on referring to the standards we found almost invariably that the fittings were wrong, not the pipe. As a rule, each piece of pipe which goes into the market has a coupling screwed on one end, and as the pipe is afterward tested under hydrostatic pressure varying from 300 to 2,000 lbs. per square inch, the inspector has a good control of each and every joint, and if the joint leaks or the coupling does not fit properly, the matter is at once referred to the gauges, to see whether

the fault lies with the die or with the tap. Now, as I understand it, the fitting makers do not use such control. The fittings are taken out of the tapping machine and boxed up for shipment, and it is left for the user to find out whether they are standard or not.

Another thing I would like to call attention to is the defect in the construction of the ordinary wrought-iron coupling sleeves, or sockets as we call them, for while all the pipe threads are taper the sockets are tapped parallel by running the tap all the way through. Now it is very evident that if we screw a taper pipe into a straight socket, a few threads only will have to stand the whole strain. It is claimed that in screwing on, the sockets will expand to match the taper of the pipe, but it does so only to a very limited extent and the strain will remain on a few threads only. In a line of piping screwed together with the straight socket, the joint is decidedly the weakest part, so that strengthening the joints means strengthening the whole line. They found it out very quickly at the oil wells where the pipes are put to very severe tests, for instance in letting tubing down a well, often over 2,000 feet deep, the whole weight is carried by the top joint, and of course the straight sockets would not stand it, so they ordered the threads longer for this class of goods, and insisted on having the sockets tapped taper. The great pipe lines from the oil region to tidewater, referred to by Mr. Bond, have all taper sockets. These sockets are first straight tapped by running a tap through, and are then tapered by running a taper tap first in one end and then in the other end, which of course adds considerably to their cost of manufacture. A great deal of thought has been given to the subject of devising some means to manufacture them by some cheaper method, so that they could be put on common pipe also, without adding to the cost. Mr. Briggs made some interesting calculations as to how much power would be required to expand the straight tapped sockets by means of a mandrel with grooved rollers, similar to the Dudgeon flue expander, but we did not adopt this method. We made some experiments with a mandrel cut in sections, which had the proper taper thread on the outside and a tapering hole in the center, and we expanded the sockets by forcing a taper pin into the hole of the mandrel by means of a hydraulic press. We found it took more pressure than we anticipated, and the sockets would not expand uniformly, owing, I suppose, to the variation in the flow of the iron. I have heard that some Western party has adopted this method of expanding now, but I presume it is keeping an inspector busy to

assort the sockets. The socket which comes nearest to what is wanted is the so-called "Morse recessed socket." In outside appearance it is the same as the common socket, but it has a recess or chamber in the center which gives room for the cuttings and allows the socket to be tapped from both sides at the same time, the same as other fittings. The recess is rolled in the iron and forged in the socket, and we use collapsing taps to save the backing out, which enables us to make them as cheap as the common socket, that is for all sizes from $2\frac{1}{2}''$ upward. As the thread of these sockets has the same taper as the pipe, the strain is distributed over the whole socket instead on a few threads only, and in fact it is making as perfect a screw joint as it is possible to make it. We put these sockets on common pipe, when so desired, without extra charge.

As regards Mr. Bond's suggestion about making a new standard, I agree with him that eight threads per inch is too coarse for $2\frac{1}{2}''$ and $3''$ pipe, and the oil well people want finer threads and longer threads than the Briggs standard calls for. A lot of $6''$ and $8''$ drive pipe which we sent to the Russian oil fields recently had threads $4\frac{1}{2}''$ long and sockets $10''$ long, but I consider this as going to the other extreme. In order to accommodate the oil well people, the pipe manufacturers formed an association and decided on a standard for oil goods—in 1878, I think. They adopted ten threads as the standard for line pipe; they are making it with nine threads to the inch now, but the change was made, I suppose, because some customers wanted it. The so-called oil-well casing is very light pipe, and is used to keep the water out of the wells. The pipe manufacturers adopted fourteen threads as a standard for all casing, no matter what diameter, while our Canadian neighbors insist on ten threads for their casing.

In order to keep the outside diameter of the pipe as near standard as possible, we use gauges similar to the one illustrated in Fig. 108, and as soon as the first pipe comes out of the finishing rolls, the finisher cools one end off and tries his gauge, and if the pipe is not the right size, the rolls are adjusted until it is right, and the outside diameters of our pipe made to-day are still the same as the original Pascal Iron Works standard.

Mr. Grinnell.—I have been indirectly instrumental in having this subject brought to the attention of the Society, and I would gladly go into the details of the question, in the matter of the manufacture of pipe to standards, but Mr. Stetson, I see, is on the list of gentlemen who are to speak on the subject, and therefore I

will leave all details and simply say a word to call the attention of the Society to the importance of this question. As every one here present knows, the matter of standards is entering more and more largely into all our mechanical work, and from the slightest consideration you will all agree with me that any tendency to depart in this manufacture of pipe from a standard which secures good and reliable work causes loss of time and even injury to life and limb. Whatever may be said in reference to the present condition of this matter, as it now rests with the manufacturers of pipe, those of us who are using pipe in large quantities know by actual experience that there is a wide divergence in the size of pipe, and especially in the larger sizes. I can commend Mr. Bond's paper to you most earnestly; I have considered the matter with him before the paper was prepared, and think he has covered the ground thoroughly, or sufficiently, at any rate, so that in my judgment some practical result may come from our consideration of the subject. It would be a good work on the part of the Society to appoint a committee which should confer with a committee of pipe manufacturers to the end that a more rigid and a closer condition of interchangeability of pipe threads and fittings may be possible in the future. A good work has certainly been done in bringing the manufacture of round rolled iron and of bolts and nuts to a standard, and I think this Society can do equally good work in improving the condition of pipe manufacture.

Mr. W. J. Baldwin.—Of the necessity of a standard for threads for wrought-iron pipe there is no question. Evidently, there may have to be two standards, one for the well-tuber, and one for the steam, water, and gas fitter. The steam-fitter evidently wants a finer thread than present so-called standards. An inch, inch and quarter, and an inch and half pipe, and probably the two-inch pipe, are all 11½ threads to the inch. A thread as coarse as 11½ cuts too far through these pipes; that is through pipes that are lap welded. The butt welded pipe seems to be a little thicker as it comes to the user. Consequently, where the present thread might suit very well on a thick pipe, at present it cuts too far through the sizes I have mentioned, and therefore I would argue against the use of eleven threads to the inch, as it tends to aggravate a difficulty which now exists. Evidently one-inch pipe would want fourteen threads to the inch, the same as three-quarter pipe now has. I think that would be an improvement for the steam-fitters. One and one-quarter inch would probably want the same thread. One reason

which the steam-fitter finds for his fittings and pipe not generally coming together, is that the manufacturers of fittings—some of them, not all—use a long straight tap. They grind it on the point to shorten and sharpen it. They make a long tap, and as it wears they simply take it to the grindstone or emery wheel and shorten it. The result is, the internal threads in fittings are parallel at the top or point of the V; the bottom of the thread is not. This is an objection, of course. A great many manufacturers of brass goods also use a straight tap. They depend on the brass fittings or valves spreading and taking the shape of the pipe.

Mr. Stetson.—I have not intended to take much part in this discussion, as it seems to me that I have not much concern in this pipe matter. It is among the pipe makers and not the tool makers. We usually make exactly whatever is demanded. Some of the remarks have called to my mind certain facts, and one is that in our manufacture of the taps suited for straight fittings, we have had so much trouble that we will make no more of those taps unless the man sends the fitting, because the range of sizes are so large that we have not been able to establish a standard unless it is given to us by the party ordering; so that I should suppose there was an opportunity for improvement there. In regard to the changes of the thread, it is very difficult to make changes of that kind, and the Sunday work would be much interrupted by the Saturday night profanity when it was found that your new fittings were 11, and not $11\frac{1}{2}$, or 9, and not 8. I acknowledge that that in the hands of the ordinary pipe-fitter would not make much difference. It shows how we lean on one another. I had thought that the service to which the pipe of the water sprinkler was put was the most critical of any that came under my observation, and we had accepted the standards pretty much as they gave them to us, and their fittings are certainly always creditable; the threads are perfect and indicate that they keep their tools in very good condition. The fittings which are sent to us very frequently, would indicate that discount on them was moving up toward 85 per cent. In regard to the brass pipe, the difficulty comes in there that it is drawn with a great many thicknesses for the position into which it is to be put. Take water-pipes in cities or in places where the pressure increases and decreases. While the fitters there, I suppose, have generally charged for the heaviest brass pipe which is used about the fitting of the house they do use very great differences of thickness of pipe, and it will be satisfactory to us if the

diameter of those pipes and the number of threads could be known. So that there is a want of overhauling of this subject, and I would suggest that it would be well for us to get at it as soon as possible, as the English are thinking of adopting our system and we ought to set them a good example. Their pipe fittings are tapped straight. Their taps are straight, and they certainly must do better work than we do or they couldn't get along at all. We have had the samples from Whitworth, and they must work to the size of their pipe better than we do.

Mr. W. J. Baldwin.—If our pipe standard agreed with that of English makers, our trade in wrought-iron pipe and fittings with South America and the Spanish speaking Republics of Central America would certainly increase. This difference of gauges is keeping our iron pipe out of these markets. These people commenced with English pipe and English fittings, and they have to purchase English pipe and fittings to make their alterations and keep up their standard.

Mr. Bond.—It was meant simply to state that in those oil-pipe lines, even in pipe of such large diameter, the thread is finer than it would be in the old system of $2\frac{1}{2}$-inch pipe; so that it would show that they made the thread finer from reasons of economy, safety and efficiency, and it would seem as though the change to a finer thread would be reasonable to ask, in case any change should be made.

Two pieces of 2-inch pipe are exhibited which were sent to us by the Providence Steam and Gas Pipe Company. They are both nominally intended for standard 2-inch pipe. The variation in size is over $\frac{1}{16}$ of an inch in the outside diameter, and nearly an eighth of an inch in the inside diameter, and yet they were both threaded with the same die. They both screw in the gauge nearly alike, and though rather larger than standard in the thread, each might pass for 2-inch pipe. They both came from the same manufacturer.

Mr. W. J. Baldwin.—I used a large quantity of $2\frac{1}{2}$-inch pipe where 2 inch would be large enough for the purpose of conveying the water or steam, simply to escape the danger from difficulty with the thin size, because $2\frac{1}{2}$ inch is a thick pipe. The thread is eight to an inch, but there was sufficient metal left to prevent a weak link in your chain. The lap-welded 2-inch pipe is the poorest pipe in the system; it is of very irregular thickness and outside gauge, and is approached only in this respect by the $1\frac{1}{4}$ inch, which is very irregular.

Mr. Bond.—In pipe received from the same company there was a difference of $\frac{3}{16}$ of an inch.

Mr. Baldwin—Would not the thickening of pipe generally be an advantage? With the present thickness is wanted a finer thread, or else the pipe should be thickened, if the thread is unchanged.

Mr. Bond.—The pipe, of course, of $2\frac{1}{2}$ inches diameter and over, has eight threads per inch, and that is very much coarser, and this change to eleven threads, if it includes sizes over $2\frac{1}{2}$ inches diameter, would evidently materially increase the number of threads.

Mr. Baldwin.—Two inches would be coarser than it is now?

Mr. Bond.—Yes, sir.

Mr. Baldwin.—That would be a disadvantage from my standpoint, because it would get tapered.

Mr. Bond.—It would be an advantage to some extent in doing away with fractional threads, but what the manufacturer wants is to keep down the weight, as he sells pipe by the foot. It is the interchangeability of the ordinary pipe thread that is of great importance. If there are different kinds of pipe, like that used for oil-pipe lines or the casing for oil wells, that should not interfere with the other question. Twelve threads per inch might be more practicable.

Mr. Schuhmann.—The 2″ oil tubing that goes down the well is actually 2″ standard pipe, except the thread. They are tested with 2,000 lbs. pressure, and when hanging down a well, say 2,000 feet deep, the weight causes a tensile strain on the upper sections of over 6,000 lbs. per square inch. So there is a large margin for safety when used for ordinary steam fitting of, say a 100 lbs. pressure, even if common pipe should be a little lighter than the standard.

Mr. Baldwin.—The question with the fitter is not so much the resisting of internal pressure by this thin pipe, but its standing the bending and torsion to which it is subjected. The likelihood of breakage close to the fitting in use, is another of the principle objections to it.

Mr. Grinnell.—I would like to say a word with reference to the practice in England. I had occasion some two years ago to form a connection with a very prominent engineering firm in Manchester, and the senior member of that establishment was at my place, and the character of the work in this country in the matter of steam fittings was quite a revelation to him. And I would say that in their work in England, for a 2-inch pipe and larger they use a cast

iron flange pipe in length not exceeding seven feet put up and bolted together, and the use of a larger pipe than 2 inch of wrought iron is very rare. It seems to any one acquainted with the business here that they are very far in our rear in this matter. We are using pipe there constantly for steam connection, for large engines and boilers, of **10** and **12** inches in diameter. It is very much lighter and stronger, and less liable to strain joints and cause leakages than cast-iron pipe. The result of this interview between this party and myself was that they ordered of us a pipe-fitting tapping machine, and they are now trying to inaugurate in England the use of cast iron fittings in the place of their wrought iron fittings. Their wrought iron fittings are made by hand, formed up and welded and then tapped with a straight tap. We could tap on that machine, which we sent over there, six times as many elbows in a given time as they could tap, and of others a proportionate number, and I state it as a matter of interest to this Society that it is worth our while to co-operate with some establishments over there and see if a uniform system of threads and fittings cannot be brought about. It certainly would be greatly to their benefit, if not to ours, for they are in the dark ages as to this matter of the use of wrought-iron pipe and fittings.

CC.

STEAM ENGINE TESTS.

MADE IN THE MECHANICAL ENGINEERING LABORATORY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BY C. H. PEABODY, BOSTON, MASS.

THESE tests were made as a part of the regular laboratory work of the third and fourth year classes in mechanical engineering and electrical engineering, primarily for instruction. They were arranged in series, a the conditions of a series, except one which was chosen for a variable, being as nearly constant as possible, with the expectation that each series, when complete, would give definite information as to the effect of changing that variable. Two theses, with these tests for the subject, have been presented by students in the classes of 1884 and 1885, each arranging and investigating the tests so as to develop special points of interest or to draw such inferences as seemed warrantable.

The engines tested were a Harris-Corliss engine of eight inches diameter and twenty-four inches stroke, with a normal speed of sixty revolutions per minute, and nominally of sixteen horse-power; and a Porter-Allen engine of ten inches diameter and twenty inches stroke, which at 230 revolutions per minute, is rated at eighty horse-power. The latter is commonly run at about 200 revolutions per minute, driving shafting and the ventilating fan in the New Building, and exerts about twenty horse-power; during the tests additional power was applied at a friction brake.

The Harris-Corliss engine was run at powers varying from two to eighteen horse-power, at different speeds, and both with a cut-off and a throttling governor.

The steam for running the engines during a test was drawn from the general supply (used also for heating the buildings) generated by two horizontal, externally fired, tubular flue boilers, of the common type. The steam pressure was commonly seventy-five pounds to the square inch, and the amount of priming determined

by careful tests by a Barrus calorimeter was from one-half of one per cent. to two per cent. These determinations were not made in connection with the engine tests, though under the same conditions and during the course of the tests, and therefore give the quality of the steam used.

The exhaust steam was condensed at atmospheric pressure in a surface condenser, and weighed in a tank. Power was applied by a sensitive friction brake, cooled by water circulated through a coil cast into the rim of the pulley, and driven by a belt from a line of shafting. The brake was driven by either engine as required.

The tests usually lasted from an hour and a half to an hour and three-quarters, the time for the exercise being two hours, including starting and stopping. At each five minutes a gong was struck at which the counter and steam pressure gauge were read, indicator cards were taken at each end of the cylinder, and the condensed water was weighed in the tank. In the intervals between observations, the areas of the diagrams on the cards were measured with planimeters, the initial pressure was measured, also the pressures at cut-off, release and compression, the per cent. of cut-off was determined, and the mean effective pressure, and mean back pressure, if any, were computed. The release and compression of each engine was constant during a test, and was assumed at some convenient per cent. of the stroke which insured that the valve was then closed. The cut-off was so nearly constant during a test with the throttling governor on the Harris-Corliss engine that it was treated in the same way; when the variable cut-off was used on that engine, and when the Porter-Allen engine was tested, the cut-off was assumed as nearly as could be estimated from the card, immediately after the valve closed.

These data, together with the barometric pressure and the temperature of the engine-room, were recorded on a printed form or log, while the test was in progress. The piston displacement and clearance for each end of the cylinder were also given.

From the data given, were calculated and recorded on the log the following results of the test:

(*a*) The revolutions per minute; the water per revolution and the water per horse-power per hour, given by the tank measurement; the indicated horse-power of each end of the cylinder, and the total indicated horse-power.

(*b*) The average was found, for all the cards taken at each end of the cylinder, of the initial pressure, the apparent cut-off, the pressure at cut-off, release and compression, the mean effective pressure and the mean back pressure, and these averages were assumed to correspond to an ideal pair of cards representing the entire test.

(*c*) The volume, including clearance, which was filled with steam, at cut-off, release and compression, being readily determined, together with the average indicated pressure, the weights of steam in the cylinder at those points were found by the aid of steam tables. This calculation was made for each end of the cylinder separately, and the sum of the weights for the two ends, at a given point of cut-off, was called the weight per revolution for that point. It is properly double the average weight per stroke. To the weight of water per revolution given by the tank was added the weight of steam per revolution at compression, and the sum was called the mixture in the cylinder. The weight of steam at cut-off and at release was divided by the mixture in the cylinder, and thereby the per cent. of the mixture shown by the indicator at those points, was found.

Thus the tests were made to show as special points of interest, the water per horse-power per hour actually used, the amount shown by the indicator, and the condensation and re-evaporation in the cylinder. The tests here reported are numbered from 1 to 49 in the order in which they were made. As, however, that order was chosen for the convenience of the work in the laboratory, and to give proper variety of instruction to the different classes, the order in which they are presented is changed; they are grouped in four series, and each series is arranged in order of the horse-power. A few of the tests are omitted because in them some of the conditions assumed to be constant, varied.

Abstracts of the several series are given in Tables I., II., III., and IV.

TABLE I.

TEST ON HARRIS-CORLISS ENGINE USING AUTOMATIC GOVERNOR.

No. of Experiment.	Duration of Experiment.	Revolutions per minute.	Boiler pressure by gauge.	Initial pressure.		Mean back pressure.		Mean apparent cut-off.	Release.	Compression.	Mean effective pressure.		Indicated horse-power.	Water per H. P. per hour by tank.	Per cent. of the mixture of water and steam in the cylinder accounted for by the indicator.			Re-evaporation in the cylinder per hour.	Re-evaporation in the cylinder per H. P. per hour.
															Cut-off.	Release.	Difference.		
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	h.—m.		*lbs. per sq. in.*	*C. E.*	*H. E.*	*C. E.*	*H. E.*	*per cent.*	*per cent.*	*per cent.*	*C. E. lbs. per sq. in.*	*H. E.*		*lbs.*				*lbs.*	*lbs.*
1	1—35	68.55	59.95	51.01	46.9	....	...	1.49	100	2.2	6.02	2.51	1.781	149.29	17.0	67.7	57.7	144.9	81.36
2	1—25	68.80	71.10	65.0	47.6	....	....	0.98	100	2	8.70	2.96	2.44	112.9	16.7	66.9	50.2	148.97	61.07
3	1—45	66.04	71.90	67.26	63.3	....	....	10.10	100	2	22.23	18.34	8.17	51.83	32.5	63.6	31.1	145.25	20.25
18	1—25	71.14	73.83	70.75	67.1	....	....	7.8	98	2	23.16	16.06	8.38	48.05	32.48	67.69	35.21	149.97	17.893
9	1—40	67.14	73.57	70.49	70.33	....	....	9.6	98	2	24.50	17.57	8.60	45.73	35.5	65.3	29.8	123.10	14.313
32	1—25	63.17	49.94	48.72	48.77	....	...	31.6	95	2	26.26	28.83	10.49	40.73	61.21	72.81	11.6	51.29	4.889
4	1—20	63.80	70.60	63.67	66.7	....	....	26.35	100	2	36.30	32.02	13.27	41.23	47.7	65.2	17.5	99.14	7.47
16	1—20	65.85	73.95	70.67	70.16	....	...	22.15	98	2	38.16	32.20	13.93	38.19	49.43	66.42	16.99	93.96	6.747
31	1—15	62.00	69.65	69.59	69.69	....	....	34.1	98	2	41.48	42.41	15.67	36.35	63.36	69.97	6.61	38.59	2.463
17	1—20	60.20	72.97	71.47	70.17	....	....	35.90	98	2	51.41	40.44	16.62	37.82	59.97	78.36	8.39	54.44	3.275
33	1—10	61.81	73.14	73.30	72.59	....	....	40.0	95	2	48.86	50.25	18.46	34.69	67.47	72.75	5.28	34.61	1.875
6	1—30	62.89	72.20	66.4	65.0	22.5	21.8	35.25	100	1.6	28.4	17.9	8.87	63.28	63.9	80.1	16.2	97.34	10.98
7	1—25	62.37	74.20	67.8	68.7	17.4	16.17	41.50	100		34.9	28.6	12.06	53.47	66.2	78.9	12.7	86.30	7.15
8	1—45	62.00	72.30	69.3	65.3	....	....	46.20	100		41.7	30.6	13.65	50.12	68.3	78.1	9.8	69.58	5.096

TABLE NO. II.

TESTS ON HARRIS-CORLISS ENGINE AT DIFFERENT SPEEDS.

No. of Experiment.	Duration of Experiment.	Revolutions per minute.	Boiler pressure by gauge.	Initial pressure.		Mean back pressure.		Mean apparent cut-off.	Release.	Compression.	Mean effective pressure.		Indicated horse-power.	Water per H. P. per hour by tank.	Per cent. of the mixture of water and steam in the cylinder accounted for by the indicator.			Re-evaporation in the cylinder per hour.	Re-evaporation in the cylinder per H. P. per hour.
															Cut-off.	Release.	Difference.		
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	h.—m.		lbs. per sq. in.	C. E.	H. E.	C. E.	H. E.	per cent.	per cent	per cent.	C. E. lbs. per sq. in.	H. E.		lbs.				lbs.	lbs.
36	1—25	31.612	74.14	72.99	64.49	....	....	1.29	95	2	8.00	5.73	1.30	110.434	19.809	64.849	45.04	67.981	52.135
35	1—25	23.0	75.02	73.61	74.36	....	....	2.80	95	2	11.74	10.80	1.56	87.521	20.287	55.305	35.018	49.571	31.763
37	1—25	30.62	75.47	75.8	75.02	....	....	6.6	95	2	19.79	17.17	3.41	59.174	28.511	57.680	29.169	60.833	17.862
40	1—25	35.79	71.01	69.89	69.81	....	....	7.6	95	2	17.92	15.85	3.64	55.945	32.476	63.998	31.522	66.766	18.341
39	0—40	35.98	76.22	76.77	76.39	....	....	6.1	95	2	19.05	15.85	3.77	52.856	31.845	62.689	30.844	64.002	16.957
41	1—25	50.40	68.25	67.31	68.06	....	....	6.4	95	2	15.75	13.29	4.40	52.665	35.032	69.268	34.236	83.378	18.937
38	1—25	35.71	73.97	7.4	74.08	....	....	10.5	95	2	22.92	22.27	4.86	53.128	34.041	57.794	23.753	63.223	13.010
42	1—25	48.56	71.11	80.06	70.55	....	....	14.8	95	2	26.56	25.39	7.60	43.808	43.819	62.005	18.186	62.552	8.230
20	1—20	33.39	72.42	70.76	70.6	9.352	7.62	9.5	98	2	19.91	13.88	3.39	78.24	26.939	61.258	34.309	96.092	28.357

TABLE NO. III.—TESTS ON HARRIS-CORLISS ENGINE WITH THROTTLING GOVERNOR.

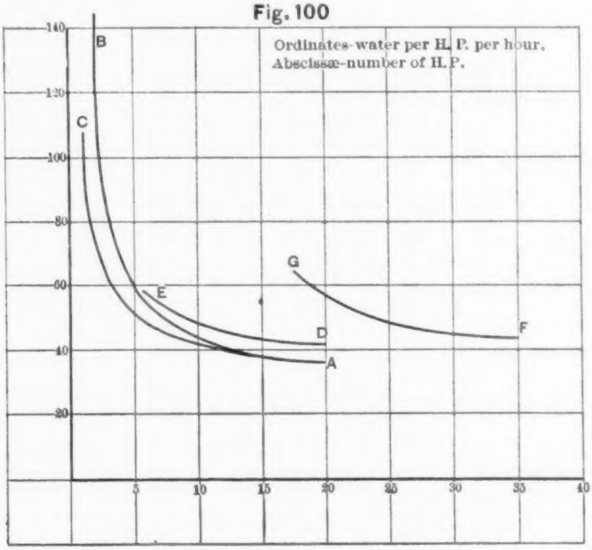
No. of Experiment.	Duration of Experiment.	Revolutions per minute.	Boiler pressure by gauge.	Initial pressure.		Mean back pressure.		Mean apparent cut-off.	Release.	Compression.	Mean effective pressure.		Indicated horse-power.	Water per H. P. per hour by tank.	Per cent. of the mixture of water and steam in the cylinder accounted for by the indicator.			Re-evaporation in the cylinder per hour.	Re-evaporation in the cylinder per H. P. per hour.
															Cut-off.	Release.	Difference.		
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	h. m.		*lbs. per sq. in.*	*C. E.*	*H. E.*	*C. E.*	*H. E.*	*per cent.*	*per cent.*	*per cent.*	*C. E. lbs. per sq. in.*	*H. E.*		*lbs.*				*lbs.*	*lbs.*
15	1—40	68.25	76.09	48.93	51.95	....	....	18.18	98	2	12.49	16.45	6.019	57.013	40.2	70.8	30.6	111.335	18.497
22	1—20	64.84	75.44	65.31	65.06	....	....	12.94	98	2	18.46	27.24	8.954	45.839	38.436	67.823	29.387	126.453	13.0
25	1—20	69.59	75.6	51.96	50.12	....	....	30.66	98	2	21.69	22.61	9.288	47.039	52.316	69.980	17.664	81.056	8.727
23	1—25	67.18	75.8	66.9	65.4	....	....	18.6	98	2	22.6	30.1	10.689	44.575	43.647	65.919	22.272	110.66	10.353
24	1—20	70.73	76.	67.9	63.8	....	....	24.3	98	2	31.68	31.67	13.497	36.898	53.426	72.896	19.470	101.338	7.101
26	1—5	67.57	72.5	61.7	59.6	....	....	43.35	98	2	38.11	37.88	15.466	42.554	57.947	69.916	11.969	80.905	5.231
14	0—55	65.05	75.8	65.2	64.6	....	....	40.4	98	2	38.9	41.2	15.87	38.97	59.8	68.4	8.6	55.0	3.468
29	1—25	69.74	73.90	60.5	56.7	....	....	63.8	98	2	48.39	37.48	18.003	41.464	71.128	74.366	3.238	24.793	1.377
28	1—20	68.01	72.69	65.88	63.2	....	....	61.6	98	2	50.39	46.55	19.849	40.940	69.00	72.557	3.557	29.610	1.492
27	1—30	66.93	73.42	66.39	65.72	....	....	61.3	98	2	55.04	47.48	20.033	41.115	70.426	74.518	4.092	34.441	1.719

TABLE NO. IV.—TESTS ON PORTER-ALLEN ENGINE.

44	1—15	203.13	74.87	73.41	71.78	....	....	10.9	90	10	14.76	7.58	17.802	63.957	47.926	73.371	25.445	378.36	21.253
48	1—25	200.48	74.58	74.57	72.75	....	....	19.5	90	10	19.10	13.74	25.828	46.399	65.766	81.236	15.470	239.41	9.269
46	1—10	202.1	75.48	73.87	75.13	....	...	22.2	90	10	18.46	14.42	26.069	49.008	68 100	79.393	11.293	184.38	7.073
49	1—25	198.2	73.25	72.97	73.39	.	. . .	24.1	80	10	19.55	15.45	27.216	42.244	76.816	85.208	8.392	126.514	4.649
47	1—15	197.39	74.0	72.12	72.47	....	....	27.2	90	10	24.39	18.09	32.890	44.968	69.831	72.380	2.549	45.975	1.398
45	1—15	200.11	73.66	73.27	73.31	....	...	27.6	90	10	24.42	20.21	35.039	43.318	70.298	72.340	2.042	37.964	1.083

The first series was made on the Harris-Corliss engine at normal speed, and controlled by the automatic cut-off governor. The cut-off varied from $\frac{98}{100}$ of one per cent. to 41 per cent. of the stroke, and the indicated horse-power varied from 1.78 to 18.46. The notable feature is the large amount of water per horse-power per hour at small powers, amounting to 149.29 pounds for the smallest power. To show this effect more clearly the curve A B of Fig. 100 has been drawn with the pounds of water per horse-

Fig. 100



power per hour for ordinates, and the number of horse-power for abscissæ.

The tests 6, 7, and 8, placed at the bottom of Table I., were made with considerable back pressure, applied by throttling the exhaust steam between the engine and the condenser. So far as may be, considering the small number of tests, they show the disadvantage of excessive back pressure.

The second series was made on the same engine with the automatic governor, but at speeds varying, in different tests, from 23 to 50.4 revolutions per minute, the speed for each test, however, being nearly constant. These experiments are represented by the curve A C, Fig. 100, and show the curious fact that slow speeds show better economy at small powers than high speeds. For larger powers the curves coincide for some distance. If the mean

apparent cut-off be chosen for the abscissæ, as in Fig. 101, all the tests of both series fall very near the same curve, indicating that, with this engine, the efficiency increases with the length of cut-off up to 45 per cent. of the stroke, and that it appears to depend principally on the length of cut-off. It should be said, however, that at very slow speeds the fly-wheel was insufficient, and that the engine would not pass the dead centers if much power was applied, consequently the tests that varied greatly from normal speed were all at very small powers. The third series was also on the Harris-Corliss engine, but the automatic governor was disconnected, the cut-off for each test was fixed, and the engine was controlled by a Huntoon throttling governor. The series is represented by the curve D E in Fig. 100, which shows that in this series the efficiency was in all cases less than that given by the automatic governor. The initial pressure in the cylinder was usually about ten pounds less than the boiler pressure, a sufficiently narrow margin on which to govern.

Fig. 101

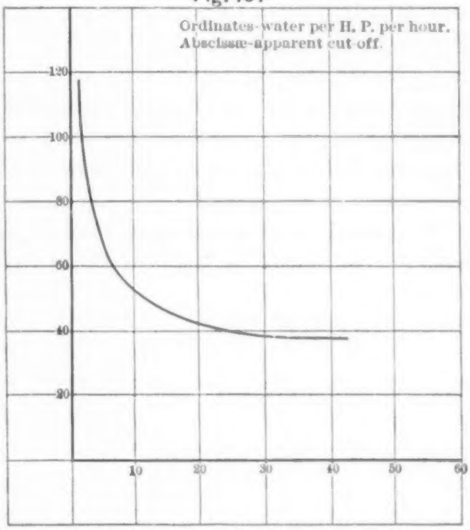
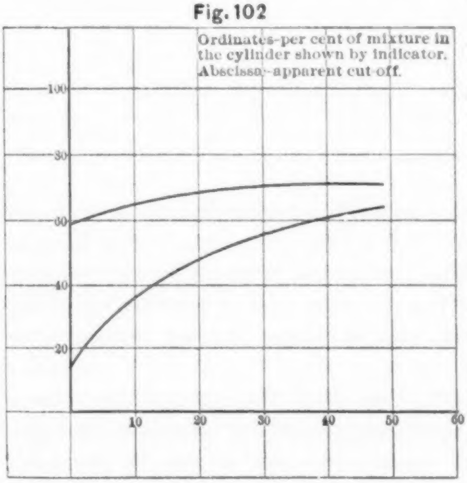


Fig. 102

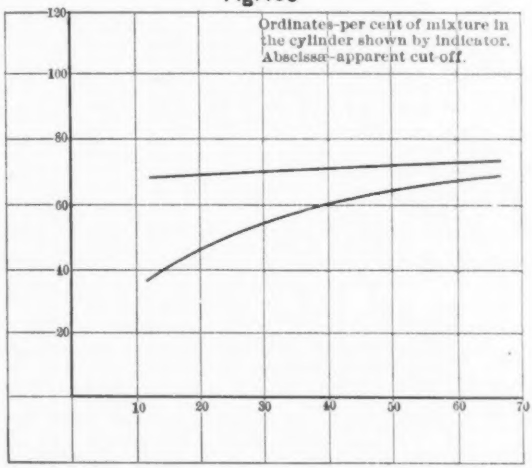


The fourth series was made on the Porter-Allen engine while doing its usual duty of driving the shafting and apparatus in the

mechanical engineering and electrical engineering laboratories, and of ventilating the New Building. Additional power was applied at the friction brake used in connection with the tests on the Harris-Corliss engine, giving a range of from 17.80 to 35.04 indicated horse-power. The water per horse-power per hour is represented by the curve F G in Fig. 100. The tests show the great disadvantage of running an engine at a fraction of its proper power, though the considerable back pressure occasioned by exhausting into the heating system of the New Building, materially increases the amount of water per horse-power per hour used.

The large condensation and consequent re-evaporation in the cylinder is shown in each of the abstracts of the tests, by the

Fig. 103

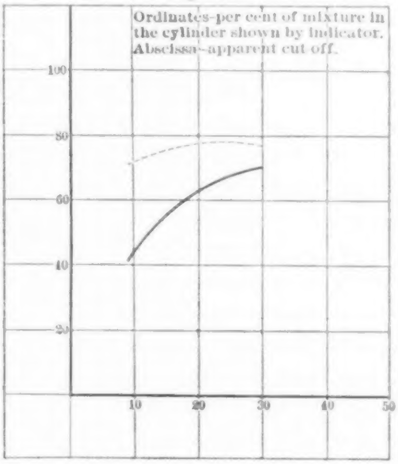


columns 16, 17 and 18. This is much more noticeable, as is to be expected, at the low powers of the first and second series than in the higher powers of the same, or in the third and fourth series. It is quite remarkable that the per cent. of the mixture of steam and water shown by the indicator at release is much less variable than that shown at cut-off.

To show this more clearly, Figs. 102, 103 and 104 have been prepared with the mean apparent cut-off for abscissæ, and the per cent. of the mixture of steam and water shown by the indicator for ordinates. In each the lower curve is for cut-off and the upper curve is for release. Fig. 102 represents both series I. and series II. ; for it has been shown that when the cut-off is

taken for a basis of comparison they reduce to one series. Fig. 103 represents series III., and Fig. 104 series IV. The near approach of the upper curve of Fig. 103 to a horizontal line is particularly noticeable. These diagrams indicate that the customary method of estimating the real amount of water per horse-power per hour from that shown by the indicator at release, is proper, provided that the correction added be determined for the type

Fig. 104



and size of engine in question. Of course such an approximate method is to be recommended only when direct methods cannot be applied.

The tests reported in the paper are only the incomplete beginning of the work which may be expected from the laboratory; at the present time we have nearly as many more which we are not ready to report on, and it is hoped that in course of several years the number of series may be increased and made more complete, till finally conclusions may be shown which will be of material value. The suggestions indicated by those already made will probably be modified, but it is thought that we are on the right track to accomplish something in addition to the very important work of instructing our students.

CCI.

THE COURSE IN MECHANICAL ENGINEERING AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND THE LABORATORIES OF MECHANICAL ENGINEERING AND APPLIED MECHANICS.

BY GAETANO LANZA, BOSTON, MASS.

Any one engaged in teaching Mechanical Engineering must value very highly any opportunities he may have to submit his work to the inspection and criticism of those who are actively engaged in the practice of the profession. It is very natural, therefore, that this visit of the Society to the Massachusetts Institute of Technology should be an occasion of peculiar gratification; and in extending to you a cordial invitation to visit the departments of Mechanical Engineering and Applied Mechanics, it is with the hope that you will feel free to inspect and criticise all our methods.

Two years and a half ago, when the writer was called upon to take charge of the Mechanical Engineering department, the erection of two new buildings had just enabled us to have the much needed space for a laboratory and drawing room, which had been previously crowded into such small corners of the one Rogers building as could be afforded in view of the needs of other departments. The task was then taken up, with the aid of Profs. Schwamb and Peabody, of organizing the course and the laboratory in such a way as seemed to us best suited to the requirements of such an Engineering school as our own.

Before going farther, it should be said that by "the laboratory" is meant the laboratory of Mechanical Engineering proper, as distinct, on the one hand, from the Mechanical laboratories or workshops, and, on the other, from the laboratory of Applied Mechanics, which is a laboratory for investigating the Strength of Materials, and also distinct from the Electrical Engineering laboratory of Prof. Cross.

The following are the principles which have guided us in the development of the course and the laboratory:

1. The proper function of such a department is to drill the stu-

dent in the theoretical (especially mathematical and physical) principles that underlie his profession, and to conduct this drill in such a manner that he may be able to apply them to the practical cases that he will meet after he leaves the school and enters active life.

2. If, instead of this, the student is taught to depend upon formulæ, hand-book rules, or rule-of-thumb methods, instead of upon his own power of applying principles to the solution of his problems, he has not been given the education that he has a right to expect.

3. In order to accomplish the objects stated, as large a practical element as time will allow should be introduced, in the recitation room, the drawing room, and the laboratory. This practical work should always follow and not precede the theoretical instruction bearing upon it. The student should be made familiar with the machinery which is to be found in the market to-day, and the models used in instruction, the illustrations, and the practical applications of his theory should all have to do with modern machinery and not with obsolete or impracticable combinations, and the style, system, conventions, etc., used in the instruction in drawing should be such as would be suitable for use in a draughting office.

In the laboratory he should be drilled in just such work as he will meet with in the practice of his profession, and this work, such as boiler tests, engine tests, measurements of power, etc., should be carried on with machinery of such proportions as would be employed by the engineer, and the conditions of working and the methods of conducting the experiments should be those suitable for use in practice.

He should be taught to perform his experimental work with the care and accuracy necessary to insure results of real value, and he should also be made to carry on experimental investigation.

4. Besides this he should have a good course in shop work; and his graduating thesis should consist of something which he has investigated for himself, the chief value of the thesis being, in my opinion, the practice the student acquires in carrying on original investigation on his own account.

5. Any attempt or claim of a technical school to give a student an education which will supersede that given subsequently by experience in practice, or to send him out an accomplished engineer is futile and impossible.

The regular work of the course may be classified as follows:

1. The Mathematics, Physics, and Applied Mechanics, which are given outside of the department; the last including the strength of materials.
2. The recitation-room work.
3. The drawing-room work.
4. The shop work.
5. The laboratory work.

Mention has already been made of the importance of the first. A few words should, perhaps, be said about the instruction in Applied Mechanics, which is also in the charge of the writer, and which extends through the third and fourth years. In this course the student is taught the strength of materials, both the theories and the experimental results, and is also given practice in testing in the laboratory of Applied Mechanics. It is believed that besides understanding the principles of the strength of materials, he should also be familiar with the experimental results obtained by testing, especially the more recent tests on full-sized pieces, and have practice in performing the tests himself.

2. In regard to the recitation-room work of the Mechanical Engineering Department proper, it begins in the second year with a study of the principles of mechanism, construction of gear teeth, etc., given by Prof. Schwamb; and this is followed, in the last part of the year, by two courses: one on machine tools, and the other on the mechanism of cotton machinery; the object of these courses being to familiarize the student, after his study of the principles of mechanism, with the shop and mill machinery which is to be found in the market to-day, and to this end he is shown, in addition to his recitation-room work, the machinery in our own shops and in other shops in the neighborhood, and also the cotton machinery in the laboratory, and is required to study their mechanism, the machines being run and stopped at will for his instruction. Excursions are also made to cotton mills near by.

In the third year he has a course on the slide valve and link, on thermodynamics, and the theory of the steam engine, and on steam boilers, all given by Prof. Peabody, and in this course the practical work of the laboratory, boiler and engine testing, etc., is kept constantly in view, and the student is shown how his work will apply to such tests as are made in the laboratory, or will have to be made by him in practice.

In the first half of the fourth year instruction is given partly by

Prof. Peabody, and partly by the writer on a variety of mechanical engineering subjects, as dynamometers, governors, fly-wheels, springs, rotative effect, effect of reciprocating parts, balancing of engines, injectors, steam-pumps, cylinder condensation, hydraulics, etc.

In the second half year a course on turbines is given to all the students, and an option is also given them during the second, and a portion of the first half, year, between a course on Marine Engineering, Locomotive Construction, or Mill Engineering. It is probable that ere long the number of these options will be increased.

DRAWING.

In this connection care is taken to have the drawings made according to a system suitable for use in the workshop; so that the student shall not, when he goes into practice, make such blunders as to omit dimensions over all, or employ a fanciful variety of methods of putting on his dimension lines, or make other similar mistakes. He is first taught to make working drawings from his own measurements of the piece, actual parts of machines being used and not model-makers' models. He has also to make the necessary drawings in connection with his course on mechanism and gear construction.

In the third year the class makes detail drawings of the different parts of some machine, as an engine lathe, a planer, etc., and when these are completed, assembly drawings from their detail drawings, so that they may know how to set up a machine. In the latter part of the third year they are required to make some mechanism designs. One of last year's designs was a set of cone pulleys for a lathe, arranged so as to give a variation of speeds changing by equal amounts.

In the first half of the fourth year a course is given by Prof. Schwamb of boiler drawings and machine designs. In these designs the proper strength for the different parts is taken into account, and the student is obliged to study thoroughly every detail, just as he would if the entire responsibility for the manufacture and the success of the machines designed rested upon him.

SHOP WORK.

The shop work, which is in charge of Prof. Schwamb, includes carpentry, pattern making, forging, chipping, filing and machine tool work. It begins with the second year and extends through the

fourth. On the part of Prof. Schwamb an invitation is extended to you to visit the shops, and to inspect the work done by the students. The proper method of conducting instruction in shop work has been publicly discussed on a great many occasions, and the methods pursued at the Massachusetts Institute of Technology are well known. It is with no desire to start a discussion on this subject, but merely to express a personal opinion, that we state our view that this method works best with our classes where the students have a great many other duties at the same time as well as their shop work.

THE MECHANICAL ENGINEERING LABORATORY.

The objects to be accomplished in such a laboratory are threefold.

1. To give to the students practice in the performance of boiler and engine tests, pump tests, measurement of power, and other experimental engineering work that they are liable to be called upon to perform in the practice of their profession; and this work should be conducted on a practical scale, the conditions under which the tests are made being those of actual practice.

2. To give to the student skill in making original investigations, and in making them with such care and accuracy that the results may be of real value to the engineering community.

3. To publish the results of such investigations from time to time, and thus to add gradually to the common stock of knowledge.

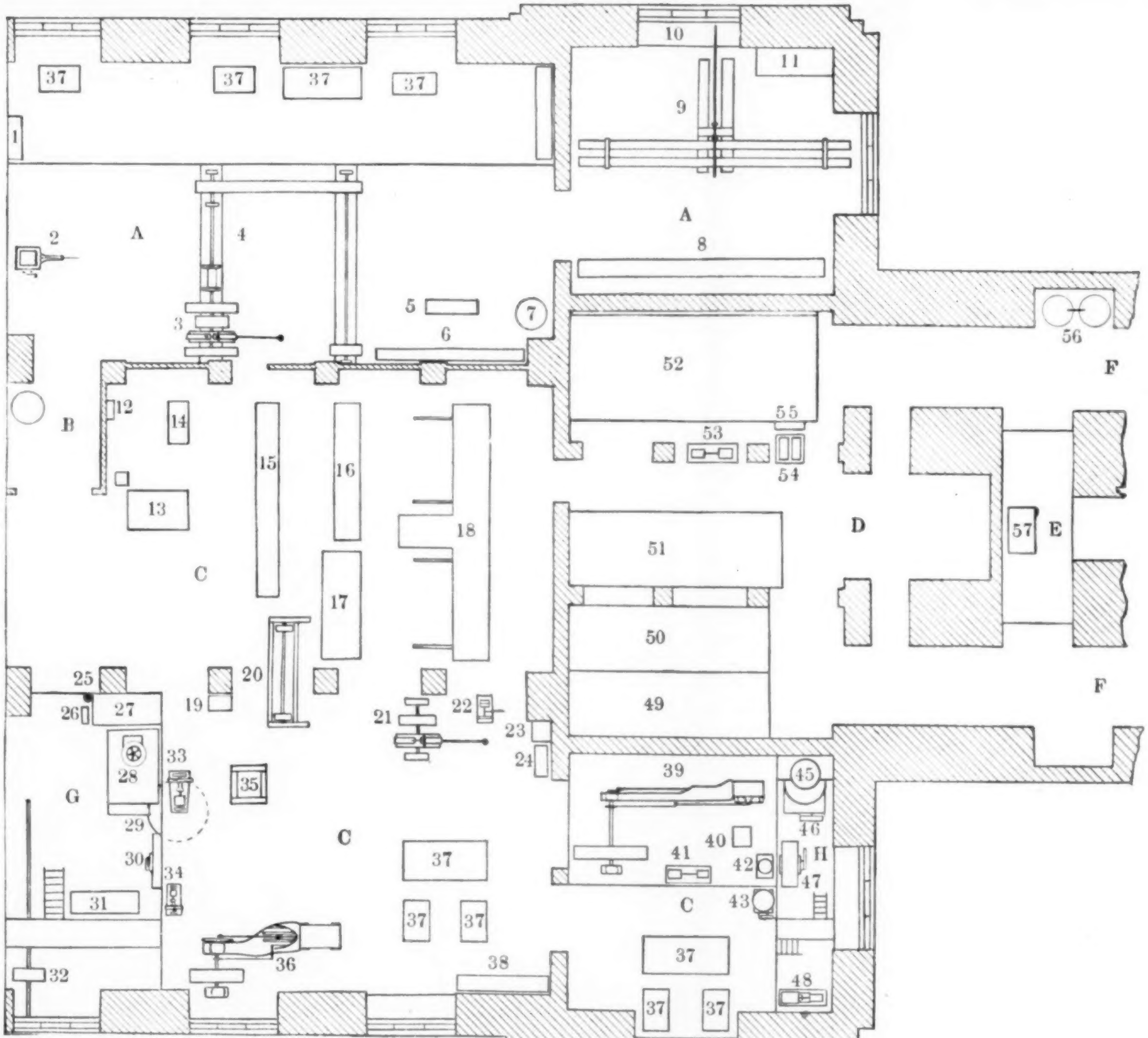
The student begins laboratory work at the middle of the third year, the last part of that year being devoted to a drill in making steam-engine tests. The laboratory work is then continued through the fourth year, and consists partly of a more extended drill in the usual mechanical engineering tests of boilers, pumps, power, etc., and also contains a large amount of original experimental investigation.

APPARATUS AND WORK OF THE LABORATORY.

The following partial list of the apparatus of the laboratory will make it possible to explain intelligibly the character of the work which we are now doing.

The laboratory, of which a plan is shown in Fig. 109, contains as a portion of its equipment,

FIG. 109.



PLAN OF THE LABORATORIES OF ENGINEERING AND MECHANICS, MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

A. Applied Mechanics Laboratory.
B. Entrance Hall.
C. Mechanical Engineering Laboratory.
D. Boiler room.
E. Elevated platform.
F. Coal bins.
G. Large pit.
H. Small pit.

1 Bench and vise.
2 Olsen testing machine (50,000 lbs.).
3 Friction brake (30 H. P.).
4 Shaft-testing machine.
5 Cement-testing machine.
6 Specimen case.
7 Forge.
8 Transverse time-testing machine.
9 Transverse testing machine (50,000 lbs.).
10 Bench and vise.
11 Dark room.
12 Yarn-testing machine.
13 Cotton card.
14 Drawing frame.
15 Speeder.
16 Fly frame.
17 Ring frame.
18 Mule.
19 Standard gauge and square inch.
20 Belt-testing machine.
21 Friction brake (20 H. P.).
22 Transmission dynamometer.
23 Bench.
24 Turning latne.
25 Mercury column.
26 Knowles pump.
27 Tank for turbine.
28 Swain turbine.
29 Cistern for turbine.
30 Tank on scales.
31 Large surface condenser.
32 Shaft to new building.
33 Large centrifugal pump.
34 Small centrifugal pump.
35 Belt machine.
36 Porter-Allen engine (80 H. P.).
37 Desks.
38 Instrument case.
39 Harris-Corliss engine (16 H. P.).
40 Small condenser.
41 Blake pump.
42 Barrus' calorimeter.
43 Tank on scales.
45 Small tank at foot of stand pipe.
46 Large tank on scales at foot of stand pipe.
47 Tank on scales.
48 Deane vacuum pump.
49 Low-pressure boiler, No. 1.
50 Low-pressure boiler, No. 2.
51 High-pressure boiler, No. 3.
52 High-pressure boiler, No. 4.
53 Blake pump.
54 Worthington pump.
55 Mack injector
56 Tanks for use in boiler tests.
57 Tanks on scales for boiler tests.

1. An 80 H. P. Porter-Allen engine, which also furnishes power to the New Building and to the Mining Department.

2. A 16 H. P. Harris-Corliss engine, furnished with a throttle governor in addition to its own automatic cut-off governor, either of which can be used, the other being thrown out of action for the time being. The automatic cut-off governor is also furnished with a lever by means of which we may so load it as to vary the speed of the engine at will.

3. A surface condenser (in which the condensing water passes outside of the tubes) arranged so that it can be readily put in communication with the main steam pipe from the boiler or with the exhaust of the Harris-Corliss, the condensed steam being delivered into a tank on scales where it can be weighed.

4. A surface condenser (in which the condensing water passes through the tubes), connected with the exhaust of the Porter-Allen and delivering the condensed steam into a tank on scales where it can be weighed.

This condenser is also arranged in sections so that the condensing water may be passed back and forth once, twice or three times, at the option of the experimenter.

5. A nicely made Prony brake for the purpose of using up and of weighing the work done by either engine; also the use of another Prony brake in the Applied Mechanics Laboratory.

6. The four horizontal tubular boilers used for furnishing steam for power and heating purposes to the Rogers and the New Building.

7. Steam and vacuum pumps as follows: one Worthington, two Blake, one Knowles steam pumps, and one Deane vacuum pump.

8. One Mack injector.

9. A machine specially made for determining the ratio of the tensions on the tight and loose side of a belt when the pulley and belt slide over each other with different speeds.

10. A machine for determining by actual weighing the sum of the tensions required to enable a belt to carry a given power at a given speed with not more than a given amount of slip.

11. A mercury column.

12. A Swain turbine arranged in such a way that it can be run and experiments can be made on the work done, the water used, etc., under different gates.

13. A transmission dynamometer.

14. A Whitin card, an English drawing frame, a Lowell speeder, fly frame and ring frame, a Mason mule.

Besides these a good supply of indicators, thermometers, gauges, anemometers, and other accessory apparatus.

We also make use for experimental purposes of a Brown 40 H. P. engine, a horizontal tubular boiler, a Deane pump, and Hancock inspirator and a number of looms, which are in the mechanical laboratories on Garrison Street.

This list of apparatus will perhaps make it possible to explain the character of the work done.

1. In regard to the work of the kind referred to under the first head we are well equipped for making careful and complete boiler tests—engine tests in which the water passing through the engine is condensed and weighed, measurements of power, calorimeter tests, pump tests, valve setting, etc. For the power measurements we make use of the cotton machinery.

2. In regard to the part which it is intended that the laboratory should fill in matters of original investigation, it may be said that it is perfectly feasible to have a great deal of such work done at the hands of the students, provided only the necessary supervision is exerted to arrange the work properly, and to see that it is accurately done.

This is accomplished in this laboratory both with tests intended for drilling the students in the processes, and also with those intended exclusively for original investigation.

Thus each engine test is made to fill some place in a series of cut-offs, of speeds, of back pressures, etc., with either throttling or automatic cut-off.

There are now on file results of about 80 carefully conducted tests, which have been made the subject of two theses; one by Mr. A. L. Fitch and one by Mr. Thos. W. Fry, and an account of them is given you in Prof. Peabody's paper at this meeting.

Our boiler tests also are to be used as means of systematic investigation, on the value of different coals, grate areas, height of bridge wall, etc., etc.

We have also apparatus for testing the transmission of power by belting, which is a problem that we have been at work upon for some time.

As it had been pointed out by Prof. Holman of this Institute that the great discrepancy of results in the ordinary friction theory of belting was due to the fact that the different experimenters used different speeds of slip, we made a large number of experiments on the different results obtained with different speeds of slip, and then

by attaching counters to a series of shafts, and varying the power transmitted at will, we were able to ascertain a somewhat average value for the slip of 8 and 10 inch double belts transmitting different powers. Then obtaining our coefficient of friction by using this slip we found Briggs' and Towne's coefficient of .42 altogether too high and a coefficient of .27, much nearer the truth. But now we have an apparatus in which we hang a transmitting shaft by two belts to the driver and driven shaft, the two last being in one line, and through this combination drive a brake on which we can vary the power at will. Then by means of levers we load the belts, thus ascertaining the least tension necessary to transmit a certain power at a certain speed, measuring the corresponding slip by counters. These experiments are described in my paper on belting, presented at this meeting, and it is believed that this work is entirely new.

Then with our condenser we try a number of experiments on efficiency of condensers, etc.

The cotton machinery is used for four purposes:

1. To teach the second year the mechanism of the machinery.

2. To teach the fourth year how to operate it, and to adjust it to suit the different numbers and qualities of yarn that it is desired to make.

3. For power measurements.

4. For original investigation.

This rough outline of the work done in the laboratory will explain to you the direction in which we are working.

LABORATORY OF APPLIED MECHANICS.

The description of this laboratory, a plan of which is shown at the end of this paper, will be very brief. It is essentially a strength of materials laboratory. It contains:

1. An Olsen 50,000 lbs. testing machine for tension and compression.

2. Accessory apparatus devised at the Institute for determining the modulus of elasticity.

3. A 50,000 lbs. transverse testing machine suited to determine the breaking strength and deflection of full-size beams up to 25 feet span, under concentrated or distributed loads; also the strength of framing joints, full size.

4. Arrangements for subjecting a number of full-size timber

beams at once to a moderate load, and determining the effect of time, and also of seasoning under load upon their deflection and breaking strength.

5. A machine for testing the strength of mortars and cements.

6. Machinery for determining the breaking strength of shafting subjected to a twist by actually transmitting power through it, and at the same time to a transverse load corresponding to the pull of the belt.

Also arrangements for determining the twist and deflection of shafting under combined twisting and bending due to the belt pull, and studying the effect of different distances of hangers and positions of pulleys upon the twist and deflection of the shaft.

The object of this laboratory is to give the students a practical familiarity with the behavior of the materials themselves when subjected to such stresses as occur in practice; and it is believed that whenever possible the pieces should be full size, and the conditions as nearly those of actual practice as possible.

We have already ascertained a number of constants for use in determining the transverse strength of timber, and we intend to do the same for shafting, besides making other experimental investigations on the strength of materials.

CCII.

TRANSMISSION OF POWER BY BELTING.

AN ACCOUNT OF THE WORK DONE UPON THIS SUBJECT IN THE MECHANICAL ENGINEERING LABORATORY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BY GAETANO LANZA, BOSTON, MASS.

It is well known to mechanical engineers that the rules for determining the proper width of leather belting to carry a given power at a given speed differ enormously from each other. Any one may readily satisfy himself of the truth of this statement by a glance at the "Use of Belting," by John H. Cooper, where he will find such a mass of different rules, that he will need some further evidence on the subject before he can decide which rule is right, and which rules are wrong.

The greater part of these rules are no better than guesses; being merely the practice of this or that mechanic, based upon no experimental evidence whatever.

Rules of this character will not be considered in this paper, and only those will be discussed which have as their basis some experimental investigation, whether correct or incorrect; but even these differ in their results, in some cases, by as much as one hundred per cent.

During the last two years we have been carrying on a series of experiments in the laboratory, with a view of solving this problem with such completeness as to leave no room for doubt as to the correctness of the results. At the present time we are engaged upon one of these series, which we believe to be the final one, and which must, by the time it is completed, give us a definite answer to the problem which we set out to solve. The work done upon the subject has formed part of the regular laboratory work, and also the subject of two theses—one by Mr. A. J. Purinton, and the other by Mr. A. L. Merrill.

An account of this work, and a summary of the results obtained, will now be given in the order in which they were determined; but beforehand I will state briefly what had already been done by others.

The only experiments of which the writer is aware are the following:

1°. By General Morin.

2°. By Henry R. Towne of the Yale & Towne Company.

3°. By Edward Sawyer of Charlestown, Mass.

4°. By Professor S. W. Holman of the Massachusetts Institute of Technology.

(1°) As to those of Morin, he used a fixed cast-iron drum, over which hung the belt, the ends hanging vertically, and being of equal lengths; these two ends he loaded with equal weights, and then added weight on one side until the belt slipped, and thus determined the two tensions T_1 on the tight side, and T_2 on the loose side. He then determined the co-efficient of friction, f, from the formula

$$f = \frac{\text{hyp log } T_1 - \text{hyp log } T_2}{\pi}.$$

The results obtained by him are as follows:

New belting on smooth cast iron, dry	0.284
New belting on smooth cast iron, wet	0.377
New belting on rough cast iron	0.281
Old belting on rough cast iron	0.277

He does not state what was the speed with which the belts were slipping when he obtained these results.

(2°) Mr. Henry R. Towne performed his experiments in the same way, only that he allowed his belts to slip at a speed as nearly 200 feet per minute as he could judge by the eye.

He obtained as result $f = 0.58$; but he and Mr. Robert Briggs recommend for use two-thirds of this, or $f = 0.42$.

(3°) Mr. Edward Sawyer, of Charlestown, used also a fixed drum, and performed the experiments in the same way as the other two, with this exception—that, when he had loaded the heavy side sufficiently to make the belt slip, he then placed additional load on light side, until he just stopped the slipping; then, calculating his co-efficient of frictions by the same formula, he obtained results varying from 0.12 to 0.17. Whichever of these results is used, the rule for finding the ratio of the tension on the tight and loose sides of a belt is given by the formula

$$\frac{T_1}{T_2} = e^{f\theta};$$

where e = Napierian base, and θ = circular measure of arc of contact between the belt and the pulley; and, having this ratio, it is easy to compute the width of belt necessary to convey a given power at a given speed.

4°. In 1882 Professor S. W. Holman of the Physical Department of the Institute of Technology undertook a set of experiments with a view to ascertain the cause of the enormous discrepancy in the results of the different experimenters. He caused the pulley to slide under the belt, hanging weights on the loose side of the belt, and attaching the other end to a spring balance.

He found that, with a low speed of slip, he obtained as low a result as 0.12, while with a speed of 200 feet per minute he obtained about 0.58 and intermediate values, with intermediate speeds of slip; hence that the co-efficient of friction varies with the speed of slip.

One important function of the laboratory of mechanical engineering is to undertake and carry on original investigations of engineering problems. Recognizing, therefore, the importance of the belting problem, we set out to determine:

1°. What is the average value for the speed of slip which we realize in practice under ordinary conditions of working?

2°. What is the co-efficient of friction which is obtained with the average speed of slip?

3°. How does the co-efficient of friction vary with the different kinds of belt and of pulleys?

This work has been made a part of our regular laboratory work, and was also carried on in connection with the two theses already mentioned.

A summary of the results obtained up to the end of the last school-year will now be given, but I will preface it by the following remarks in regard to the mode of procedure and the nature of the results.

The slip tests were made entirely on 7-inch, 8-inch, and 10-inch double belts, by loading them with a known horse-power by means of a nicely made Prony brake on which the power used could be weighed. These tests were made as follows: Placing a fixed load on the brake, readings of counters attached to the driving and driven shafts were taken at definite intervals; and, the diameters of the pulleys being known, the slip of the belt was readily computed.

The slip of these belts under ordinary loads was, on an average,

about 3 feet per minute. Experiments were then made upon a machine driven by power, and specially designed for the purpose, where a pulley was caused to slide under the belt at such a rate of speed as might be desired; and thus the ratio of the tensions, and hence the co-efficient of friction under various speeds of slip, was determined.

The average value of this co-efficient under a speed of slip of 3 feet per minute would seem to be, in the light of these tests, about 0.27, corresponding (if the admissible stress per inch of width be taken at 66⅔ pounds) to the rule that a belt 1 inch wide must travel 1,000 feet per minute to transmit one horse-power.

This, it will be seen, requires much wider belts than Briggs's and Towne's rule, to take 0.42 for co-efficient of friction; and, as a matter of fact, we never realize in practice while driving, a slip anywhere near 200 feet per minute, which was the slip used in Mr. Towne's experiments.

I will next proceed to give the summaries.

SUMMARY OF TESTS MADE IN 1883-84.

No. of Experiments.	Kind of Belt.	Side next Pulley.	Nature of Pulley.	Maximum Co-efficient of Friction.	Minimum Co-efficient of Friction.	Humidity.	Speed of Slip, in Feet, per Minute.
1	Old oak-tanned..	Hair,	Lagged,	0.2700	0.2500	0.39	1.91
2	" " ..	"	"	0.2730	0.2570	0.36	1.91
3	" " ..	Flesh,	"	0.2660	0.2460	0.49	1.91
4	Raw hide.......	Hair,	"	1.0420	0.9825	0.44	1.91
5	" "	Flesh,	"	0.5695	0.5250	0.44	1.91
6	" "	Hair,	"	0.8800	0.8340	0.44	1.91
7	New oak-tanned.	"	"	0 2850	0.2620	0.38	1.91
8	" " .	Flesh,	"	0.2800	0.2640	0.39	1.91
9	Rubber.........		"	0.3780	0.3450	0.39	1.91
10	"		Cast iron,	0.3860		0.43	1.72
11	New oak-tanned.	Hair,	"	0.1440		0.48	1.91
12	" " .	Flesh,	"	0.1710		0.48	1.91
13	Raw hide.......	Hair,	"	0.2510		0.48	1.91
14	" "	Flesh,	"	0.2650		0.48	1.91
15	" "	Hair,	"	0.2260		0.55	1.91
16	Old oak-tanned..	"	"	0.1560		0.55	1.95
17	" " ..	Flesh,	"	0.1793		0.44	1.75

SUMMARY OF SLIP TESTS, 1884-85.

No. of Experiments.	Description of Belt.	Speed of Belt, in Feet, per Minute.	Horse-Power transmitted.	Speed of Slip, in Feet, per Minute.	Remarks.
1	10" double belt......	1311	14.69	14.76	Inclined at about 45° to the horizon. The belt was very slack.
2	" " "	1350	11.12	9.64	
3	" " "	1365	10.23	7.13	
4	" " "	1385	8.31	5.75	
5	" " "	1414	5.31	3.57	
6	" " "	1411	5.31	3.34	
1	8" double belt.......	1537	14.69	10.98	Nearly vertical. The belt was very slack.
2	" " "	1586	11.12	7.49	
3	" " "	1605	10.23	6.52	
4	" " "	1630	8.31	4.61	
5	" " "	1666	5.31	2.70	
6	" " "	1664	5.31	2.14	
7	10" double belt......	1315	8.88	4.33	The belt was now tightened to about ordinary tightness.
8	" " "	1303	11.75	5.41	
9	" " "	1298	12.66	3.12	
10	8" double belt.......	1597	6.11	1.53	The belt was now tightened to about ordinary tightness.
11	" " "	1610	8.21	1.97	
7	" " "	1548	8.88	3.44	
12	" " "	1593	10.15	4.65	
8	" " "	1536	11.75	2.20	
13	" " "	1576	12.05	5.14	
9	" " "	1528	12.66	4.09	
14	" " "	1568	13.99	4.68	
15	" " "	1517	15.47	3.71	
10	7" double belt.......	1617	6.11	2.70	Horizontal belt. This belt was rather slack.
11	" " "	1631	8.21	4.29	
12	" " "	1617	10.15	5.70	
13	" " "	1600	12.05	6.90	
14	" " "	1591	13.99	6.35	
15	" " "	1539	15.47	7.94	

It will be seen that, when the belts experimented upon were ordinarily tight, the speed of slip of these belts had for its greatest value 5.41 feet per minute, and that a rough average might be taken at about 3 feet per minute.

I will next proceed to give a summary of the tests for determining the co-efficient of friction during 1884–85, a part of which were done as regular laboratory exercises, and a part by Mr. A. L. Merrill for his theses.

The summary is as follows:

SUMMARY OF FRICTION TESTS MADE BY MR. MERRILL.

HAIR SIDE NEXT PULLEY.				HAIR SIDE NEXT PULLEY.			
Test No.	Speed of Slip, in Feet, per Minute.	Ratio of Tensions.	Co-efficient of Friction.	Test No.	Speed of Slip, in Feet, per Minute.	Ratio of Tensions.	Co-efficient of Friction.
1	2.09	2.22	0.255	21	6.84	2.39	0.275
2	2.09	2.23	0.255	22	6.84	2.41	0.280
3	2.09	2.22	0.255	23	6.84	2.48	0.290
4	2.09	2.23	0.255	24	6.84	2.58	0.300
5	2.09	2.23	0.255	25	6.84	2.67	0.313
6	2.09	2.08	0.235	26	7.00	2.88	0.337
7	2.09	2.02	0.225	27	7.00	2.96	0.345
8	2.09	2.03	0.225	28	7.00	2.83	0.330
9	2.09	2.02	0.225	29	7.00	2.90	0.339
10	2.09	2.03	0.225	30	7.00	2.90	0.339
Average,	2.09	2.12	0.240	Average,	6.92	2.64	0.310

HAIR SIDE NEXT PULLEY.

11	2.83	2.34	0.270
12	2.83	2.38	0.275
13	2.83	2.38	0.275
14	2.83	2.39	0.275
15	2.83	2.41	0.280
16	2.38	2.49	0.290
17	2.38	2.49	0.290
18	2.38	2.50	0.291
19	2.38	2.49	0.290
20	2.38	2.51	0.294
Average,	2.605	2.398	0.278

FLESH SIDE NEXT PULLEY.

31	2.09	1.93	0.210
32	2.09	1.93	0.210
33	2.09	1.92	0.210
34	2.09	1.92	0.210
35	2.09	1.91	0.210
Average,	2.09	1.92	0.210
36	3.38	2.27	0.260
37	3.38	2.18	0.250
38	3.38	2.16	0.246
39	3.38	2.17	0.248
40	3.38	2.16	0.246
Average,	3.38	2.19	0.250
41	7.00	3.20	0.370
42	7.00	3.17	0.367
43	7.00	3.11	0.361
44	7.00	3.06	0.357
45	7.00	3.05	0.355
Average,	7.00	3.12	0.363

No. of Experiment.	Kind of Belt.	Side next Pulley.	Nature of Pulley.	Co-efficient of Friction.	Speed of Slip, in Feet. per Minute.
18	Oak-tanned	Hair	Cast iron,	0.776	238.0
19	"	Flesh	"	0.45	238.0
20	"	Hair	"	0.82	210.0
21	"	Flesh	"	0.51	210.0
22	"	"	"	0.37	15.4
23	"	Hair	"	0.30	15.4
24	"	"	"	0.33	15.0
25	"	"	"	0.33	15.0
26	"	Flesh	"	0.36	15.0
27	"	Hair	"	0.34	16.9
28	"	Flesh	"	0·42	16.9
29	Raw hide	Hair	"	0.36	14.9
30	" "	Flesh	"	0.38	14.9
31	" "	Hair	"	0.33	15.1
32	" "	Flesh	"	0.45	15.1
33	" "	Hair	"	0.38	13.9
34	" "	Flesh	"	0.45	13.9
35	" "	Hair	"	0.42	14.9
36	" "	Flesh	"	0.52	14.9
37	" "	"	"	0.74	12.8
38	" "	"	"	0.67	12.8
39	Oak-tanned	Hair	"	0.43	12.7
40	"	"	"	0.37	12.7
41	"	"	"	0.32	12.7
42	"	Flesh	"	0.37	12.4
43	"	"	"	0.31	12.4
44	"	"	"	0.32	12.4
45	Raw hide	"	"	0.60	12.5
46	" "	"	"	0.58	12.5
47	" "	"	"	0.57	12.5

I will next give a table which exhibits a comparative view of the results thus far referred to under a variety of aspects.

	Co-efficient of Friction.	T_1 required to transmit 1 H. P. at 1,000 Ft. per Minute, in Lbs.	T_2 corresponding, in Lbs.	$T_1 + T_2$ corresponding, in Lbs.	T_1 required to transmit 1 H. P. at 1,500 Ft. per Minute, in Lbs.	T_2 corresponding, in Lbs.	$T_1 + T_2$ corresponding, in Lbs.
Morin	0.28	55.2	22.2	77.4	36.8	14.8	51.6
Towne (experiments)	0.58	39.3	6.3	45.6	26.2	4.2	30.4
Towne (given to be used	0.42	45.0	12.0	57.0	30.0	8.0	38.0
Sawyer	0.12 to 0.17	105 0 to 79.6	72.1 to 46.6	177.0 to 126.2	70.0 to 53.0	48.0 to 31.0	118.0 to 84.0
M. E. Lab., with slip of 3 feet per minute	0.27	57.8	24.8	82.6	38.5	16.5	55.0

It would seem reasonable, that with a belt travel of about 1,500 feet per minute, which is about the speed of the belts used in making the slip tests, the speed of slip should not be more than about three or four feet per minute; and this would necessitate a co-efficient of friction of about 0.27, which means that the belt should have a strain of 55 pounds per horse-power transmitted.

This is the value of the co-efficient of friction deduced as an average by Mr. Merrill in the tests which he made for his thesis.

It is also evident, that if we use a higher co-efficient, as 0.42, we must, in order to realize it, have a strain upon the belt of only 38 pounds per horse-power transmitted; but then we should have a speed of slip much larger than would be suitable to use in practice: and that, if we determine the width of the belt on the basis of 38 pounds, and then strain it more, we are no longer keeping within the limits of safety intended.

Moreover, while the work described above would seem to throw a great deal more light upon the problem of belting, there are two objections that might theoretically be raised to this form of experiment, which objections can only be refuted by another form of experiment.

These objections are the following:

1°. Is the ratio of tensions the same when the belt is driving as when either the belt slides over the pulley, or the pulley under the belt?

2°. Is the ordinary friction theory correct for a driving-belt?

In regard to this, I will say, that the few experiments we have thus far made this year show very conclusively that these results are nearly correct. It will be observed that by means of the brake and the counter we are able to determine $T_1 - T_2$; and, in order to determine T_1 and T_2, we must first have some other function of the tensions besides their difference. We have been relying upon the frictional machine to supply this want by giving us the ratio of the tensions. In the work we are now doing, however, we are actually weighing the sum of the tensions, or $T_1 + T_2$, and thus we can determine T_1 and T_2. Professor Peabody, Professor Schwamb, and myself have all had a hand in getting up this new apparatus for the laboratory; but we had it ready for use only about two weeks ago, and hence our results are but few, and some of them are doubtless partially vitiated by the inexperience of the students when first put on these tests.

However, the results which we have point very clearly to a confirmation of the conclusions drawn from the preceding results.

The machine itself I will not stop to describe, but will merely say that the power is transmitted from the driving-shaft to another shaft in the same line through an intermediate shaft, which is hung from these by two belts of equal length; the pulleys being of equal diameter. Thence the power passes to a brake, where it is weighed. Now, the boxes which contain the hanging-shaft are attached to levers, by means of which the sum of the tensions on each of the belts by which it is suspended can be weighed. Then counters are attached to each shaft in the series, and also to the brake.

Thus we can, by means of the brake and counter readings, determine $T_1 - T_2$ for any given case, and then $T_1 + T_2$ by direct weighing; hence T_1 and T_2 are determined without the assumption of any friction theory.

This machine enables us to answer either of the following questions; viz.:

1°. In order to transmit a given power at a given speed of belt, what is the least value of $T_1 + T_2$ with which we can succeed to drive at all, without having the belt slip off? what is the speed of slip we obtain under these conditions? and what the values of T_1 and T_2?

2°. If a given power is to be transmitted with a given speed, and the speed of slip is not to exceed a given quantity, what is the value of $T_1 + T_2$ required for the purpose? and what are T_1 and T_2?

These questions can be definitely answered, and then the question of width of belt is to be determined by so fixing it that it shall be able to bear the required value of T without injury and without losing its tightness. And the amount of strain which should be put upon it is determined thus: if we assume, with Briggs, a safe-working strength of $66\frac{2}{3}$ pounds per inch of width through the lace holes, we merely need to divide T_1 by $66\frac{2}{3}$ to get the width of belt required.

I will now give the results of the few experiments which have been made thus far:

Test No.	Horse-Power.	Speed of East Belt, in Feet, per Minute.	Speed of West Belt, in Feet, per Minute.	Slip of East Belt, in Feet, per Minute.	Slip of West Belt, in Feet, per Minute.	$T_1 + T_2$.	$T_1 - T_2$ of East Belt.	$T_1 - T_2$ of West Belt.	T_1 of East Belt.	T_2 of East Belt.	T_1 of West Belt.	T_2 of West Belt.	$\frac{T_1}{T_2}$ of East Belt.	$\frac{T_1}{T_2}$ of West Belt.	f for East Belt.	f for West Belt.
1	4.86	1440	1489	48.60	40.80	150	111.37	107.71	130.7	19.3	128.9	21.1	6.720	6.109	0.61	0.58
2	4.90	1462	1505	43.10	25.10	175	110.60	107.40	142.8	32.2	141.2	33.8	4.434	4.177	0.48	0.45
3	4.95	1488	1499	11.80	14.10	225	109.80	108.90	167.4	57.6	167.0	58.0	2.906	2.879	0.34	0.33
4	4.78	1522	1528	4.50	3.10	250	103.50	103.20	176.8	73.2	176.6	73.4	2.415	2.406	0.28	0.28
5	4.79	1529	1536	7.70	5.60	275	103.90	102.80	189.5	85.6	189.0	86.0	2.213	2.197	0.26	0.25
6	4.58	1504	1519	14.90	16.50	200	100.50	99.50	150.3	49.7	149.8	50.3	3.024	2.958	0.35	0.34
7	4.95	1521	1526	4.70	12.60	275	107.30	107.06	191.2	83.9	191.0	84.0	2.278	2.274	0.26	0.26
8	5.02	1527	1531	3.90	11.80	300	108.40	108.20	204.2	95.8	204.1	95.9	2.132	2.128	0.24	0.24
9	5.06	1550	1551	1.30	12.30	250	107.80	107.70	178.9	71.1	178.9	71.1	2.516	2.516	0.29	0.29
10	5.03	1540	1544	0.40	8.90	275	107.80	107.60	191.4	83.6	191.3	83.7	2.289	2.285	0.26	0.26
11	5.06	1550	1552	2.59	9.50	300	107.80	107.60	203.9	96.1	203.8	96.2	2.121	2.121	0.24	0.24

It will be seen that, in transmitting about five horse-power, the least value of $T_1 + T_2$ with which it was possible to run was **150** pounds = 30 pounds per horse-power: this is the value we should obtain by using 0.58 for co-efficient of friction, the value obtained by experiments of Towne; but we then should have a speed of slip of about 50 feet per minute, which is manifestly very much in excess in what is either safe or economical to allow. On the other hand, we obtain much better running with 275 or 300 pounds for $T_1 + T_2$, 250 being rather light; and this gives a speed of slip of 3 to 12 feet per minute. Now, 275 pounds for five horse-power is 55 pounds per horse-power, which is just what we should obtain by using a co-efficient of friction of 0.27.

While there are doubtless some discrepancies in these few experiments, which will be eliminated as soon as we have a larger number, and while we shall before long be able to make out a table showing exactly how much we must strain our belts to accomplish any desired result as to transmission and slip, which table will furnish us readily the proper sizes of belt to use for any given case, nevertheless even what has been done is sufficient to enable us to say with certainty that a co-efficient of friction of 0.42 is altogether too large, and is never realized in practice, as the belts are in practice strained more than this co efficient implies, and also that a co-efficient of 0.27 is much nearer the truth, and hence that our rules for belting if based upon the latter would be much nearer correct than if based upon the former.

ADDED SINCE THE MEETING.

Mr. H. R. Towne.—Professor Lanza's experiments cover the most thorough investigation of the efficiency of belting as a means of transmitting power, which, so far as I know, has yet been published. The subject is one of great interest and importance, and it is strange that it had not been sooner brought within the rapidly enlarging field of exact determination and rule based upon experimental investigations conforming so closely to the conditions of actual practice as to make sure that the results arrived at are substantially identical with those obtaining in use.

In 1867 I undertook to determine the co-efficient of friction of leather belting on cast-iron pulleys by means of a series of workshop experiments, made carefully, but with no other apparatus than a few pulleys, belts and weights. My experiments and their

results were published fully in the *Journal of the Franklin Institute* for February, 1868, the preceding number of which contained a discussion of the theory of belt transmission, and also working tables and rules (based on the co-efficient determined by me) prepared by my friend the late Robert Briggs of Philadelphia, at whose suggestion I had taken up the subject. The absence of satisfactory data, and the need of better information on this subject at that time, was evidenced by the fact that the results embodied in the above two papers, which are usually referred to as "Briggs's and Towne's Experiments," were subsequently copied, adopted and referred to by nearly every writer of general text-books on engineering who treats of belting, such as D. K. Clark and Professor W. C. Unwin, of London; Professor Reuleaux, of Berlin, and numerous American authors. This general acceptance of the results of those early experiments is my excuse for referring to them now, and for offering the following brief explanation of the difference between those results and the recent work of Prof. Lanza.

For nearly twenty years the experiments referred to above have been generally accepted as offering a reliable guide to practice in the matter of belting, notwithstanding the crudeness of the means employed in making them, and the further fact that the method of conducting the tests had been fully set forth in the original publication. The period which has thus passed has been one of great progress, and has seen wonderful advances in all mechanical arts. It is gratifying to see that the better knowledge thus obtained has been applied to a reinvestigation of a subject of such importance, and that the results indicated by the latter are such, and are so presented, as to afford a much more reliable guide than any data previously existing. It is greatly to be hoped that Professor Lanza will continue his investigations, and will finally reduce his results to convenient tabular form for reference and use.

The most prominent point of difference between the recent experiments and the earlier ones is the fact, now for the first time clearly brought out, that the *speed of slip* between the belt and the pulley is a factor of prime importance in determining the co-efficient of friction. The earliest experimenter, General Morin, of Paris, whose investigations of friction have been a standard of reference for a generation, in his report gives no information as to the speed of slip, thus, tacitly at least, leaving it to be inferred either that such speed was a constant factor, or else that it is an

unimportant one. The experiments of the present writer above referred to were made under the approximately uniform condition of 200 feet per minute slip. This fact was clearly stated in the original publication, where it was also explained that this speed was adopted as the slowest which permitted of substantial uniformity under the various conditions of test with the crude apparatus employed. Professor Lanza's experiments, with his refined and apparently perfect apparatus, show that in practice the speed of slips never reaches so high a figure, and probably, under proper conditions, rarely exceeds 2 or 3 per cent. of this rate. The other experiments referred to in Professor Lanza's paper, those of Mr. Sawyer and Professor Holman, approximate much more closely to the conditions of practice in this regard, the latter investigator having discovered the chief explanation of the discrepancies in previous work by proving that variations in the speed of slip would cause changes in the co-efficient of friction ranging from .12 to .58.

In view of the time when, and the conditions under which the experiments of the writer were made, no apology is offered for the fact that they must now be set aside in favor of those arrived at by Professor Lanza with his elaborate and beautifully designed testing apparatus. It is somewhat consoling, however, to know that the investigations of Professor Holman, and the recent work done by Professor Lanza, both show that at the speed of slip of 200 feet per minute, a co-efficient of .58 to .82 is developed (the average of four tests reported by Professor Lanza, with speeds of slip ranging from 210 to 238 feet per minute, gives $f = 639$), thus fully confirming the correctness of the results obtained in my experiments notwithstanding the crudeness of the apparatus employed.

After carefully reviewing our experiments, Mr. Briggs and I considered it prudent to reduce our co-efficient of friction of .58 by one-third, and therefore assumed for practice $f = .42$, and on this latter value the tables in Mr. Briggs's paper were based. In the light of Professor Lanza's experiments, so far as published, I fully concur in his determination of a co-efficient of .27 as being more nearly correct than .42.

In conclusion I will express the hope that Professor Lanza's experiments may be continued and extended until their indications have settled conclusively all important points involved in the transmission of power by belting. I would suggest, also, that his apparatus, which is at present so arranged that the belts are verti-

cal, should ultimately be modified so as to place the belts in horizontal position, and, also, that the experiments should cover the ordinary range of conditions as to relative sizes of driving and driven pulleys. It is to be hoped, also, that the experiments may throw some light upon the question of very high belt speeds as affecting the efficiency of transmission and the durability of belts, and, finally, that the results arrived at in all of these respects may be reduced to such convenient and simple form as to come into general acceptance and use.

CCIII.

NOTES ON COMPARATIVE VALUES OF METAL SURFACES FOR WARMING AIR.

BY W. J. BALDWIN, NEW YORK CITY.

THE experiments reported here were made by the writer, assisted by Mr. F. W. Wright, of New York City, and extended over a period of one year.

Their prime object was to ascertain the best form to give what is known as "compound coil-radiator" surface, viewing it from the commercial stand-point.

In doing this some *data* and much general information were collected which may be of value to the members of this Society; incomplete though the results may be from a purely scientific point of view. It must, therefore, be remembered our investigations were not in the interest of pure science, and progressed only in any one direction, to the extent of showing whether we were on the road to accomplish our prime object, or whether we were making a retrograde movement, and were, therefore, often suddenly interrupted and a new mode of procedure adopted, or a new form of coil tried.

It may be explained here for those who do not follow the improvements in air-heating surfaces that "compound-coil surface" is the covering of the pipes of an ordinary box-coil with a spiral or helically coiled wire, which is again wound in the helical form around the pipe of the coil. A section of pipe covered in this manner is shown in Fig. 124.

To warm air with plain box-coils requires from six to twelve pipes in height, when set in rows directly over each other, to make the air sufficiently hot to maintain the heat of rooms in cold weather with the limited supply of air that will pass through flues and registers with natural draft currents, the temperatures varying from 110° to 150° Fahr. at the registers, with pressure of steam ranging between 2 and 10 pounds, and other conditions, such as length of flues, their position in outer or inner walls, etc., being factors against or for the temperatures.

With cast-iron surfaces covered with tapering prongs and known

as "pin surfaces," a depth of 6½ inches will give results equal to a common box-coil ten pipes high; and should the velocities be quickened, the "pin" heater will pass the increased quantity of air without apparent diminution of its temperature and with an increased condensation of water, whereas the plain box-coil will at once show a fall of temperature due to the increased quantity of air, and will show but a comparatively slight increase of water condensed.

By progressive changes in box-coils covered with this "secondary" surface we have reduced the number of pipes in height for coils, until we find that with two pipes in height and with about the same floor-space occupied by the "pin" radiator, we can con-

FIG. 124.

dense equal or greater quantities of water, with equal or greater results in air-warming.

The points, I think, that will be of interest to the members will be our method of receiving the water of condensation; the temperatures at which the air generally leaves coils or heaters, and the average of the water condensed for the different heaters for a given time, with the progressive forms of the new coils.

After making experiments with different forms and numbers of pipes in height of the new coil—which I will call "secondary-surface" coil—and with "pin-surface" and other old forms, we found that we were getting variations so great in the quantities of water (apparently) condensed that something must be wrong with our method.

In all these trials we were taking steam at about 50 pounds pressure from the boiler and reducing it through a "Curtis" regulating valve (*a*) to 20 pounds, with slight variations, thence passing

it to a receiving cylinder (*b*), 9 inches in diameter by 42 inches long, as shown at Fig. 125, with a steam-trap (*c*) at the lower end to take away the water separated or condensed.

From this separating cylinder we again reduced the pressure through a "Handren & Robins" regulating valve (*d*) to 2, 5, or 10 pounds pressure, as required for the tests, the object of the latter valve being to secure a constant pressure in the heaters under test, and which we found could be done within the limits of one-quarter of a pound.

The steam as it passed through the latter valve was assumed to be at maximum density. It was certainly as free of water when

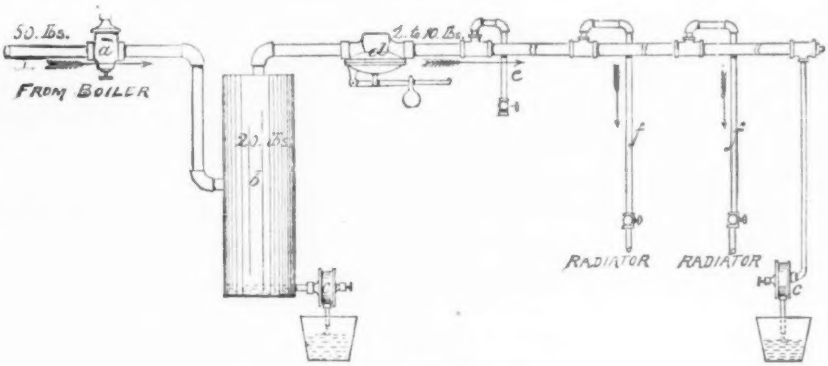


FIG. 125.

it expanded through the regulator as we can hope to get it in best practice.

Our first method of receiving the water of condensation was to use the Hawes steam-trap on the return end of each coil or radiator, the trap being used in the ordinary manner. Difficulty was found in adjusting the traps to pass only the water of condensation, and after using them some time, and comparing the results, it was apparent we were getting that which would be of little practical use *with short tests*, as the amounts of water varied so greatly for 20-minute tests for the same heaters under, as far as could be seen, similar conditions. It would appear that the amount of water for a number of consecutive tests should give the actual average, but apparently there was an element of uncertainty, caused presumably by the draft of steam through the radiators disturbing water that could lodge in the bottoms of bases or in the enlargement of fittings below the line of the bottom side of the outlet-pipes.

When the trap was "blown through" the passing steam drew the pockets empty, and these places had to fill before any considerable quantity of water ran off by the trap. This water could not be accounted for, and in the "pin" radiator of six sections, on account of the large flat bottoms of its sections, this water made a considerable factor. In the small pipe-coil of only 16 1-inch pipes, 38 inches long, it was probably not one-tenth of the amount. The great internal surface of the pin-sections compared to the small internal surface of the pipe affected the results when agitated, as all these surfaces are covered with beads of water draining slowly toward the outlet.

It might be well to remember here that we were making comparative trials only between heaters for our own purpose, and that previous to this we did not think it necessary to take into consideration matters, apparently slight in themselves, which we found afterward could not be neglected, such as a particular way of branching pipes, feeding different heaters under test at the same time, or other little matters which may appear as I go along.

The water, as it left the traps, was received into iron buckets of equal size and weight. Some vapor, or steam, passed the traps or had to be allowed to pass to assure us we were not holding water back. This vapor condensed on the sides of the buckets, and would in itself be sufficient to destroy the value of any but preliminary and comparative tests. The condensation in the pipes leading to the heaters from the main also brought an error into the result which should be eliminated by careful tests of the pipe alone, and a constant error established for time, but for our purpose of comparison we again omitted to take it into consideration, as all were served alike, and as the condensation from *plain direct* surface, as this would be, would always be against the heater doing the greatest duty.

To prevent condensation on the sides of the pails we tried receiving the water of condensation under water from the traps in the receiving pails, noting the rise of temperature of the water in the pails for comparison. As we had to contend against the cooling of the water in the iron pails, we substituted wooden ones, our objections to not using them earlier being variations in their weights, due to soakage, and the inability to drain them quickly. We still found too great variations for results from the same heaters, the steam leakage from the traps now entering into the problem, as it was all condensed in the pails; the trap with the

greatest percentage of leakage destroying the comparison, and the leakage in any case raising the quantity of water above that actually condensed by the heater. This warranted us in going no further in this direction, and proved conclusively that it was impossible to get accurate results as to the water condensed by this style of trap, or, in fact, from any trap, and that the attempt to measure the units of heat by the rise in temperature of water, whether discharged through a trap or not, could not be relied upon so long as the accuracy of a trap or any other means of drawing water from a heater could not be assured, the latent heat of the escaping vapor being a large and unknown factor.

At this time I determined to receive the water of condensation

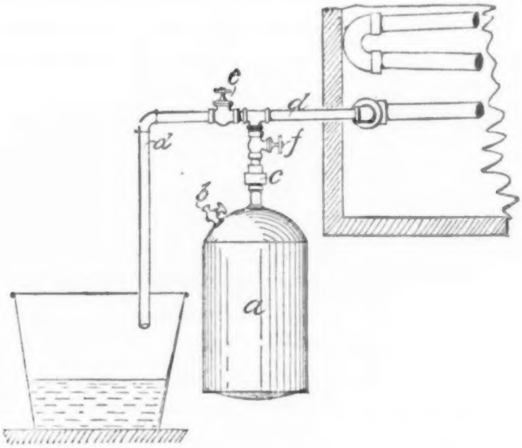


FIG. 126.

under pressure and by gravity only, or, in other words, by *overflow*, and without traps, receiving only the water which would overflow by the natural increase for a given time. The arrangement shown in Fig. 126 was then substituted and used at the drip-pipe of each heater. It consisted of a galvanized-iron cylinder, *a*, $8\frac{3}{4}$ inches long by 6 inches in diameter, not including the bulges of the heads. It had an air and vent cock *b*, a ground union joint *c*, with $\frac{1}{2}$-inch pipe connection *d d*, and valves *e* and *f*. These cylinders were made of No. 22 iron, double-seamed and soldered, and safely bore 20 pounds pressure, and would presumably bear the full boiler-pressure with safety.

By this means the water of condensation and the air was drawn

away from the heaters before the tests were commenced, through the valve *c* and the pipe *d*, into a pail. When all was nicely warmed the valve *e* was closed and the valve *f* opened and water allowed to run into the cylinder for an indefinite time, but sufficiently long to assure us the effect of blowing at the cock *e* had been overcome by the filling of pockets or depressions in the pipes or castings, and that only water which *overflowed* was being received. Then the cock *f* was closed for a moment and the cylinder removed and another substituted; the test commencing from the moment of closing *f*, the water forming during the time of the change being held in the pipe connections and allowed to run into the cylinder the moment the union was screwed tight, air being allowed to escape by the cock *b* until vapor appeared.

At the end of twenty minutes the valve *f* was closed, signifying the end of one test and the commencement of another, and so on for a number of hours.

The cooling of the cylinders between tests and the steam necessary to warm them were not taken into consideration.

After adopting this method we found the water of condensation to be very much more uniform, and the first four consecutive trials gave 3 lbs. 14 ozs., 3 lbs. 14 ozs., 3 lbs. 13 ozs., and 3 lbs. 13½ ozs., for an experimental secondary-surface heater, two pipes high, directly over each other, by six pipes wide and 42 inches long over bends and headers, made of ¾-inch pipe with 18 pounds of No. 14 round wire forming the secondary surface, the nominal heating-surface being 30 square feet, the actual pipe-surface being 13 square feet, and the secondary wire-surface being 22.5 square feet. By "nominal surface" is meant the commercial rating.

At the same trial six sections of "pin" center-connection indirect radiators, nominal surface 60 square feet, gave 5 lbs. 8½ ozs., 5 lbs. 7½ ozs., 5 lbs. 7¼ ozs., and 5 lbs. 8⅜ ozs., this being the standard heater. The variations, as you may see, were no greater than an ounce, whereas by our former methods it often exceeded a pound for the same heater on 20-minute tests.

Our results were so regular by this method that we went no further in this direction, reasoning that we were getting the actual water condensed, plus the condensation in the ¾" inlet pipe and valve and that formed in the drip-pipes and cylinders which received the water, and which may be assumed approximately to be not over six ounces per square foot of the exposed pipe or cylinder.

In all the comparisons drawn hereafter, whenever the term

"standard heater" is made use of, it must be remembered it is six sections of "pin" radiators, with center connection, each section containing 912 pins, being nominally *ten* square feet of surface to a section, and actually 8.37 square feet (and sometimes called $8\frac{1}{2}$ square feet), the casting being $6\frac{1}{2}$ inches in height by 41 inches long, and each pin with a base of one-half inch, a top of one-quarter inch, and a length of eleven-sixteenth inches, with the pins in staggered rows, as shown in Fig. 127.*

The floor-space occupied by a single section is 41 inches in length by 3 inches in width, six sections being a little over 18 inches in width, when the gaskets are between them. To find the floor-space for any number of sections, therefore, allow three inches for the width of each section, plus one-half inch for each outside

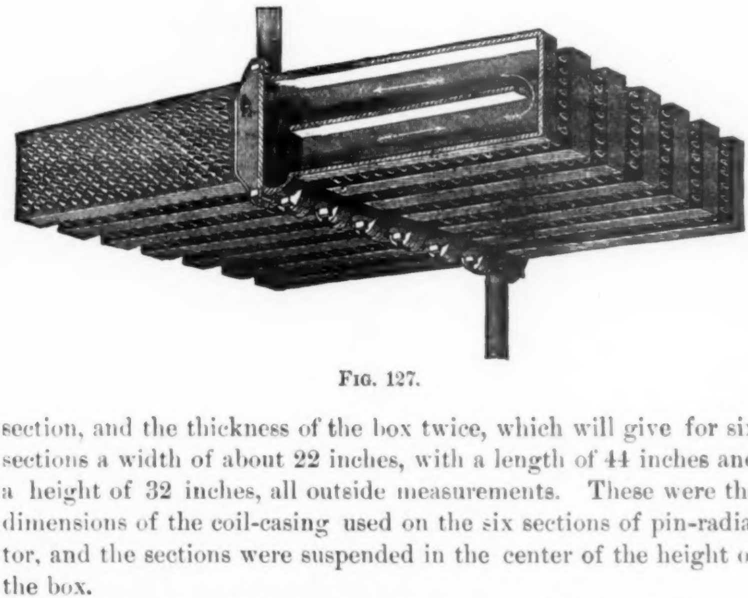


FIG. 127.

section, and the thickness of the box twice, which will give for six sections a width of about 22 inches, with a length of 44 inches and a height of 32 inches, all outside measurements. These were the dimensions of the coil-casing used on the six sections of pin-radiator, and the sections were suspended in the center of the height of the box.

The trial heaters for indirect purposes were 42 inches long over the fittings, so as to take the same length box as the "pin" sections, and were of the box-coil class, in some cases made of $\frac{3}{4}$-inch pipe, and in others of 1-inch pipe, each pipe being covered for 34 inches of its length with secondary wire-surface, the dimensions of which will be given, with the weight and shape of the wire used,

* Some makers use 938 pins on a section and others less than 900. Sections with 938 pins have a total surface of 8.49 square feet.

and will be alluded to as "compound coil," and should they be simply alluded to as "pin" and coil, the former will mean the standard heater of six sections and the latter the trial heater for the time.

Early we found there was no necessity for making the compound coil higher than four pipes.

Trial 1.—A trial coil of compound form made of ¾-inch pipes, four pipes high by six pipes wide, with 3 pounds of No. 14 round wire in the helical form on each pipe, with an assumed nominal or commercial value of 60 square feet, an actual *plain* surface of

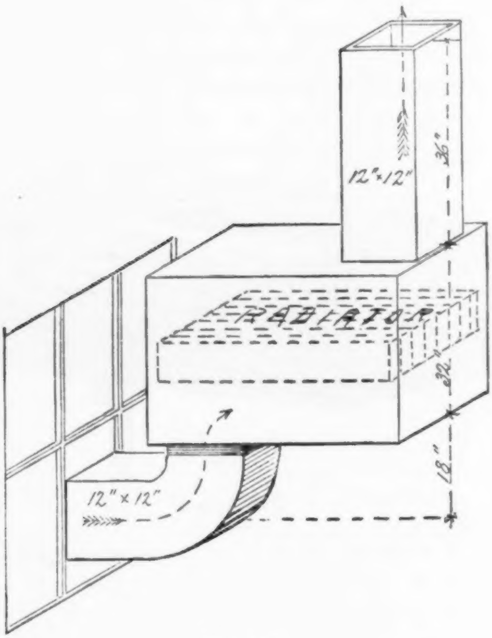


FIG. 128.

24.5 square feet, and a secondary or wire surface of 45 square feet, was then tried against a standard heater of 60 square feet of nominal or commercial surface, and an actual surface of 50.22 square feet—natural draft only being used. The inlets to the coil-boxes were each *one square foot* in cross-section and the outlets *one square foot* in cross-section, and otherwise, as shown in Fig. 128.

The results were: *Trial heater*—Water condensed per square foot (nominal), 4.85 ounces per hour; pressure of steam, 2½ pounds;

air entering box, 62.25° Fahr.; air leaving box, 170° Fahr.; velocity at mouth of outlet, 244 feet per minute. Standard heater—Water condensed per square foot (nominal), 4.725 ounces per hour; steam pressure, $2\frac{1}{2}$ pounds; air entering box, 61.5° Fahr.; air leaving box, 157° Fahr.; velocity at mouth of outlet, 266.7 feet per minute.

This gave a higher temperature of air with less velocity and slightly more water condensed for the trial heater than for the standard.

Trial 2.—A trial heater was then used of two pipes high and six pipes wide, with 3 pounds of No. 14 round wire per pipe—or just half the former trial heater—and the results were: Trial heater, 30 square feet nominally; water condensed per square foot (nominal), 5.71 ounces per hour; steam pressure, 4 pounds; air entering, 69.5° Fahr.; air leaving, 140° Fahr.; velocity at top of outlet, 220 feet per minute.

Standard heater, 60 square feet nominal; water condensed per square foot (nominal), 3.91 ounces per hour; air entering, 71.5° Fahr.; air leaving, 157° Fahr.; velocity at top of outlet, 230.5 feet per minute. Figure 129 shows cross-section of this trial heater, and the principal point to be observed is the great amount of work done by the two lower rows of tubes of a heater, even when the conditions are somewhat unfavorable, as indicated by the slow motion of the air through the standard heater compared with the foregoing trial. The velocities of the air entering the boxes were noted in this trial (No. 2) to be 180 feet per minute for the trial coil and 190 feet for the standard heater.

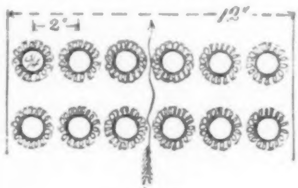


FIG. 129.

Trial 3.—With the same coils, when the conditions of air were about the same, but with 10 pounds of steam, the condensation was 6.25 ounces per hour for the coil and 4.4 ounces for the pin (nominal surface).

Atmospheric conditions seem to affect the velocities of air through the heaters, and also appeared to affect the amount of condensation.

Trial 4.—This trial was made on a damp day. The trial heater was four pipes high by six pipes wide, and was the same in every respect as the trial heater in No. 1. Trial heater gave water condensed per square foot (nominal), 3.48 ounces per hour, with 10

pounds steam pressure; temperature of air leaving, 168° Fahr.; temperature of air entering, 71° Fahr.; velocity of air leaving, 172 feet per minute; velocity of air entering, 138 feet per minute. While the standard heater gave 3.83 ounces water per hour; temperature of air leaving, 169.5° Fahr.; temperature entering, 72° Fahr.; velocity leaving, 169.5 feet per minute; velocity entering, 127 feet per minute.

In the foregoing experiments the wire helices were all of No. 14 round wire, the external diameter of which was 0.4 of an inch. In the following experiments with box-coils, No. 14 square wire was used, with a larger helix, and the pipes used in the coils were all *one*-inch steam pipe. The helix in this case was .56 of an inch external diameter.

Trial 5.—The trial heater in this case was a box-coil of only two

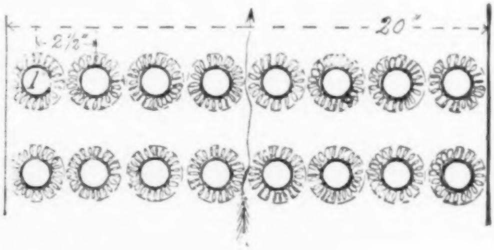


FIG. 130.

pipes in height by eight pipes wide, 42 inches long over the fittings and containing three pounds of No. 14 square wire to the pipe, with an actual pipe measurement of 22.5 square feet and a secondary surface of 60.8 square feet, the assumed commercial surface being 60 square feet nominal. The trial heater gave 13 lbs. ½ oz. of water for the hour, with 6 pounds of steam-pressure, and the standard heater gave 12 lbs. 12½ ozs. water for the hour, which will be 3.466 ozs. per hour per square foot for the trial heater and 3.4 ozs. for the standard heater, when they are given the same *nominal* value. The temperature of the air leaving the box for the trial heater was 165°, and for the standard heater 159°. Fig. 130 shows the cross-section of the pipes of the trial heater.

Trial 6.—This trial was made with the same number and size of pipes in the box-coil, the only difference being the pipes were staggered, as shown by Fig. 131. Trial heater gave 14 lbs. 1 oz.

for the hour, and the standard gave 13 lbs. 8 ozs. for the same time, the pressure of steam being 4 lbs. This would be 3.75 ozs. per hour per square foot for the trial heater and 3.6 ozs. for the standard, the temperature of the air leaving each heater at 178° Fahr.

Trial 7.—The same heaters with the top of the boxes removed gave trial heater, water, 14 lbs. 8 ozs. for the hour; standard heater, 14 lbs., or 3.86 ozs. and 3.73 ozs. per hour respectively.

As these experiments were made at a time when the entering air ranged between 60° and 75° Fahr., the question arose as to whether the trial heater would do equal work with the standard if they were both taking air at, say, zero, or even higher, say °+20. To prove this, to a certain extent at least, we tried cooling the air with ice by passing it down through sieves of broken ice. It was

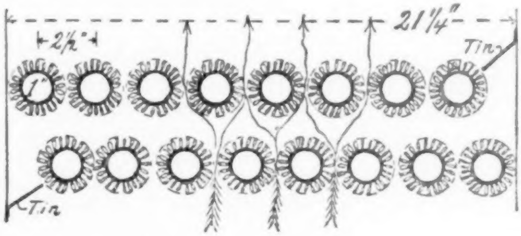


FIG. 131.

with difficulty that by this method we cooled the air from 69° to 50°, and from it we obtained no practical results.

We then resolved to force the air through the heaters with a fan, reasoning that it could make little difference whether the heat was taken from the heaters by extremely cold air at natural-current velocities or moderately cold air with *forced* currents, where we had it in our power to pass three or four times the air that would pass by natural currents. The remainder of the experiments on indirect heaters were conducted with the staggered coil, just before described and shown in Fig. 131, the difference being in the amount of air passed or in the boxing and air-openings.

Trial 8.—*Trial heater* with staggered pipes (Fig. 131), fan running, inlet and outlet for air each one square foot: Water condensed for an hour, 32 lbs. 4 ozs.; steam-pressure, 5 lbs.; water condensed (nominal square foot) per hour, 8.6 ozs.; air entering,

72° Fahr.; air leaving, 141.2° Fahr.; velocity, 685 feet per minute at the outlet (or 685 cubic feet of air at 141.2° Fahr.).

Standard heater gave: Water for the hour, 34 lbs. 12 ozs.; water condensed per (nominal) square foot per hour, 9.26 ozs.; air entering, 72° Fahr.; air leaving, 141.6° Fahr.; velocity, 602 feet per minute at outlet (or 602 cubic feet of air at 141.6° Fahr.).

It will be noticed in this trial that the trial heater gave the smallest condensation and gave the greatest number of units of heat to the air. This is accounted for on the supposition that the direct radiation from the standard heater is very much greater than from the trial heater, on account of the very small primary surface of the box-coil, as compared with the primary surface of the pin-radiator, and also that the secondary surface on the box-coil intercepts the radiant heat very largely, and communicates it to the air by contact, the same as it does the heat it conducts by actual contact with the primary surface. Something like this goes on between the pins as well, as they radiate one to the other and reradiate, but as the conducting power of the pin is sufficiently great to supply all the heat the air can take away, the result is different, and in any case the flat outsides of the castings and top and bottom lose heat that is not communicated to the air. The heat lost by radiation in both cases is very apparent, at the low velocities, as the condensation is very much greater for the units of heat added to the air than with the high or forced velocities, the times being the same. This, of course, is to be expected, as the heat lost by radiation is directly as the time, while that given to the air must be in a ratio nearly equal to the amount of air passed.

Trial 9.—Same heaters as before. Trial heater—Water for the hour, 31 lbs. 8 ozs.; steam pressure, 5 lbs.; water per square foot per hour, 8.4 ozs.; temperature entering, 70° Fahr.; temperature leaving, 135° Fahr.; cubic feet of air passed per minute, 655.

Standard heater—Water for the hour, 33 lbs. 8 ozs.; water per square foot per hour, 8.93 ozs.; air entering, 70 Fahr.; air leaving, 137° Fahr.; cubic feet of air passed per minute, 560.

Trial 10.—The same heaters were used with a decreased quantity of air. Water per hour, 31 lbs.; water per square foot per hour, 8.16 ozs.; air entering, 82° Fahr.; air leaving, 149.6° Fahr.; cubic feet of air passed per minute, 484.

Standard heater—Water per hour, 31 lbs. (same as trial heater); water per square foot per hour, 8.16 ozs.; air entering, 82.5° Fahr.; air leaving, 152.3° Fahr.; cubic feet of air passed in a minute, 413.

It will be noticed that with Trial 8 the temperature of the escaping air agreed, and that with this last trial (10) the quantity of water condensed was just equal. These three experiments also show a rapid decrease of air passed without a corresponding decrease in water condensed, which goes to substantiate the constant loss of heat by direct radiation.

In these trials with fan-pressure, the air being supplied from a

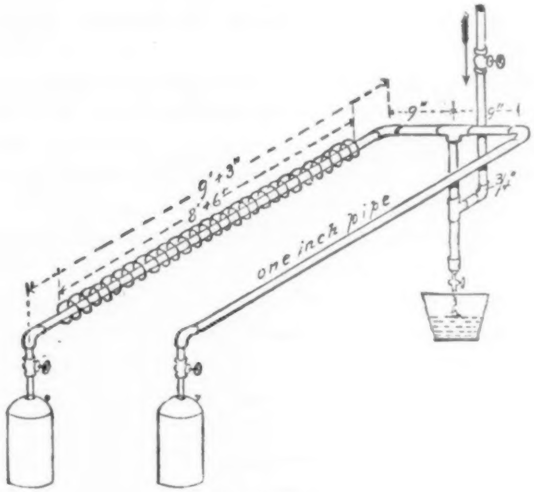


FIG. 132.

common source, and consequently equal pressure, every precaution being taken to give the heaters equal conditions, it will be noticed the coil passed the greatest quantity of air. This is only what might naturally be expected to follow, as the passage for air between the pipes and secondary coils is much greater than in the pin-radiator, and the resistance consequently less.

Trial 11.—This trial was made with *four* pounds of secondary wire surface on each pipe, and gave 36 lbs. 12 ozs. water for the hour, against 32 lbs. 12 ozs. for the standard heater, the pressure of steam being five pounds. This trial was made with the fan running, passing about 600 cubic feet of air per minute through each coil.

Trial 12.—The same coil was tested against a standard heater, natural currents, with the whole bottom of the box removed in each case. The condensation for an hour was: Trial coil, 16 lbs. 6 ozs; standard heater, 14 lbs. 14 ozs.; steam pressure, 5 lbs.

Trial 13.—Same coils, whole top and bottom of coil-boxes removed, allowing all the air that could possibly pass the coils or radiators, with natural currents to go by: condensation per hour, trial heater, 20 lbs. 13 ozs., and for standard heater, 16 lbs. 14½ ozs.; steam pressure, 5 lbs.

Fig. 132 shows how tests were made with a plain and a wire-covered 1-inch pipe. Each pipe was 9′ 3″ between centers of elbows and nine inches from center of elbows to center of tee—being, say, 10 feet lineal of *one*-inch steam-pipe. In the one case it was plain; in the other the pipe was covered with the wire helix, nine pounds of No 14 square wire being used.

In the plain pipe the condensation per lineal foot for an hour was 1 lb. 15¾ ozs., and in the pipe with the secondary surface on it, it was 3 lbs. 6¼ ozs. The pressure of steam was 10 lbs.

This last trial was made to establish the value of the secondary surface for wall coils, where the air is unconfined and not forced to pass between the loops of the coil.

Where a board was used at each side of the coil to confine the air very much more water was condensed, but the results were not accurately recorded. This will account for the coils doing a greater duty per lineal foot in most cases.

If we now consider the surface of the receiving cylinders to have the same condensing value as the plain pipe surface, we will have a little under two ounces of water condensed per hour per lineal foot of the plain inch pipe, and if we divide the increased condensation due to the secondary surface by the length of pipe actually covered, we have 2.65 ounces per lineal foot as the increase, and the total value per lineal foot 4.65 ounces for the compound surface.

More experiments will be made during the coming cold weather which I hope to be able to give to the Society at a future meeting, when, if possible, I will have the matter tabulated and the results reduced to some common standard of comparison.

APPENDIX IV.

TO

NOTES ON COMPARATIVE VALUES OF METAL SURFACES FOR WARMING AIR.

BY WM. J. BALDWIN, NEW YORK CITY.

The velocities of the air through the twelve-inch square hot-air flues, as measured by the anemometer, are assumed in the paper to represent the cubic feet of air passed for the same time. This assumption is *undoubtedly incorrect*, as the units of heat found in the air, on such a supposition, exceed those in the steam condensed by about 33 per cent. This is partly accounted for by the fact that a large anemometer was used (nearly seven inches in diameter), which, when introduced into the flue through a trap and kept moving by the muffled hand and arm of the operator, reduced the area of the flue sufficiently to give a greater lineal velocity through the reduced part than through the remainder of the square flue. The corners of the flue, also, must have had a slower movement of the air than that near the center, and as the anemometer was large, it did not go sufficiently into the corners, and was probably held near the center of the current for too much of the time; therefore the *units of heat* in the steam condensed are the real and only reliable measure of comparative efficiency of these surfaces.

CCIV.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

XIIth Meeting.

No. 204.—8.

CAN condensation be diminished in the cylinder of a steam engine by the use of a non-conducting lining of glass, porcelain or other similar material?

DISCUSSION.

Mr. Chas. E. Emery.—During the years 1864–8 I was an Assistant Engineer in the U. S. Navy, detailed as one of the Engineer Corps making experiments on the expansion of steam at the expense of the government at the Novelty Iron Works, New York, under the general directions of Horatio Allen, Esq., President of the Novelty Iron Works, and Chief-Engineer Isherwood, at the head of the Bureau of Steam Engineering, U. S. Navy. These experiments covered a wide range. The principal feature was to ascertain the amount of useful work which could be obtained with a given quantity of steam used with different measures of expansion, to determine which, required the use of a number of cylinders of different sizes. The results of these experiments are published in *Appleton's Encyclopædia*, but in so condensed a form as to be of little or no service to the profession. To make them useful, some party connected with them should publish, in connection with the results, a statement of all the conditions, thereby explaining many seeming discrepancies. As these experiments progressed, I was much interested in the general subject, and finally, in 1866, tested the question of the quantity of steam which could be measured out of three small cylinders, one of iron, another of iron enameled, and a third of glass. The two first named were simply pieces of 1½-inch gas pipe about 18 inches long. The three cylinders were made of exactly the same capacity, as measured with water, by means of displacement screws in the iron ones. Each cylinder

was in turn connected with a valve which admitted steam from a boiler and exhausted it into a condensing coil from which the water of condensation was collected into a bottle. The exhaust port of the valve was contracted where it first opened, so as to let the steam escape slowly at first, and then more rapidly, thereby imitating in the cylinders the range of temperature in the cylinder of a steam engine, but of course without the performance of work, as there was no piston in either cylinder. As each cylinder was tested, it was covered with hair-felt so as to eliminate the influence of external temperatures. With $22\frac{1}{4}$ lbs. of steam, the enameled tube, for a given number of movements of valve, measured off 26 per cent. less steam than the iron tube, and the glass tube 52 per cent. less; that is, a glass tube of the same actual capacity as an iron one, apparently held less than half as much steam, the fact being, of course, that the surfaces of the iron tube were cooled by the lower temperature of the exhaust, and required first to be heated by the incoming steam before the pressure rose to maintain the temperature, and it required a little more steam to do the reheating than to fill the cylinder. Further experiments were made with 20 lbs. of steam, dried by passing it through a pipe jacketed with 75 lbs. of steam, when the enameled tube saved 30 per cent. and the glass tube 60 per cent. as compared with the iron.

These comparatively simple experiments were apparently sufficient to warrant the construction of an experimental engine to test the same principles doing actual work. A box-shaped surface condenser was constructed with two independent compartments connecting severally to the two ends of a double-acting air pump, and upon the condenser were erected two cylinders, each 8 inches in diameter by 8 inches stroke, with piston rods connected to a cross head between the two, giving motion through side connecting rods to external cranks on a main shaft at the end of the frame. Each cylinder was provided with a main slide valve and sliding adjustable cut-off, the latter as well as the air pump being operated from the main cross head. One cylinder was simply well constructed in the ordinary way, the other, located at the end of the frame opposite the crank shaft, was varied as required to compare with that first named. The first experimental cylinder constructed had the valve seat separate from the cylinder, but bolted to it along the center line of the cylinder ports. The cylinder and the half ports connected with it, and the half ports connected with the valve seat, also the piston heads and the interior of the cylinder

heads, were carefully enameled. The process distorted the castings very much, but the separate seat was scraped down onto the cylinder so as to make a steam-tight joint, and the cylinder itself ground out by special machinery so as to be approximately true; several of the cylinders being spoiled in getting one that was satisfactory, and the one employed was sufficiently untrue to require grinding down to the iron for a little distance at one point to make it possible to run a piston in it. The piston was necessarily packed with hemp carefully plaited and set up, notwithstanding which, great difficulty was found in keeping it tight. When experimenting, both cylinders could be operated at the same time, using steam at the same pressure from the same boiler, but exhausting into separate condensers with the water of condensation delivered to separate hot-wells and tanks, where it was carefully weighed separately. The resistance of the engine was furnished by a large Dimpfel blower, driven at high speed, the discharge orifices being closed by a slide when it was desired to decrease the power. The largest saving was 27 per cent., which was obtained when using steam at 75 lbs. pressure, cut off at .03 of the stroke, but I find notes stating that the average result of experiments considered most reliable was about 17 per cent. I next took advantage of the loose seat on the lined cylinder to put in what I called a Cornish valve, by means of which the steam was admitted to one end of the cylinder only, and transmitted to the other end before being exhausted to the condenser. Meanwhile a large steam drum had been added to the boiler, and other precautions taken to ensure dry steam. The result was that the cost of the power was reduced in both engines. In later experiments the steam pressure was not allowed to exceed 40 lbs., on account of the difficulty with the pistons referred to, but with this pressure, cut off at .15, the cost of the power in the common cylinder was 31.991 lbs. of water per horse-power per hour, and in the lined cylinder 23.12, a saving in the latter of about 27 per cent. In another case with steam at 25 lbs. pressure, cut off at .36 of the stroke, the cost with the common cylinder was 39.8, and with the lined, 27.1, a saving of over 30 per cent. No practical work could, however, be done with the lined cylinder, on account of the difficulties with the piston—so finally that cylinder was bored out to the iron, and packed with rings in that ordinary way, the larger portion of the enamel remaining on the cylinder and piston heads and in the passages. The result was to reduce the saving to from 12 per

cent. to 18 per cent. Other experiments were made with this cylinder, testing different degrees of cushioning and various other changes and conditions, which I cannot take time to enumerate. In the final experiment with non-conducting materials there was constructed a cylinder with trunk piston working through rings at the middle of the cylinder with spaces in cylinder on either side of the rings lined with glass plates ¼ inch thick. Plates of same thickness were also applied on the cylinder covers and piston heads. The wearing surface of the piston was of iron, but the glass being ground approximately true, the iron surface was not exposed to direct radiation from the body of steam in the cylinder. The cylinder ports passed out through the cylinder heads and were enameled up to the valve faces. This construction was very expensive, but it was made to move perfectly without steam on. On heating up the combination, however, with the greatest care in starting the engine, crunching noises immediately commenced, necessitating a stop to remove the pieces of broken glass. Another trial resulted in the same way, and still another, until a very large percentage of the glass was displaced, and finally the engine was got to run well enough, but in no condition for tests, so the experiment was abandoned. In this case the glass for the interior of the cylinder was moulded in segments accurately ground to each other at the edges. The glass on the cylinder and piston heads was moulded into a casting provided with dovetail recesses, the projections extending only about half way through the thickness of the glass. The glass in practice flaked off down to the projections, and cracked in all directions, though occasionally some pieces of large size remained. Great care had been taken in the annealing of the glass, in fact, the cylinder piston heads were necessarily annealed with the iron plates into which they were moulded.

Finally, years afterward, through the liberality of Capt. C. P. Patterson, Superintendent of the Coast Survey, new cylinders were made of different sizes, and connected to form a compound engine, when the whole apparatus was erected at the Metropolitan Mills, owned by Messrs. Hecker & Bros., of New York. This firm furnished steam and facilities for the experiments. The results of the trial are not accessible at the moment, but the experiment showed that the greater portion of the economy due to the use of non-conducting material in steam cylinders could be secured by compounding the engines.

I will only add, in conclusion, that within a year Mr. George

Westinghouse, Jr., called upon me to learn my experience with non-conducting materials, and in the end fitted up one of his engines with enameled plates on the cylinder heads and piston. The results, as reported by one of the members of the Society to Mr. Westinghouse, showed no economy by this change. I attribute this to the speed at which the engine was run, the alternations of temperature being so rapid that the moisture on the surface had more influence than the quality of the material.

It will be interesting to state further in this connection that one of a series of experiments made was to ascertain the effect of variation in speed of revolution with constant steam pressure vacuum and cut-off. The results are shown in the following table:

TABLE.

Steam pressure	80 lbs.
Vacuum	24 inches.
Cut-off	23 inches.

Revolutions per minute.	Water per 1 H. P. per hour.
30.51	40.25 lbs.
44.96	37.36 "
58.34	34.56 "
60.23	34.50 "
80.21	31.60 "
105.61	29.38 "

The quantity of water required is considerably less than will be found necessary in other small engines, even though provided with automatic cut-off gear, for the reason that these particular experiments were made with the cylinder which had remaining on its heads and in the ports large portions of the enamel used in the experiments above referred to.

It would naturally be supposed that a portion of the better results was due to the vacuum, but other experiments showed that in these small engines there was no gain by having a vacuum when the steam pressure was 80 lbs. or upward.

Mr. Church.—As to the experiments of Mr. Westinghouse alluded to, it was impracticable to enamel more than the cylinder heads and the piston heads. If the entire surface had been enameled better results might have been obtained.

No. 204.—9.

What is the best method of determining approximately the economic efficiency of small steam engines, without expensive appliances and preparation?

DISCUSSION.

Mr. Towne.—If I understand the purport of this inquiry, it is whether there is any method by which small engines can be tested as to their efficiency and some reasonable determination arrived at without expensive preparation and special appliances. If any member present can give any information on that point, it will be very interesting, I think. We all know that it can be done in an expensive way. The question is whether there is any approximate way of getting at it with only moderate expense.

Prof. Lanza.—It seems to me that the feasibility of making such tests cheaply must depend upon the possibility of obtaining some cheap and portable form of condenser to use in addition to the indicators. This condenser should be sufficiently large to condense all the exhaust steam of the engine.

Mr. Towne.—Of course it is assumed that an indicator might be made use of. I understand Prof. Lanza's suggestion to be that that be supplemented by a condenser.

Prof. Lanza.—If such a portable condenser, preferably a surface condenser, could be devised, we could carry it around along with the indicators. Then we could attach it to the exhaust pipe of the engine, and thus, condensing all the exhaust steam, we could weigh the condensed water by delivering it into a tank on scales, which could be easily obtained anywhere. We could thus determine the water per horse-power per hour, the condensation and re-evaporation during the stroke, and all other quantities needed in such a test. Perhaps such a condenser could be made of small brass tubes and iron pipes.

Mr. Towne.—That leaves open, however, the question of the amount of moisture.

Mr. Church.—I have been accustomed in instances of that kind to carry a very low water line in the boiler, even where it might be said to be dangerously low, securing thereby exceeding dryness of steam. I would judge of this dryness only by the color of the steam. Then with the Prony brake, a very efficient and cheap kind of which we are using daily in our test room, we can arrive at

what is called for here; namely, an *approximate* method of determining the efficiency of small engines. This has been with me an exceedingly satisfactory method, and gives a remarkably accurate determination of water consumption.

Mr. Durfee.—It has occurred to me since Prof. Lanza proposed the condenser, that a cheap form of condenser might be devised by prolonging the exhaust pipe into the reservoir, and then injecting into that pipe, by means of a fan or other method, a blast of cold air which would condense the steam. I think that some such construction could be made for a small engine sufficiently portable, inexpensive and efficient.

No. 204.—10.

What is the best kind of stuffing boxes and packing for pneumatic pressure, for moving parts (reciprocating or rotary) and for joints?

DISCUSSION.

Mr. H. R. Towne.—I may state that on making this inquiry of a firm engaged in the manufacture of pneumatic appliances, particularly pumps and presses, I was told that the cup-leather is the best method of packing stuffing boxes. But I believe also that hemp gaskets plaited in the usual manner are sometimes used. The objection apparently to cup-leather in a case of this kind would be the absolute dryness, which would seem liable in time so to dry the leather as to make it very brittle and incompetent to do its work. I am in hopes that some one present will be able to shed some light on the subject.

Mr. Bancroft.—There was a packing used in France made by Mr. Giffard which was stated to give extraordinary results for this purpose. As he constructed it, it was made of vulcanized rubber, the side next to the moving part being vulcanized considerably harder than the other side, and this ring of rubber was placed in a seat made with curved sides so that the air pressure pushed it forward and packed it against the reciprocating parts, very much as the cup-leather acts, but without its disadvantages.

The President.—Was there any wearing of the rubber by attrition?

Mr. Bancroft.—A slight wear; but the pressure of the air packs

it forward and takes up the wear. We have used this same form of packing for hydraulic work very successfully, using a leather ring instead of rubber, the seat being formed as shown in Fig. 116.

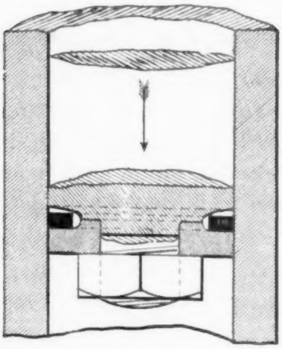


FIG. 116.

The President.—Are there any gentlemen here who have had experience with the Bessemer process? In the Bessemer process of course packing is required very much. Can Mr. Durfee tell us anything about that?

Mr. Durfee.—I have constructed packing rings for the piston's high pressure blowing cylinders in this way: The piston is made with an ordinary "junk ring," the same as would be made for a three-ring metallic packing, but with more space between the inside of the cylinders and the outside of the junk ring. That space was packed with segments of leather, breaking joints with each other, laid in loose and with their ends pretty close together, and the follower screwed down tight, and the whole thing turned off. That made a very satisfactory packing, indeed. Fig. 117 shows its arrangement, together with the method of setting it out by a central adjusting screw, A, whose conical end acts upon the inner ends of five radial rods (one of which is shown at B). The outer ends of these rods abut against the middle of curved blade springs having their extremities bearing against the inside circumference of the junk ring at points equi-distant from each other; thus by turning the adjusting screw, A, the junk ring is acted upon at ten points of its circumference. Obviously this arrangement is not well suited for horizontal cylinders.

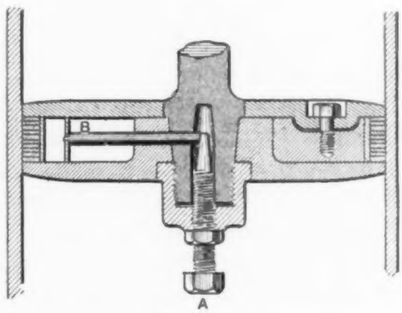


FIG. 117.

I used the above described arrangement for the first time twenty years ago, and I think that the leather segments for the packing of the pistons of blowing cylinders are now quite generally used.

For hydraulic packing, cup-leathers are very generally used, and many persons find fault with them, saying they are good for nothing; but, for cup-leathers as for every other mechanical detail, there is a right and a wrong way of construction. We will suppose Fig. 118 to represent a vertical section of one side of a hydraulic cylinder having cup-leather packing on the end of its piston or "ram;"—now if the cup-leather is turned down at the corner A, and left unsupported on its convex side, it will last but a comparatively short time, as the pressure will crowd the corner of the leather into the angle made by the bottom of the ram with the side of the cylinder, causing cracking and failure at that point. I know of one instance in which the frequency of such failure, and the consequent annoyance and expense, caused the abandonment of the manufacture of a hydraulic organ blower which had been largely introduced, and which in every other respect gave entire satisfaction. Now if the construction is slightly changed, and a metallic support is provided for the rounded corner of the cup, its life will be very much longer than if this corner was left unsupported. For the support of these cup leathers I have usually used a hard brass ring let into the piston (as shown at B, Fig. 119) and turned to a thin edge behind the leathers, in such way that the leather before pressure is applied is in close contact therewith over the whole exterior surface of its rounded corner.

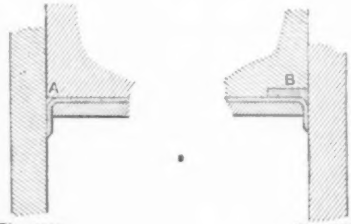


Fig.118 Fig.119

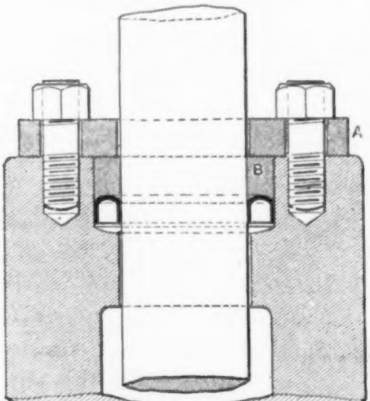


FIG. 120.

Mr. Bancroft.—With reference to that same supporting corner that Mr. Durfee speaks of, we had a curious experience some years ago with a differential accumulator, in which we had a stem running up through the accumulator with two different diameters, the difference in the area raising the weight. This accumulator should have lifted with 2,500 pounds

pressure per square inch, with no allowance for friction. The packing was arranged as shown in Fig. 120, the ring B being concave, as shown where it supported the leather forming two supporting corners. Now I put 3,500 pounds pressure on it and it didn't move, and I concluded of course that something was stuck fast abcut it. I examined it and found nothing whatever the matter; put it together again, and the same result followed. Then I concluded I would take that packing out. I removed the ring A, took the ring B out and put the pressure on, expecting to blow the packing out. I put 2,000 pounds pressure on and it didn't move. There was nothing whatever to hold it except the friction. I then, without removing the leather packing, put back the ring B reversed so that its straight side came against the leather, and that accumulator went up with 2,650 pounds pressure, and fell when the pressure was reduced to 2,350 lbs.

Mr. Towne.—What is your explanation of that, Mr. Bancroft.

Mr. Bancroft.—I presume it was simply that the pressure of the water made the packing tight at the edge of the leather, and that we had the friction due to the pressure over the whole length of the packing. I repeated that experiment several times with the same result, but we have abandoned that corner in consequence of that experiment.

No. 204.—11.

What metal or alloy can be substituted for steel in springs for watches, cars, wagons, etc.? Is aluminum, or aluminum bronze available?

DISCUSSION.

Mr. Bond.—I have had no experience in springs, excepting in regard to those which we used for the railroad oil-testing machines, made under the direction of Prof. Thurston; we tried only one material, and that was the steel used for car springs. We never have had occasion to use an alloy as a substitute for that purpose. The steel springs furnished by the A. French Spring Company (Limited), of Pittsburgh, seem to answer very well for this purpose. Tests which were made of these springs at the Stevens Institute under the conditions of a Fairbanks transverse testing machine showed no further permanent set after a dozen compressions, and the amount of deflection was uniform for constant incre-

ments of load of 500 pounds each. The springs used on our machine gun of course are of a good deal lighter weight, but they have always been made of steel so far as I know.

Mr. Stetson.—I do not know that I have had any experience that would be of any value in this line of springs. I had occasion once to make some very strong ammunition for cartridges, and we used aluminum mixed with copper. It was difficult to roll, and in its manufacture it formed a spring very quick by drawing. The temper seemed to draw into it so that the spring could be very easily produced, and it was necessary to anneal this metal very frequently, and by the common methods employed in copper it was reduced to ductility. One or two drawing processes would get a very fine spring temper in it. I should think that such metal might be valuable in a watch spring, for example, but I do not think it would be available for larger work, as it is too expensive.

Mr. Bond.—There is one case in point which I did not think of when I was speaking before. Several years since I found that the case spring of my watch was broken, and I sent it back to the factory at Waltham for repairs. Mr. Van Woerd, at that time superintendent, said that he would like to try a new material for the case spring, and said they were making experiments with an alloy called "magnesium bronze;" he said he would put that kind of a spring in the watch, and have me try it and see how it would work. It has now been in use for three years, and I will venture to say that there is no steel spring in any watch, at least in any I have ever seen, used the same length of time, that has the same elasticity. I handed the watch to Mr. Benedict, of New York, the other day, and asked if he could see any difference in the elasticity; he thought there was possibly a little loss of action at the open limit only. The spring seems to still have an equal tension in all positions. I lately had occasion to have the watch repaired, and the jeweler wanted to know what I was doing with a brass case spring. He thought it was brass, as it had that color, but it was the alloy called by Mr. Van Woerd "magnesium bronze."

Mr. Powel.—I know an instance of a small spring that was used in a machine for making a special cut on sewing machine work, in which a steel spring, necessarily small, on account of limited space, and of considerable stiffness, caused great trouble by breaking. The life of a steel spring, was from a few days to probably a month. From the construction of the machine it would have been difficult to change its size, and experiments were made in substitut-

ing different materials. We found that German silver would last for a year or longer where a steel spring would last only a few days, the size of the springs being the same.

Mr. Bond.—I might say, to supplement my former remarks, that the reason for using a bronze spring was that Mr. Van Woerd thought that a spring of this kind would thus be non-magnetizable.

Mr. Towne.—I may mention that in the construction of the large Emery pressure gauges, for very high pressures, a small hair-spring is required, and we have found a steel spring, gold plated, very satisfactory, the gold plating protecting the steel thoroughly against rust even under pretty severe conditions. Of cour-e this is only available where expense is not very important.

No. 204.—12.

What is the best place for the dome on a locomotive boiler.

DISCUSSION.

Mr. Soule.—The best location of the steam dome of a locomotive boiler seems to be purely a matter of judgment. The prevailing custom seems to be to put the dome of a wagon-top boiler on the wagon top, manifestly so that the throttle-valve shall be as high as possible above the surface of ebullition in order to secure dry steam. In the straight-top type of boiler the dome is generally placed forward in order to avoid passing wet steam, which would probably result if the dome were placed over the crown sheet. The almost universal English practice is to use the straight-top type of boiler. I have seen straight-top boilers with the dome over the crown sheet, but have generally learned that the arrangement resulted in a considerable amount of water getting into the cylinders. On the other hand, I have seen American locomotives with straight-top boilers having two domes, one placed over the crown sheet and the other on the barrel forward near the stack. In such engines the throttle-box is invariably placed in the forward dome; in that way dry steam is secured. But there is one discrepancy between theory and practice which confronts us in this connection, and that is, that while the high dome on a wagon-top engine is supposed to be so arranged in order to secure dry steam on account of the great distance between the surface of ebullition and the throttle-box,

nevertheless it will invariably be found that locomotive engineers, who have had experience with both wagon-top and straight-top engines, will tell you that they carry their water much higher in wagon-top boilers than they do in straight-top boilers, even when the arrangement of tubes and the level of the crown sheet is the same in both cases. I cannot find that this peculiar practice is based on any good reason other than the reduced chances of burning the crown sheet.

An incidental advantage in having but one dome, and having that as far back as possible, is that the engineer is thus better able to command a good view of the track on crooked portions of the line than he would if the steam dome were forward near the stack.

On the other hand, the prevailing tendency to use straight-top boilers with fire-box crown sheets stayed from the roof sheets, and again the more general introduction of the Belpaire type of fire-box, makes it almost imperatively necessary, in order to secure sound construction, that the dome should be placed on the barrel forward of the fire-box.

No. 204.—13.

Are descending smoke-flues admissible from boiler to base of chimney stack?

DISCUSSION.

Mr. Durfee.—In the course of my experience on several occasions I have had to do that very thing. In one case the smoke pipe from the boiler was carried into a flue descending as much as 30 feet, and going underground perhaps 20 feet. In the employment of Siemens Gas Furnaces I have on one occasion placed a gas producer at a considerable height above the furnace in which the gases were consumed. That is contrary to the usual construction; but the arrangement worked perfectly well. I have always built in the base of the chimney, for both boilers and gas furnaces in which descending flues are used, a small furnace having grates of 18 inches by 2 feet, burning a small amount of coal, just for starting the movement of the air in the chimney itself. The old practice in England and in some of the older rolling mills in this country was to carry the waste gases from the furnace to the space around a "hay-stack" boiler. There they rose ten feet

perhaps, then passed through a large horizontal flue which communicated with a central descending flue, and from this descending flue they passed by a horizontal underground flue to the chimney. In some cases these horizontal flues have been of very great length. I have in mind one mill where the chimney was 150 feet high and the gases from twenty-four furnaces were run into it, and the flue to the chimney was, I think, 15 feet in diameter.

Mr. W. J. Baldwin.—In a building in New York (the Hotel Dam) the boilers are under the sidewalk and the chimney is in the rear of the building. The architect provided a flue from the front wall of the building to the chimney. We found the water came up and filled this duct. A cast-iron box 24 inches square, and water-tight, was placed in the flue at its lowest point, its length being about 20 feet. The whole length of the underground flue was over 60 feet. From the boilers the flue went down perpendicularly, presumably 10 feet. The uptake of the chimney was between 90 and 100 feet, and probably two-thirds of the area of the box and underground flue. To start the fire the first time, we did as Mr. Durfee proposed—lit a fire at the base of the perpendicular chimney. Much moisture was found to form in this flue at first, due presumably to damp brick-work, etc. You must remember the cast-iron flue was practically in the water. After a few weeks' firing we found the dew-point was not formed within the flue. From then until now that flue has been in operation and is giving good satisfaction. The conditions were: about 10 feet downward, 60 feet horizontally, and 100 feet up. It was a cast-iron box in water. In time the earth presumably became dry behind it, but for three weeks fully there was a dew-point in the box, although since that there is not.

Such conditions, of course, are not desirable, and I only cite this to show an extreme case.

Mr. Church.—I mention the fact that the question originated with me in order to call attention to the large value of these topical discussions to the Society. I have in hand now a client who desires very much to carry a descending flue about 10 feet vertically downward, underneath the floor, and about 70 feet horizontally to the base of a stack. But about the middle of the length he desires to expand the flue out so as to make it serve as the floor area of a drying-room, and he desires to put baffle plates in the expanded flue. This form of construction is made desirable, in his judgment, not only for the sake of his drying-room, but by the construc-

tion of the building. I have not felt like lending my own endorsement to the scheme, and I have held the matter in check until I could receive something in the way of suggestion in the matter from the discussion before the Society. I would like to know, in an off-hand way, if such a plan as that, particularly considering the question of expansion of the flue, covering it with a cast-iron plate and taking advantage of it for drying purposes, would be a thing to venture upon.

Mr. W. J. Baldwin.—I would say that in the N. Y. Tribune Building the flues of the four boilers are carried down within the intermediate walls. The depth down is presumably 16 feet. Then they are carried under the floor of the basement to the uptake. There is no difficulty whatever with the draught. The chimney proper is probably one hundred and fifty feet in height. In New York, and I think in a great many other cities whose buildings contain boilers in their basements or under sidewalks, this condition exists. Where it does not exist is the exception. The chimney *has* to be in the walls of the building, and the boilers are always, or nearly always, under the sidewalks, and to get from the area under the walk to the chimney flue such a down-draught is necessary and is the custom, unless overhead iron smoke pipes are used, which warm the basement inordinately and which are not well liked by the fire underwriters.

When the horizontal length of flue underground is inconsiderable, the down flue is nearly balanced by the same length of uptake, and as the effective height of a chimney can only be rated from the level at which the gases leave the boiler, the efficiency of the down-draught is not as much impaired as may appear at the first consideration. After evaporation from brick-work, etc., ceases in the horizontal ducts, the effect of cooling the gases in these flues is comparatively slight.

Mr. Kent.—The case stated by Mr. Church introduces two new complications of the problem of the downward flue. I would say, in direct answer to the question, that descending smoke-flues are admissible but objectionable, and they may be employed provided you have means for producing a strong ascending current in the main chimney. Mr. Church's plan introduces a drying floor which will cool the gases in the chimney; secondly, he introduces baffling plates. Every baffling plate you introduce in a chimney flue checks the draught. In that case I should say it would be advisable to have some means in the chimney of creating an artificial

draught. I think it would be desirable to put an exhaust fan in the chimney which could be worked by boy-power before the engine was started and afterward by the engine.

Mr. Church.—I wish to disclaim entirely any responsibility for that plan. I could not submit for a moment to these baffle plates.

Mr. Towne.—The question under discussion seems a settled one in this respect, that the draught in the chimney is of course directly the result of the difference in temperature and gravity between the gases in the chimney and the air outside. The draught is diminished, on the other hand, by the friction it encounters in passing from the boiler to the top of the stack. Anything that tends to diminish the difference in temperature, by lowering the temperature of the gases, so far diminishes the draught. Anything that tends to increase the friction between the grate and the top of the stack diminishes it also.

With regard to the dropping of the flue, I have always considered that it is simply equivalent to cutting off so much from the top of the chimney, and that it is immaterial whether the flue drops or not, provided you add an equal amount to the top of your chimney.

So also in regard to baffling plates. If it is desirable to cause the gases to traverse a tortuous passage, it may be done if the friction thereby created is compensated for by an increased draught obtained either by heightening the chimney or by introducing artificial draught in any way.

With this view of it, I think there is no objection whatever to the use of a down-take in a smoke flue if the question of cost is properly considered. If it is worth the cost of adding a proper amount to the height of your chimney, then there is no objection to making the down-take or to using baffling plates.

No. 204.—14.

What is the best method for burning anthracite culm of poor quality.

DISCUSSION.

Mr. Durfee.—I presume the simplest method for burning anthracite culm is the adoption of a very large grate area and a very slow combustion at any particular point of that area. This simple method is carried out, I believe, with entire success in some locomotives of the Reading Railroad which have been in use eight or ten

years, and I believe they are changing all their old locomotives to this form. Some of the locomotives in use on the Reading Railroad have a grate area of 4 by 16 feet, and the combustion at any one point is very slow. If the combustion was rapid, there would be a formation of clinkers, and the draught would be impaired. I have used with success anthracite culm with quite a percentage of dirt in it, in the proportion of 25 per cent. of the whole fuel used (the balance of the fuel was bituminous coal) in gas producers in connection with a Siemens furnace. Anthracite culm alone can be used with success with a blast under the producers, but the producer capacity must be large, so that the combustion therein is quite slow, in order to avoid the formation of clinkers.

Mr. Kent.—I have been asked this same question several times in the past year or two, and my answer has generally been, "Go to Scranton and find out." And if there are any gentlemen here from Scranton perhaps they can tell us something about it. In Scranton, the Lackawanna Coal and Iron Company are using anthracite culm to a large extent, and there are two inventors in Scranton who have invented and patented shaking grates, the object of which is to cut away the clinker from the bottom of the grate, and I believe they are succeeding very handsomely.

Mr. Durfee speaks of the advisability of having a large grate surface and slow combustion. I think in Scranton they are using just the opposite. But the conditions in which they use rapid combustion, are that they have some means of cleaning away the clinker rapidly. The culm is generally burned with forced blast by steam jet, and my own opinion, which is subject to revision after studying the subject more carefully, is that the steam jet by itself should not be used, but that you should have an additional air blast as from a fan, for the reason that the steam jet carries in too much moisture. If you can carry in dry air enough to support combustion, and steam enough to cool the coal at the grate bars, then you will have the best conditions. You want cool grate bars, and the cooling can be produced by steam.

I would like to hear from some Scranton gentlemen on the subject.

Mr. George Schuhmann.—I would like to confirm what Mr. Durfee said about the locomotives on the Reading road; they have the so-called Wootten fire-box, which has a very large grate surface, about 70 square feet, and the combustion is slow. The draught has been lessened by enlarging the exhaust nozzle, which also reduces the

back pressure in the cylinders. I have nothing to do with these locomotives myself, but I see them pass every day and sometimes make inquiries about their working, and they seem to give general satisfaction. All the new locomotives which they are building have these large fire-boxes, which seems to indicate that the officials of the Reading road at least have satisfied themselves that large grate surface and comparatively slow combustion is the best method of burning anthracite culm. They burn what is known as buckwheat coal, which I believe is screened culm.

Mr. F. W. Dean.—I have had opportunities to observe culm burning at Scranton as referred to by Mr. Kent, and would say that the tendency is quite the opposite of that stated by him, and grate area is being increased very much. Recent boilers of the locomotive type have had as much as 64 square feet of grate area, the diameter of shell being some 60 inches. The results are stated to be to the satisfaction of the designer. In another instance boilers having a somewhat smaller grate, but still a very large one, are doing good service. Mr. Kent is correct in saying that the blast is of the steam jet type. Probably no culm-burning boiler in or near Scranton is without this. It generally blows air into the ash-pit, although in some cases jets are very successfully introduced into the chimney near the top.

Fire-boxes, 16 feet long by 4 feet wide, have not recently been built on the Philadelphia and Reading Railroad, the tendency now being toward boxes square in plan. Some recent very heavy passenger locomotives have grates 9′ 6″ long by 8′ 0″ wide, g ving 76 square feet of grate area.

After an extensive observation I cannot see that these locomotives should be said to burn culm in any proper sense. They burn clean coal of all small sizes from pea to stove size.

In regard to culm burning under stationary boilers, I have often heard doubts expressed of the efficacy of combustion chambers. I should think that possibly a judicious application of a mass of fire-brick might be beneficial to combustion.

No. 204.—15.

What is the best method for drilling holes in plate glass? Is there anything better for true holes than a copper tube and emery?

DISCUSSION.

Mr. Durfee.—Many years ago I had occasion to drill a number of holes through glass. The holes were $\frac{3}{16}$ of an inch in diameter and the plates were perhaps $\frac{1}{8}$ of an inch thick. The method adopted was that of using an ordinary bow drill with spirits of turpentine as a lubricant. The hole was drilled from one side until the point of the drill just punctured the opposite side of the glass. Then the glass was turned over and the hole finished by drilling from the opposite side. I do not remember that I cracked a single glass in that work. That drilling operation is a very good one for a hole of moderate size.

Mr. Oberlin Smith.—I used to drill holes in plate glass years ago, and I always had pretty good success with a very hard drill lubricated with turpentine, as Mr. Durfee has mentioned; but I did not turn over and drill through the other side, because the drill would be apt to run sideways somewhat. I used to put a piece of perfectly flat cast iron under the glass, with a little piece of paper between, clamping all firmly together, of course seeing that the iron fitted the glass at that place. By letting the drill puncture the iron a little it went clear through and did not break the bottom side of the glass, except the corner which would crack off a little—perhaps a sixty-fourth to a thirty-second of an inch. It was not more than that, I think, and some of the holes I worked were as large as half an inch in diameter in plates perhaps $\frac{1}{4}$ of an inch thick.

Mr. Ashworth.—Those who are not familiar with glass can scarcely realize how stubbornly it refuses to yield itself to the hands of the operator. The Hemingray Glass Company with which I was connected did considerable of this drilling. The drills used by that company were built up of lead reduced to a conical form, having no cutting edges whatever, saturated with turpentine and emery. There was no drilling from the other side. This produced a countersunk hole, and it was drilled out afterward with a straight rod and emery. A system of doing it efficiently was desired, and that system was the sand blast steam jet. By that means 100 holes could be drilled for one by the old method not only accurately but rapidly; a clear straight hole and with perfect success, without breaking. The breakage is very small in plate glass. When we enter into colored glass the fractures are very numerous. The subject of glass, I think, is one which will probably occupy our time in the

future to a great extent. As mechanical engineers, applying ourselves to the other branches and to work in metals, we are apt to form the idea that we can form, mould and drill glass with impunity; but such is not the case. Even the tubes which to the ordinary eye seem to have been bored and turned and polished have been only blown and drawn out, and though they result marvelously in their diameters so far as accuracy is attainable by that process, yet they are not accurate enough for the operations of a piston.

Mr. Durfee.—Mr. Campbell has just reminded me that in a certain place in Bridgeport glass is drilled in large quantities by means of a black diamond. The plates are perhaps $\frac{1}{4}$ of an inch thick, and the drill is lubricated with oil and spirits of turpentine, and when the hole is nearly through the glass is reversed. The plate to be drilled is held in a mechanical holder so that it can be reversed perfectly.

Mr. Kent.—Some time ago I had occasion to want some plate glass drilled and in considerable quantity, and I thought the best place to go to have it done was at the plate-glass maker's. I wrote to one who said he could do anything of the kind, and I got a price from him which was almost prohibitory. I was in somewhat of a quandary, when somebody told me of the sand-blast process. I got a price from a party using it which was low enough to allow of the order being given. I believe the plates are now being drilled by the sand-blast process, but I have not seen them yet and do not know what they will be like. But I am obliged to Mr. Ashworth for what he has told us.

Mr. Towne.—Undoubtedly the sand blast is the best method of cutting glass that we know of where the work is to be done in large quantities and can be sent out to be done.

It happened that in the Yale & Towne works this question came up some time ago, and noting this inquiry on our list I wrote to the gentleman who had the work in charge, and obtained the following data in regard to it, which may be of interest to persons who want to do this work in small quantities and in their own establishments instead of sending it away to be done. The holes which we had to drill are $\frac{7}{16}$ of an inch in diameter. The work has been done for more than ten years, and very satisfactorily and economically. The cost is a small fraction of what it was done for originally by some of the plate glass people. The best tool for the work has been found to be a brass tube $\frac{5}{100}$ of an inch thick. The

cutting agent is emery, number 5 II, and the lubricant simply water, which has been found as efficient as oil or turpentine and much less troublesome. The glass is ⅛ of an inch thick, and the workman is able to drill 30 to 40 holes per hour. The drill is run at 2,000 revolutions per minute, and the drilling of 40 holes through the ⅛-inch glass uses up about one inch of the tube. It is important to keep the emery well washed and cleaned, that is, with the dust removed from it which results from the abrasion of the glass. In our case, as the holes are required to be all in one position, the glass is put into a steel jig, and the drilling tube is run through a steel bushing. The hole produced is exceedingly smooth and true, and is carried straight through without the need of drilling back from the other side It seems to me that the hollow tube drill must be a better method than the solid drill, because the amount of glass cut is only that covered by the area of the annular edge of the drill, instead of the whole surface of the hole.

Mr. Oberlin Smith.—How much larger are the holes than the original diameter of the tube? I suppose some emery must get in alongside of the tube after it is partly through the glass, and there must be an appreciable difference? And are the holes tapering, being larger at the top where that loose emery runs along, than at the bottom?

Mr. Towne.—There is a slight difference between the diameter of the hole and that of the drill. The tapering effect I have not noticed. I doubt if it is apparent. The emery is kept packed around the drill during the process so as to cause as much as possible to work down through the hole.

Mr. Oberlin Smith.—Is there any of the emery put inside through the tube?

Mr. Towne.—No; merely packed around it.

Mr. Stetson.—I suggest that the difference in size of the hole would necessitate a difference of method. In corroboration of one of the statements made, I would say that I watched the drilling of a part of a sewing machine with a diamond drill, and it was really comfortable to hear the little "zip" that finished the work. It was so satisfactory that it made the contractor who discovered the process quite comfortably off in this world. The suggestion came to him of a diamond instead of a copper rod and emery. I cannot see what could be better than this diamond drill in small holes.

No. 204.—16.

Standard tapers for bolts in locomotive work;—also for dowels and pins.

DISCUSSION.

Mr. Bond.—The question of a standard taper for reamers for locomotive work includes an important class of work in which it is desirable to have uniformity. The taper used by railroad people in quite a number of instances is practically the same. The taper of $\frac{1}{16}$ of an inch to the foot is now used on quite a number of the roads, though in some cases, whether intentionally or not, the standard is slightly different. In some cases I have known it to be a taper of only $\frac{3}{64}$ of an inch to the foot. Others have $\frac{1}{8}$, and I know of one case in which the intention was to have the taper $\frac{1}{16}$ of an inch to the foot, but by some mistake the taper of the first set of gauges for reamers, instead of being $\frac{1}{16}$ to the foot, was $\frac{3}{32}$. I think that $\frac{1}{16}$ of an inch taper to the foot, so far as we know from practice, is a good angle of taper for bolts which are required to hold together frames for locomotives, or, in fact, any such work. Mr. Coleman Sellers, in an address delivered before the Master Mechanics' Association in Chicago, June, 1883, considered that the grinding of reamers, and the preservation of standard sizes in reamed holes for frame bolts, using gauges for the purpose, was made a much more simple operation by having a taper rather than a straight fit. The bolts are easily removed, and they certainly can be made to fit the entire length as well as if they were straight. In dowel pins the amount of taper per foot might be increased, and an angle represented by a taper of a quarter of an inch to the foot might be better for this purpose.

Mr. Towne.—I may mention, in connection with this question, that the subject to which this relates has been very fully discussed in the proceedings of the Master Car Builders, and a reference to their papers would shed a good deal of light on it.

No. 204.—17.

The effect of internal strain in hardened steel.

DISCUSSION.

Mr. Kent.—I do not know what is covered by this question, but I know of one instance of the effect of strain in hardened steel, which it might be well for members to know. One of the members of this Society, Mr. Gill, then of Pittsburgh, had occasion to make a knife edge for a testing machine. He took a piece of steel 10 inches long and 2 inches in diameter, formed it in proper shape, then cut it and tempered it, then ground it, and put it away on a shelf until he wanted to use it. In six weeks it broke in half, with a report like a pistol shot. That was the effect of strain in hardened steel. What caused it to wait six weeks before breaking I do not know.

Mr. Stetson.—It is not a phenomenal case to find a tool that shall have stood well for a number of days, broken. We frequently ship tools that are returned broken in halves, although they left our hands perfectly sound. I frequently find in the case of larger tools that there is an air bubble, or some want of the steel being entirely compact—some little defect of that kind, and quite generally, when there is a break, this defect is apparent. The hardest thing, perhaps, to harden is a large solid mass of steel. It is not a matter of much surprise to find a hardened and solid arbor, for instance, break the second day, and frequently, as I have suggested, this is due to some little defect in the steel. It seems as though there was a gas in there that has unlimited power. I have noticed, generally, some slight defect of that kind, and the effect of this internal strain in steel is to decrease our respect for steel somewhat. We think when we have got a hardened steel we have got something, as is familiarly said, that we can tie up to; but that is not a fact. You take a hardened and ground gauge and it is changing. It will be found in course of time that a change has taken place and that the gauges won't go together. Sometimes that leads to breaking that gauge and finding some defect in the steel, and in other ways the behavior of steel is singular. I used to laugh at the old idea of an old razor being laid away and growing better by the rest. I really believe that might be so, and I think there is a constant motion of steel. The magnetic influences might be something, but there are certainly strains that take place that are entirely unaccountable, and until we get to be finer in our methods of investigation, they must remain entirely beyond our reach.

Mr. Oberlin Smith.—I hardly agree with the gentleman that there is anything "electrical" about it. It seems to me natural

that when the external part of a piece of hard steel is put under a permanent tensile strain, it should give way some time under some accidental vibration. I have had occasion to harden a good many steel rings of the L shaped suction, shown in the cut, where one side was thick and heavy while the other was thin and light. I always found that I must dip them with the thick side, *b*, down, because then the cooling of the thick part, which takes place first, draws in the thin part, while it is still hot and ductile. In cooling afterward it is not apt to break. Dip the thin side, *a*, in first, and the thick part won't go in with it because it hasn't yet had time to shrink, and the tensile strain in the thin part is likely to break it. Although some of these do not break at first, they break at some unexpected time afterward, and the thin part drops off, apparently without special cause.

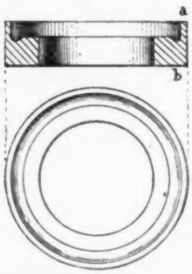


Fig. 121.

Mr. Durfee.—It may throw some light upon this subject if we consider the action of cooling and hardening upon the particles which compose the mass of steel. In cooling suddenly any mass of ferruginous material—and I do not know but that it is the fact in regard to all metals—the crystals arrange themselves perpendicularly to the cooling surfaces. In a bar of steel having a rectangular cross section, the crystals will arrange themselves on suddenly cooling the bar, as shown in Fig. 122,* and if a steel ingot is broken across, two well-defined diagonal lines (formed by the meeting of the crystals from each side) will be seen as is well represented in the engraving.

Fig. 122.

Now, in the case of hardening any piece of manufactured steel the same arrangement of crystals takes place. In the case where a bar of steel having a cross section, as shown in Fig. 123, is subjected to the hardening process, the crystals will arrange themselves perpendicular to the several cooling surfaces, and at the re-entrant angle, A, there would be an ugly strain occasioned by the interlocking of these crystals. It makes no difference what the cross section of the metal may be, the crystals will arrange themselves perpendicular to the cooling surfaces.

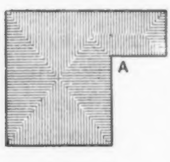


Fig. 123.

* See Vol. VI. Transactions A. S. M. E., p. 665.

Mr. Bond.—I might say in corroboration of what Mr. Stetson has just said, referring to what was mentioned in the discussion of Professor Sweet's paper on "The Unexpected which often Happens,"* that this internal strain, being relieved under rupture, manifested itself by changing the size of the parts not disturbed by the sudden giving way, causing an increase of their diameter. It may be said that under this condition of strain a hardened gauge which does not break in course of time, would change in size and become smaller, because the inner part becoming gradually incapable of resisting the strain from hardening, would finally accommodate itself to the new condition of things. Now this need not affect the interchangeability of these gauges, for while there is a change in the diameter after hardening, which has been shown to be the case, still I think under certain treatment in hardening this change can be overcome. I know we find it so, and have had no trouble during the last two years in preventing it, whereas before, in two or three weeks' time, hardened rings would change perceptibly in size. This permanency of size could be brought about, I think, by not allowing the hardening to take place entirely throughout the mass, retaining the center soft. Now, in hardening any mass of steel—a Sellers hob, for example—4 inches in diameter, and which would probably be about 18 inches long, we had at first considerable trouble in getting the steel to harden without cracking, but we found that by simply drilling a $\frac{3}{4}$-inch hole centrally through the entire length this tendency to break was obviated. It seems to give a chance for the strains to adjust themselves around some central cavity, and I do not know of but one case in which hardened work has broken where this "safety" hole has been put through, and in this instance the hole was not drilled centrally the entire length.

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* Page 158. Vol. VII. Transactions A. S. M. E.

PAPERS

OF THE

CHICAGO MEETING

(XIIIth),

MAY, 1886.

CCV.

PROCEEDINGS

OF THE

CHICAGO MEETING

(XIIIth)

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

May 25th to 28th, 1886.

COMMITTEE OF ARRANGEMENT:—N. C. BASSETT, M. C. BULLOCK, W. F. DONOVAN, C. F. ELMES, W. D. EWART, R. FORSYTH, W. FORSYTH, C. C. HILL, J. S. LANE, A. F. NAGLE, G. E. PALMER, J. A. ROCHE, A. SINCLAIR, H. S. SMITH, H. B. STONE, J. N. WARRINGTON, H. WEBSTER.

The XIIIth meeting of the American Society of Mechanical Engineers was held in the city of Chicago, beginning Tuesday, May 25th. The head-quarters of the Society were in Rooms 1 and 3 of the Grand Pacific Hotel.

The opening session was held in the club-room, No 4, in the Grand Pacific Hotel. The session was called to order by Mr. J. A. Roche of the Local Commitee, who delivered a brief address of welcome to the Society. Mr. Henry R. Towne of Stamford, Conn., Vice-President of the Society, responded to the address in the absence of the President, Mr. Coleman Sellers of Philadelphia. In the course of the meeting, the following letter from the latter was read to the members:

3301 BARING STREET.

PHILADELPHIA, *May* 24, 1886.

Mr. President, and Members of the American Society of Mechanical Engineers.

GENTLEMEN:

It is a bitter disappointment to me to find myself unable to attend the meeting in Chicago, and to perform my duties as presiding officer of a Society in which I

feel so deep an interest. A long and tedious illness has confined me to the house since the beginning of last December, and for many weeks to my bed; as yet I am unable to bear the fatigue of a long journey. I have asked the Secretary to explain my enforced absence from this meeting; but I feel called upon to say a few words more directly to you, and, in this lame way, to thank you for the honor you have conferred on me in selecting me as your presiding officer. I have for a long time been hoping against hope, that I could meet you in person and come to know those members with whom I have not had the pleasure of meeting, as well as of greeting once more many who have been endeared to me through long years of pleasant intercourse, and the attraction of kindred interests.

It is fortunate that the active conduct of societies such as this, rests more with the Secretary than with the President. The former, if wisely selected, acting from year to year, identifies himself with the Society and learns its wants and the best means of promoting its advancement. It gives me pleasure to feel that the present incumbent of the office of secretary is so thoroughly competent.

The part played in the progress of the world's industries by the interests represented in this Society, may be well illustrated in the operation of a kindred society in another country. One of the oldest associations for the consideration of engineering subjects is the Institution of Civil Engineers, 25 Great George Street, Westminster, London. The name of this society suggests a special interest in a single branch of engineering, generally considered distinct from that of the mechanical engineer, but an examination into the proceedings of this older and highly honored society will show that more than three-fourths of the matters brought prominently before it, belong solely to the province of the mechanical engineer, who is the engineer in the broadest acceptation of the term. The lectures delivered in the winter course in Great George Street are mostly on subjects connected with dynamical engineering, and the great mechanical engineers of England have been in turn the honored Presidents of the Society.

As we look back over the few years that span the life history of engineering, as a distinct profession, we see the names of great mechanical engineers loom up as the pioneers in all those great movements which have rendered modern civilization possible. The education of the engineer has already engaged your attention, and I have much that I would like to say to you on that subject, as this Society must play a very considerable part in the education of the future; but my present condition compels me to put off until a later day this task. I hope in my annual address next autumn, to review this subject, and also, without stepping outside of matter germane to this Society, to touch on the subject of the employer and the employee, in mechanical industries, and in general to review those points which bear on the progress of mechanic arts, and to suggest methods by which the mechanic arts may be developed with the least loss in unnecessary experiments—to point out the work which can be done by our Society in educating the community to a just appreciation of the value of competent engineering assistance in carrying out important enterprises.

Carefully conducted original investigations into all subjects connected with mechanics, should be encouraged by us and find a place of record in the proceedings of this Society; and the value of such papers should be attested by well considered awards to their writers, not awards of high money value, but made of greater moment by their well placed disposal.

In conclusion, to you Mr. Vice-President, I tender my thanks for your performance of my duties, and at the same time desire to express my satisfaction in having a substitute so thoroughly able and well fitted for the task. Earnestly pray-

ing for the continued prosperity of our Society and hoping in future to be able to do my full share in its active work, I am,

Very truly and respectfully,

COLEMAN SELLERS,

President.

Mr. Towne acted as President throughout the sessions.

The professional business of the evening was the discussion of Topical Queries. Messrs. Sinclair and Heminway discussed the question: "Are there any grave objections to cam motions for moving the valves of high-speed engines: what is a limiting speed for cams?"

Messrs. Lewis, Sinclair and Oberlin Smith took part in answer to the query: What is the maximum safe load for steel tires on steel rails: can this be expressed in terms of the crushing strength of the tire and rail, and per inch of width for different diameters of tire?

The next question was as to the present status in Chicago of smoke-preventing furnaces under steam boilers. Messrs. Palmer, Cole, Babcock, Durfee, Carpenter, Hawkins, Underwood, King, Harding, Minot and Walker spoke upon it.

The last query was: "How do you make successful foundations for structures upon yielding earth?" Mr. W. L. B. Jenney, one of the leading architects of Chicago, was present as invited guest of the Society, and spoke in detail of the special problems which are met in securing foundations in that city. Messrs. Palmer, Oberlin Smith, Durfee and Cole continued the discussion.

The Chair, under the rules, announced the Committee of the Society to nominate officers for the ensuing year, consisting of Messrs.

J. F. Holloway,	Cleveland, O.
J. M. Dodge,	Philadelphia, Pa.
J. E. Sweet,	Syracuse, N. Y.
W. E. Parker,	Lawrence, Mass.
W. F. Donovan,	Chicago, Ill.

The Society then adjourned to a supper tendered by the Local Committee, and a pleasant social reunion was enjoyed to a late hour.

SECOND DAY, MAY 26TH.

The second session was called to order by the Chair at ten o'clock A.M., in the large hall in the Methodist Church Block, cor-

ner of Clark and Washington streets. The Secretary's Registers showed the following members in attendance:

Allen, James M	Hartford, Conn.
Anderson, John W	South Bend, Ind.
Arnold, Bishop	Auburn, N. Y.
Babcock, George H	New York City.
Barnaby, Charles W	Salem, O.
Barnes, W. F	Rockford, Ill.
Barrus, George H	Boston, Mass.
Bassett, N. C	Chicago, Ill.
Bauer, Charles A	Springfield, O.
Bennett, F. M	Chicago, Ill.
Binsse, H. L	New York City.
Bond, George M	Hartford, Conn.
Borden, Thomas J	Fall River, Mass.
Briggs, John G	Terre Haute, Ind.
Brown, C. H	Fitchburg, Mass.
Bullock, M. C	Chicago, Ill.
Bushnell, R. W	Cedar Rapids, Ia.
Carpenter, R. C	Lansing, Mich.
Cartwright, Robert	Stamford, Conn.
Clements, W. L	Bay City, Mich.
Cobb, E. S	Terre Haute, Ind.
Cole, J. W	Columbus, O.
Collins, C. M	South Bend, Ind.
Colwell, A. W	New York City.
Crane, T. S	Newark, N. J.
Dingee, W. W	Racine, Wis.
Doane, W. H	Cincinnati, O.
Dodge, J. M	Philadelphia, Pa.
Drummond, W. W	Louisville, Ky.
Durfee, W. F	Bridgeport, Conn.
Elmes, C. F	Chicago, Ill.
Ewart, W. D	Chicago, Ill.
Fawcett, Ezra	Alliance, O.
Fingal, Charles	Chicago, Ill.
Forsyth, William	Aurora, Ill.
Foster, C. H	Chicago, Ill.
Fowler, John	Louisville, Ky.
Fraser, D. R	Chicago, Ill.
Fraser, N. D	Chicago, Ill.
Galloupe, F. E	Boston, Mass.
Giddings, C. M	Massillon, O.
Gobeille, J. L	Cleveland, O.
Goss, W. F. M	Lafayette, Ind.
Hamilton, Homer	Youngstown, O.
Hammer, A. E	Branford, Conn.
Hand, S. A	Toughkenamon, Pa.
Hawkins, J. T	Taunton, Mass.
Heminway, F. F	New York City.

Higgins, S. .... Buffalo, N. Y.
Hill, C. C. .... Chicago, Ill.
Hollingsworth, S. .... Boston, Mass.
Holloway, J. F. .... Cleveland, O.
Howard, C. P. .... Hartford, Conn.
Hutton, F. R., *Secretary* .... New York City.
Ide, A. L. .... Springfield, Ill.
Jenkins, John .... Milton, Pa.
Jenkins, W. R. .... Bellefonte, Pa.
Jones, R. R. .... Pittsburgh, Pa.
Kempsmith, F. .... Cleveland, O.
Kent, William .... New York City.
Kimball, H. .... Cleveland, O.
King, C. I. .... Madison, Wis.
Kirkevaag, P. .... Youngstown, O.
Lane, J. S. .... Chicago, Ill.
Lewis, W. .... Philadelphia, Pa.
Lipe, Charles E. .... Syracuse, N. Y.
Mackinney, W. C. .... Philadelphia, Pa.
Magruder, W. T. .... Taunton, Mass.
Metcalfe, H. .... Troy, N. Y.
Miller, W. .... Cleveland, O.
Minot, H. P. .... Columbus, O.
Mohr, L. .... Chicago, Ill.
Morava, W. .... Chicago, Ill.
Morgan, T. R., Sr. .... Alliance, O.
Murray, S. W. .... Milton, Pa.
Nagle, A. F. .... Chicago, Ill.
Palmer, G. E. .... Chicago, Ill.
Parker, W. E. .... Laurence, Mass.
Parks, E. H. .... Providence, R. I.
Pitkin, J. H. .... Canton, O.
Pond, F. H. .... St. Louis, Mo.
Prindle, E. T. .... Aurora, Ill.
Robinson, A. W. .... St. Catharine, Ont.
Robinson, J. M. .... New York City.
Robinson, S. W. .... Columbus, O.
Roche, J. A. .... Chicago, Ill.
Rumely, W. N. .... Laporte, Ind.
Scheffler, F. A. .... Erie, Pa.
Schuhmann, George .... Reading, Pa.
See, H. .... Philadelphia, Pa.
Sellers, M. .... Chicago, Ill.
Sharpe, J. .... Salem, O.
Sinclair, A. .... Chicago, Ill.
Smith, G. H. .... Providence, R. I.
Smith, H. S. .... Joliet, Ill.
Smith, J. M. .... Detroit, Mich.
Smith, Oberlin .... Bridgeton, N. J.
Snell, H. I. .... Philadelphia, Pa.
Sprague, W. W. .... Lake, Ill.

Stahl, A. W	Lafayette, Ind.
Stone, H. B	Chicago, Ill.
Sweeney, J. M	Wheeling, W. Va.
Sweet, J. E	Syracuse, N. Y.
Tallman, F. G	Beaver Falls, Pa.
Taylor, F. W	Philadelphia, Pa.
Thompson, E. B	Chicago, Ill.
Towne, H. R., *Acting President*	Stamford, Conn.
Uehling, E. A	Sharpsville, Pa.
Underwood, F. H	Tolland, Conn.
Walker, John	Cleveland, O.
Wallis, P	Aurora, Ill.
Warren, B. H	Boston, Mass.
Warrington, J. N	Chicago, Ill.
Webster, H	Chicago, Ill.
West, F. D	Cleveland, O.
Whitney, B. D	Winchendon, Mass.
Whitney, W. M	Winchendon, Mass.
Wiley, W. H	New York City.
Wood, W	Philadelphia, Pa.
Woods, A. T	Champaign, Ill.
Woodward, C. M	St. Louis, Mo.

The following report from the Council was presented and ordered upon the records:

REPORT FROM THE COUNCIL.

The Council would respectfully report to the Society the deaths of

Messrs.	Wm. Cleveland Hicks,	member,	of New York.
	Frederic E. Butterfield,	"	of Seneca Falls, N. Y.
	David S. Hines,	"	of New York.
	Emile Fr. Loiseau,	"	of Belgium.

They would further report that a Committee of the Council was appointed to prepare a plan for the regulation and encouragement of discussion upon the papers presented to the Society, who offered the following report, which was adopted.

RULES FOR DEBATE.

The Committee appointed by the Council to prepare and suggest a plan for regulating the presentation of papers and the discussion thereon at the meetings of the Society, so as to promote the orderly conduct of business and to apportion the time of the meetings equitably among the members presenting papers or participating in their debate, beg leave, respectfully, to submit the following plan:

I. Papers to be presented at any meeting must be placed in the Secretary's hands not less than nine (9) weeks in advance of the date of the meeting, in order that all such papers may be printed and ready for distribution not later than three weeks before the date of the meeting. Papers not received prior to the time named will not be entitled to presentation at that meeting.

II. Preliminary notice of date and place of the meeting of the Society shall be issued to the membership by the Secretary at least eight (8) weeks in advance of each meeting, and there shall be inclosed with such notice a blank form by which the members may signify to the Secretary their intention of attending the meeting.

III. Copies of all papers to be presented at each meeting shall be sent by the Secretary three weeks in advance of each meeting to every member who has previously signified his intention to attend, accompanied by a blank by which a member intending to discuss any of the papers to be presented may give notice of such intention by properly filling in and returning the blank to the Secretary; priority in debate on each paper shall be given to the members so signifying their intentions, in the order in which such notifications are received.

IV. At each meeting, papers entitled to be presented shall be read by abstract only (unless so short as to permit of their being read in the time limit), and preferably by the Secretary; not more than five (5) minutes to be occupied in the presentation of any paper.

V. Members who have given previous notice of their intention to discuss any paper shall be entitled to priority in its discussion as above provided for; a member who has reduced his remarks to writing may occupy not exceeding ten (10) minutes for its presentation (either in full or in abstract); extemporaneous discussion by any member, shall be limited to a time not exceeding five (5) minutes at one time.

VI. Any member having once had the privilege of the floor in debate on any paper shall not be again entitled to it until all other members desiring to speak in the discussion of each paper shall have had the opportunity to do so.

VII. Members unable to attend a meeting may request copies of any papers to be presented thereat, and may thereupon forward to the Secretary their written discussion of such papers. Written discussions so received by the Secretary, not less than three days prior to the date of the meeting, will be entitled to be presented in the same manner as oral remarks (in full or in abstract) by the Secretary.

All such discussions shall be treated as though spoken at the meeting and shall appear accordingly in the records.

VIII. The author of each paper presented at a meeting shall be entitled to a time not exceeding five (5) minutes in which to close the discussion thereon, but, if he prefers, may waive this right and submit his closing remarks in writing after the meeting, provided that all such additions be forwarded to the Secretary within not exceeding four (4) weeks after the close of the meeting.

IX. The Publication Committee shall carefully examine the papers to be presented at each meeting of the Society, and shall apportion among them the time available in the sessions for papers and discussions, allotting time to each in such proportion as they deem just. At the expiration of the time so allotted to it, the discussion on every paper shall cease, and the paper next in order shall be taken up. Should any time remain at the end of that session and after the presentation of all papers allotted to it, discussion may be resumed upon any paper previously presented, and in the order in which they were placed on the docket.

X. (Added by the Council on presentation of the report.) These rules may be suspended at any session by the Council (such suspension to be announced at the beginning of the discussions) or during the meeting by unanimous consent.

The Committee commenting on the above would call attention to the fact, that the purpose aimed at throughout is to provide for the most economical use of the time at each session, and also for the equitable distribution of that time in the presentation and discussion of the several papers. Although at first glance and by reason of unfamiliarity the rules may seem somewhat arbitrary, a careful consideration of them will, it is believed, demonstrate their desirability as a means for increasing the amount and improving the quality of the work done at each meeting, and their fairness in providing for the distribution of time among the various authors and speakers.

Respectfully submitted,

HENRY R. TOWNE, } *Committee.*
F. R. HUTTON,

A committee was appointed to take steps by which the members of the Society might be benefited by the scientific and technical reports which are issued by the United States Government. This committee consists of Messrs. Babcock, Oberlin Smith, and Wm. Hewitt.

The Council have directed that under the Rules, Art. 10–13, it is not necessary to wait until the designated thirty days in advance of a meeting before submitting to the voting membership the names of candidates seeking election at that time. They have therefore directed that more than one ballot list may be issued, the several lists to be distinguished by different colors, and that one shall be issued as soon as a sufficient number of applicants to make it desirable shall have been approved.

The Council would further present the following report of its tellers:

May 19, 1886.

The undersigned were appointed a Committee of the Council to act as tellers under Rule 13 to count and scrutinize the ballots cast for and against the candidates proposed for membership in the Society of Mechanical Engineers, and seeking election before the XIIIth meeting of the Society in May, 1886. They would report that they have met upon the designated days in the office of the Society and proceeded to the discharge of their duties. They would certify for formal insertion in the records of the Society to the election of the appended named persons to their respective grades, upon the lists Number one and Number two, colored respectively pink and yellow.

There were 314 votes cast in the ballot upon the pink list, of which 8 were thrown out because of informalities, and there were 300 votes cast using the yellow ballot, of which 5 were thrown out because of informalities.

WM. KENT,
WM. H. WILEY.
Tellers.

HONORARY MEMBERS.

Baker, Benjamin. — Dredge, James
Prof. V. Dwelshauvers-dery — Walker, Francis A.

MEMBERS.

Arnold, Bishop..........Auburn, N. Y.
Armington, Pardon..........Providence, R. I.
Bacon, Earle C..........New York City.
Barnes, W. F..........Rockford, Ill.
Bartol, George..........Cleveland, O.
Beck, Matt. A..........Milwaukee, Wis.
Bennett, Frank M..........Chicago, Ill.
Bleloch, George H..........Springfield, Mass.
Borden, Thomas J..........Fall River, Mass.
Brown, Jas. A..........Philadelphia, Pa.

Butterworth, James.............................Philadelphia, Pa.
Cavanagh, Joseph.............................Philadelphia, Pa.
Clarke, Alfred.............................Cleveland, O.
Clements, Wm. L.............................Bay City, Mich.
Cobb, Edward S.............................Terre Haute, Ind.
Collier, R. B.............................Columbus, O.
Collins, Chas. M.............................South Bend, Ind.
Crane, Thomas S.............................Newark, N. J.
Daniels, Fred H.............................Worcester, Mass.
Dingee, W. W.............................Racine, Wis.
Downe, Henry S.............................Fitchburg, Mass.
Falkenau, Arthur.............................Scranton, Pa.
Fingal, Chas. A.............................Chicago, Ill.
Fitt, James.............................Rochester, N. Y.
Forbes, W. D.............................Bridgeport, Conn.
Foster, C. H.............................Chicago Ill.
Fowler, George L.............................New York City.
Francis, James.............................Lowell, Mass.
Fraser, David R.............................Chicago, Ill.
Fraser, Norman D.............................Chicago, Ill.
Gobeille, Joseph Leon.............................Cleveland, O.
Goss, W. F. M.............................Lafayette, Ind.
Gowing, E. H.............................Boston, Mass.
Hamilton, Alex., Jr.............................Johnstown, Pa.
Holland, John.............................Dover, N. H.
Howe, Henry M.............................Boston, Mass.
Jones, William H.............................Cincinnati, O.
Kempsmith, Frank.............................Cleveland, O.
Kerr, Walter C.............................New York City.
Kimball, Hiram.............................Cleveland, O.
Kirkevaag, Peter.............................Youngstown, O.
Lane, Harry M.............................Cincinnati, O.
Lieb, John W., Jr.............................Milan, Italy.
Mason, J. A.............................Chicago, Ill.
Metcalfe, Henry.............................Troy, N. Y.
Middleton, Harvey.............................Louisville Ky.
Miller, Walter.............................Cleveland, O.
Minot, H. P.............................Columbus, O.
Mohr, Louis.............................Chicago, Ill.
Monaghan, Wm. F.............................New York City.
Moore, John M.............................Newton, Mass.
Moore E. L.............................Boston, Mass.
Morava, W.............................Chicago, Ill.
Morrison, W. A.............................Lowell, Mass.
Mullen, John.............................Shamokin, Pa.
Parker, Charles D.............................Worcester, Mass.
Pease, Charles S.............................New York City.
Post, John, Jr.............................Boston Mass.
Prentice, Leon H.............................Chicago, Ill.
Prindle, Edward T.............................Aurora, Ill.
Rood, Vernon H.............................Jeanesville, Pa.

Sargent, John W ....Scranton, Pa.
Saunders, Wm. L.......................................New York City.
Sellers, Morris..Chicago, Ill.
Sims, Gardiner C.....................................Providence, R. I.
Smith, T. Carpenter ....Philadelphia, Pa.
Taylor, Stevenson......................................New York City.
Taylor, Fred. W.......................................Nicetown, Pa.
Thompson, E. B.......................................Chicago, Ill.
Wallis, Philip ....Aurora, Ill.
Warren, John E.......................................Cumberland Mills, Me.
Watson, William......................................Boston, Mass.
Whitehill, Robert.....................................Newburgh, N. Y.
Whitney, Baxter D...................................Winchendon, Mass.
Whittier, Charles.....................................Boston Mass.
Williams, J. Newton..................................Brooklyn, N. Y.
Winter, Herman.......................................New York City.
Woodward, Calvin M.................................St. Louis, Mo.
Woolson, Orosco C...................................Stamford, Conn.
Worswick, Thomas....................................Guelph, Ont.
Wyman, Horace W...................................Worcester Mass.

ASSOCIATES.

Clark, Frank M..Boston Mass.
Low, Fred. R ....Boston Mass.
Russell C. M ....Massillon, O.
Wainwright, C. D......................................Boston, Mass.
Whitney, Wm. M.......................................Winchendon, Mass.

JUNIORS.

Higgins, George F.....................................Manchester, N. H.
Moran, Daniel F ....Brooklyn, N. Y.
Stevens, Wm. N..Brooklyn.
Stone, Wilbur M..Hartford, Conn.

Respectfully submitted by the Council.

Mr. Oberlin Smith presented a brief report as follows, on behalf of the Committee on the Appointment of a United States Test Commission.

Mr. Oberlin Smith.—With regard to the Test Commission Committee, of which I am a member, I would say that we have no written report. I have not seen the chairman of the committee, Prof. Egleston, lately, although I corresponded with him about the matter not long since. We have sent out a good many circulars, and have written to quite a number of members of Congress, and to business men throughout the country, urging the latter to write to their representatives in Congress upon this subject. The bill will come up during the present session and is likely to be reached. If it is reached, there is little doubt about its passing.

The great difficulty will be that it may be crowded out by press of other business.

Mr. Towne reported on behalf of the Committee on Uniformity in Test Specimens and Methods of Test as follows:

Mr. H. R. Towne.—I happen to be a member of this Committee, although not its chairman, and in Prof. Egleston's absence I would report informally that some progress has been made in the preparation of test specimens, test blanks for reports, and in arrangements with makers and users of testing machines for making a series of tests of the specimens provided. I think that the work which the Committee has projected will be of great interest and great value. It is a somewhat novel line of investigation, and will bring out, I believe, a great deal that will be useful. It will, however, occupy, necessarily, a good deal of time to complete, and probably no full report can be looked for earlier than the next meeting, and perhaps not until the meeting following.

Mr. Geo. M. Bond, Secretary of the Committee on Uniform Standards in Pipe and Pipe Threads, reported on behalf of the Committee as follows:

Mr. Bond.—In the absence of our Chairman, Mr. Frederick Grinnell, I can say that we have been in communication with the wrought iron pipe manufacturers individually, also with nearly all the fittings manufacturers, both brass and iron, and have received many favorable replies to the individual letters so addressed. When in New York last week I was informed by Mr. J. H. Flagler, general manager of the National Tube Works Company, that a Committee was appointed at the meeting of the Pipe Manufacturers' Association, held at Philadelphia, on the 12th inst., to confer with our Committee.

A communication has just been received from Mr. Jas. H. Murdock, Secretary of the *Manufacturers of Wrought Iron Pipe and Boiler Tubes in the United States*, stating that at a meeting of their Association held at the Continental Hotel, Philadelphia, May 12, 1886, a Committee to confer with our Committee was appointed, the members of which Committee are

Mr. L. W. Shallcross, chairman, representing *Morris, Tasker & Co., Limited.*

Mr. J. H. Flagler, representing *The National Tube Works Co.*

Mr. L. J. Piers, representing *The Allison Manufacturing Co.*, and

Mr. Jas. H. Murdock, Secretary to the Committee.

A communication was also received from Mr. S. L. Morrison, secretary of the Manufacturers' Association of Brass and Iron,

Steam, Gas and Water Works of the United States, stating that at a meeting of this Association held at Pittsburgh on the 11th, 12th, and 13th inst., the following resolution was unanimously passed:

"*Resolved*, That this Association favors the establishment of a universal wrought iron pipe gauge, to be used as a standard throughout the United States, and that any action taken by the manufacturers of wrought iron pipe to accomplish this object shall have our hearty co-operation."

There was also received from Mr. W. H. Douglas, Corresponding Secretary Cast Iron Fittings Association, a letter advising our Committee of the following action taken by them:

"At a meeting of the Cast Iron Fittings Association, held in New York, May 19, the following resolution was unanimously adopted:

"'*Resolved*, That a committee of five (5) be appointed to take into consideration the matter of a standard gauge or thread.'

"The following gentlemen were named as such committee:

"Mr. R. T. Crane, Pres't Crane Bros. Mfg. Co., Chicago, Ill.

"Mr. C. C. Walworth, Pres't Walworth Mfg. Co., Boston, Mass.

"Mr. E. G. Burnham, Vice-Pres't The Eaton, Cole & Burnham Co., Bridgeport, Conn.

"Mr. Charles Jarecki, Pres't Jarecki Mfg. Co., Erie, Pa.

"Mr. Carleton W. Nason, Pres't Nason Mfg. Co., New York City."

The latter Committee will confer with the Committee of Pipe Manufacturers as to the best means of bringing about the desired result.

A conference with the Committee of Pipe Manufacturers is expected soon by our Committee, and everything seems now ready for final, definite action.

Our Committee would ask for further time, as we hope to have something even more definite to report at the next meeting.

The Chairman.—It is understood, of course, that the Committee is continued, in view of the promising condition of its work.

Letters of invitation were read from Messrs. H. H, Belfield, Director of the Chicago Manual Training School, and L. P. Morehouse, inviting the Society to visit the school and to join an excursion to Pullman, respectively. The engagements of the Society prevented the acceptance of these invitations, and the Secretary was directed to send a note of thanks to these gentlemen for their courtesy.

The first paper presented was by Mr. Wilfred Lewis, of Philadelphia, entitled "Experiments on the Transmission of Power by Belting." The chairman introduced the reading of the papers, by the following remarks:

The Chairman.—Before commencing the reading of papers, I wish to ask the indulgence of the members in regard to the new rules in the matter of their presentation and discussion referred to in the report of the Council. They are new and different from anything we have ever attempted before, and at first may cause a little friction, like other new machinery; but I believe that in the end it will be found that they will do a great deal to improve the *quality* of our work as well as the *amount* of it which can be got through, and in addition to that, they will certainly provide, if properly carried out, for a fair distribution of our time among the papers presented. In one case which I recall, decidedly the best paper which was entered at the meeting was left until so late and the time left was so short that the paper had to be read in a very hurried manner, and no debate whatever followed. Under the new rules each paper will have its fair share of time whether first or last on the list, and, in addition to that, the time available for debate will be so much greater than heretofore that the total time devoted to each paper will be much larger and should be made more valuable. It will soon be apparent whether the rules work satisfactorily, or whether they need to be modified or improved.

Mr Lewis's paper received discussion by Messrs. Hawkins, Oberlin Smith, Towne, Underwood, Walker, Taylor, Doane, Babcock, Morgan, and Kent.

Mr. Wm. O. Webber's paper, entitled "Relative Efficiency of Centrifugal and Reciprocating Pumps," was discussed by Mr. Borden. Mr. See's paper, "Production of True Crank Shafts and Bearings," was discussed by Messrs. Kent, Taylor, Cole, Holloway, Walker, See, Babcock, Sweet, Minot, Nagle, Schuhmann, and Oberlin Smith. Mr. Babcock's paper on "Substitutes for Steam" was discussed by Messrs. Crane, Schuhmann, Durfee, Kent, and Walker.

The Society took a recess until the afternoon at this point.

Afternoon Session, Wednesday, May 26th.

The session was called to order at 2.30 P.M. The first paper was by Chas. W. Barnaby, of Salem, O., entitled a "New Steam Engine Indicator." This was discussed by Messrs. Porter and Walker. Following this the set of three papers on the topic of shop management and shop account were presented and discussed together. These papers were "The Engineer as an Economist," by H. R.

Towne of Stamford; "The Shop Order System of Accounts," by Henry Metcalfe of Troy, and "Inventory Valuation of Machinery Plant," by Oberlin Smith, of Bridgeton. These were discussed by Messrs. Partridge, Fitch, Anderson, Hand, Taylor, Durfee, Oberlin Smith, Metcalfe, Hawkins.

At the close of the debate, in view of the general interest of the topics of this group, it was moved that the preparation and reading of papers on these and cognate subjects be encouraged for discussion in the general sessions of the Society.

It was then resolved that the session previously ordered for Friday evening be held on Thursday evening.

The paper of Thomas D. West of Cleveland was entitled: "Irregularities in Contraction of Duplicate Castings," and was discussed by Professor Sweet; and that of Mr. Frederick G. Coggin, was called, "A Novel Chimney Staging." The latter was discussed by Messrs. Borden and Durfee. Mr. Thomas S. Crane's paper on "Water Purification for Manufacturing and Domestic Purposes," was discussed by Messrs. Kent and Durfee. The paper by Mr. Fred. W. Taylor on the value of water-gas and gas from Siemen's Producers for melting in open hearth furnaces was discussed by Messrs. Kent and Schuhmann.

A special committee had been appointed to acknowledge the letter of the absent President of the Society, Mr Coleman Sellers. That committee presented its report in the form of a telegram, as follows:

MR. COLEMAN SELLERS, PRESIDENT:

The American Society of Mechanical Engineers now in session sincerely regret that illness prevents you from being with us on this occasion. We tender you our warmest sympathy and unite in wishing you an early and complete recovery.

W. H. DOANE,
J. F. HOLLOWAY,
F. R. HUTTON, } *Committee.*

The receipt of this telegram was acknowledged by letter from the President, but too late to be presented to the meeting.

The paper by Mr. John H. Cooper, of Philadelphia, Pa., was read by the Secretary, entitled: "Grain Handling in California," and discussion by Mr. Hugo had been sent in in manuscript.

The session then adjourned.

In the evening a banquet was tendered to the Society by the Local Committee in the dining-room of the Grand Pacific Hotel.

Over three hundred persons were in attendance, and the Schubert Quartette Club, of Chicago, rendered several selections during the evening.

Third Day, Thursday, May 27th.

The day was devoted to an excursion. A special train was tendered by the C. B. & Q. R.R., through the courtesy of Mr. Henry B. Stone, general manager, and member of the Society. It took the party first to the stock-yards, where a visit was made to the establishment of Armour & Co.; thence the train was taken and a visit of two hours was paid to the City of Pullman, Ill., lunch being served on the train. Returning, the train stopped at the North Chicago Rolling Mill, at South Chicago, and a visit was paid to the docks, blast furnaces, converting house and rail mill.

In the evening an extra session was held for special discussion of Topical Queries. Messrs. Walker, Jesse Smith, Durfee, Babcock and Hawkins discussed the question, "Would the use of an annular jet in an ejecter induce a greater current of the fluid to be moved than if a solid jet was used of the same area?"

Messrs. Hawkins, West, King, Walker, Babcock and Magruder discussed the question: "What are the best conditions covering the molding, mixing, melting and casting of iron for the successful production of molds for printing-press ink rolls?"

The next query was as to the effect of mechanical circulation in steam boilers as a means of preventing scale. Messrs. Kent and Durfee spoke to it.

The discussion on the query, "What is the best device to catch the water of condensation in the exhaust pipe of a high-pressure steam engine; and would any economy result from entrapping the water and pumping it back to boiler?" was by Messrs. King, Scheffler, Giddings, Kent, Durfee, Babcock, and Schuhmann.

Messrs. Snell, Towne and Robinson gave facts as to the power required to drive a blower.

After a brief reference to the date of the autumn meeting of the Society, the session adjourned.

Fourth Day, Friday May 28th.

The session was called to order at ten o'clock. The paper by W. P. Trowbridge, "On the Relative Economy of Ventilation by Heated Chimneys and by Fans," was discussed by Mr. Babcock.

The paper of C. M. Giddings, of Massillon, O., was entitled: "Description of a Valve Dynamometer for measuring the power required to move a slide valve at different speeds and pressures." It was discussed by Messrs. Porter, Babcock, Schuhmann, Kent, Lewis, Holloway, Jesse Smith, Barnaby, Hawkins, Sweet, and Towne.

The paper by Prof. C. M. Woodward on "The Training of a Dynamic Engineer at Washington University, St. Louis, Mo.," was discussed by Messrs. Thurston, Kent, Hawkins, Oberlin Smith, Taylor, Sinclair, Dodge, Robinson, Durfee, Webster, and King.

At the close of the discussion the following resolutions were presented and passed with acclamation:

Whereas, The visiting members of the American Society of Mechanical Engineers desire to express their appreciation of the kind and unremitting attention extended to them by our committee of arrangements, therefore

Resolved, That we hereby tender our sincere thanks to Messrs. Wm. F. Donovan, J. A. Roche, Geo. E. Palmer, Chas. F. Elmes, M. C. Bullock, N. C Bassett, W. D. Ewart, Robert Forsyth, William Forsyth, C. C. Hill, J. S. Lane, A. F. Nagle, Angus Sinclair, H. S. Smith, H. B. Stone, Jas. N. Warrington, and Hosea Webster, whose untiring efforts and generous hospitality have made this our XIIIth semi-annual meeting one of the most enjoyable meetings ever held by this Society.

Resolved, That the thanks of the American Society of Mechanical Engineers be tendered to the citizens of Chicago for the courtesy and attention extended to our Society during this its XIIIth meeting, held in the City of Chicago, May, 1886.

We would especially thank the Chicago, Burlington & Quincy R.R., and its general manager, Mr. H. B. Stone, Mr. M. Cudahy for Armour & Co., Messrs. O. W. Potter, president, and John Parkes, manager of the North Chicago Rolling Mill Company, and the Pullman Palace Car Co., for the favors and attentions received at their hands, which have contributed so much to our enjoyment and information.

Mr. Babcock.—I wish to offer a resolution. I think all who have noticed the amount of work which we have got through with at this session, and the ease with which it has been done, will agree with me that the rules which have been made by the Council for that purpose are salutary, and that they have worked well, so far as they have been applied. The only regret might be that they have not been applied with still more strictness. I want to offer this resolution:

Resolved, That we heartily approve of the new rules established by the Council for conducting the business of these meetings, and that we favor their application in our future meetings.

Mr. Oberlin Smith.—I wish to make an amendment, if it is in order—that the time for the reading of a paper be ten, instead of five minutes. If an abstract contains all the ideas of a paper, that ought to be the paper itself. If it does not contain them all, it ought not to be read in the meeting, as presenting that paper.

The Chairman.—Before putting the motion on the amendment which Mr. Smith offers, as one of the committee which prepared these rules, I cannot forbear expressing the hope that the rules will be allowed to stand for at least one more trial. We have had twelve papers read at this meeting, and with the exception of two or, perhaps, three of those papers, they have all been very clearly presented by abstract in from two to five minutes, and I think that after a little more experience with the new rules the members will all find no difficulty in condensing their papers, so as to give a fair abstract of what they are within the five-minutes limit. As the papers are all in print, and are distributed long in advance of the meeting to all who expect to attend, there is no reason why the members should not make themselves familiar with them.

Mr. Kent.—I would make a point of order that these rules are established by the Council, and that we cannot amend them.

The Chairman.—The amendment is simply to the resolution of Mr. Babcock, expressing the sense of the meeting as to the rules.

Mr. Oberlin Smith.—I think that five minutes added to each paper would add very little to the length of the session.

The Chairman.—It would have added one hour to this meeting. We should be one hour behind time now.

The question being then put upon Mr. Smith's amendment, it was lost.

Mr. Babcock's motion was then carried, as read.

Mr. Kent.—I wish to call the attention of members to the fact that the American Association for the advancement of Science meets at Buffalo on the 18th of August. That Association has a section of mechanical science and engineering, and some of the subjects which are to be presented at that meeting will be of especial interest to members of this Society. One is the question of the education of an engineer. Another is the relation of the Government to engineers in civil life. A paper has been promised on that subject, and a very earnest discussion is expected. There has been a convention of civil engineers held in Cleveland recently, to bring about some reform in the Government's relation to civil engineers. I think mechanical engineers have not taken the inter-

est in this subject they should have taken in it. Another question is Endowment for Scientific Research, and the Co-ordination of the Several Engineering Societies of this Country. I hope those who can make it convenient to go to Buffalo, will do so. The meeting, I suppose, will have from eight hundred to a thousand members present.

The Chairman.—It is now one o'clock. Our business is ended, but there remains a paper by Professor Robinson, entitled "Experiments on the Flow of Gas from Wells." This came to the Secretary several weeks after the time at which, under the rules, the list is closed. Will the members have that paper presented and read, or will they adjourn?

Professor Sweet.—I move that the paper be laid over to our next session in the fall.

The Chairman.—I presume there will be no objection to reading the paper by title at this meeting and placing it on the minutes. What is Professor Robinson's preference?

Professor Robinson.—I would leave the matter wholly to the Society. The circumstances connected with it, I think, will necessitate the withdrawal of the paper unless it is presented at this meeting.

The Chairman.—You are satisfied with its presentation by title?

Professor Robinson.—I am not particular in regard to that.

The Chairman.—If there is no objection, then, the paper will be considered as part of the proceedings of this meeting, and will be read by title.

In closing this Thirteenth Semi-annual Session of the Society, I wish to add my own tribute of thanks to the resolutions which have been passed in appreciation of the work of the Local Committee. We have never had a meeting of more interest, or where we have had a warmer reception, than here in Chicago. I wish also to express the regret which, in common with all of you, I feel at the absence of Mr. Sellers, our President, and to thank all of the members here for their kindness and forbearance to me while occupying the chair during the session. The meeting is adjourned.

CCVI.

IRREGULARITY IN THE CONTRACTION OF DUPLICATE IRON CASTINGS.

BY THOMAS D. WEST, CLEVELAND, OHIO.

Many have been at a loss to know why foundries are so unsuccessful in producing equality of contraction in duplicate castings. Columns, for example, ranging from ten to twelve feet long when cast are generally found to vary in length from $\frac{1}{8}$ to $\frac{1}{2}$ an inch. A similar variation in contraction often perplexes the designer and engineer in such castings as half fly wheels, pulleys, and in castings for machines where uniformity in contraction is desired and figured for.

The main cause of all this variation in the contraction of duplicate castings is simply that *the mixture of iron in them is not exactly the same.* The difference in contraction which will be caused by the slightest change in the mixture of iron is often surprising. Why mixtures are not produced exactly alike is a question which many will be interested in having answered.

To obtain exactness in mixtures of iron melted in a cupola calls for conditions over which even when known foundrymen can often have little or no control. These conditions are: First, that the iron used shall be of uniform chemical composition. Second, that the fuel with which the iron is melted be also uniform in chemical composition. Third, that the foundry foreman and melter be men possessing good judgment and knowledge of iron and management of cupolas. Fourth, that the character of the work moulded will admit of uniformity in heats and charges of iron.

The difficulties to be encountered in meeting the first condition will be easily realized by those familiar with iron. The greatest element of uncertainty in obtaining like mixtures in the selection of iron is caused by the use of scrap.

With pig all graded from one cast, a uniform mixture can generally be relied on so far as that material is concerned, but upon scrap such reliance cannot be placed, for the reason that under the most favorable circumstances there can be no certainty of its being all alike

in composition. In a large pile of scrap composed of one kind of castings, although the iron may appear even in grade, yet it is certain that there must exist a dissimilarity in its chemical composition, from the fact of the scrap having been collected from castings made from different heats and from different irons. If from a scrap heap all of one kind this difficulty arises, what is to be expected of scrap piles of such variable character as are generally found in foundry yards.

The evil tendency of variable fuel to cause irregularity in the mixtures of iron cannot but be foreseen when the variable percentage in carbon and sulphur is considered which new shipments of fuel are liable to contain. This point may be said to be a delicate one, but, nevertheless, it requires attention, as sulphur and carbon in fuel are very effective elements for changing the character of iron melted in a cupola. It will of course be evident from the manipulations required for melting cast iron in "air furnaces," where the iron is not tapped out until it all is melted, and where the fuel and iron are separated, that under good management the chances are greatly in favor of greater uniformity in the mixtures of iron, at any rate for one heat, when melted in that class of furnace.

The importance of the third condition will be evident to all, from a perusal of the points here treated.

The fourth suggestion presented is practically the one which causes most variations in the contraction of duplicate castings from jobbing foundries, etc., and is one to which the jobbing foundry foreman, even if fully posted, is often forced to submit. In a jobbing or machine foundry two heats can seldom be arranged alike, and again, in one heat there may have to be several different grades of castings made, and therefore of iron melted. Under such a state of affairs, could the foreman have all the fuel and iron evenly graded for some special line of castings, it would be seldom that he could arrange his work economically, so as to insure uniformity in the contraction in some special line of castings.

There are several plans of melting by which uniformity in the mixture of iron for castings may be obtained. Some of these are more expensive than the ordinary and every-day practice will admit, and so they are little used or known. Yet, as it may be found necessary to resort to these plans in some cases, on account of the variable character of the line of work in a shop, or because some specially fine measurements are to be desired or obtained, it will be well to notice them.

The first plan is simply to make a special heat of the kind of mixture desired, and large enough to give all the iron for the number of castings desired. This iron as it comes down should be collected in a ladle large enough to hold it all, after which it is then poured out into a pig bed. The pigs thus obtained, free from any other mixture, are remelted and used for pouring off the moulds or castings desired, which may, on account of the facilities for moulding, require more than one heat. The expense of this plan, and the limit to the amount of iron which can be obtained for it, and also its frequent impracticability, would be forcibly impressed upon any who might desire to get a foundry man to practice it.

The second plan is to collect sufficient pig and scrap all of the same class to make the number of castings desired. Then let the same be charged and melted down in heats entirely free from any other mixture. This plan would resemble the former in cases where the shop cupolas were in constant daily use melting iron foreign to the special mixture desired, or where the melting capacity of the cupola is greatly in excess of the weight necessary for the special mixture, in presenting disadvantages, practically as well as economically.

The third plan is to charge the special graded iron at the first of a heat, and then, after the charges are all melted down, to shut off the blast and renew the bed with fresh fuel, and charge on whatever other mixtures are desired till the cupola is full. When this is done the blast is again started, and the balance of the heat is run off in the ordinary manner. The length of time which may elapse between the first and second blow can range from the earliest moment up to three hours or more, but the sooner the second blow is started the better are the chances for procuring hot iron. Again, if the first blow was to exceed one-quarter of the melting capacity of the cupola, it may be necessary for the breast to be knocked out and to have the dirt and slag cleaned out. All details necessary to be followed in manipulating this third plan have been given elsewhere by the writer, so that no further reference need here be given.* However, it might be well to state here that this third plan is more economical and often more practical than the first two plans given.

The fourth and last plan which will now be noticed is one which can be practically applied by almost all foundries, although it is not nearly so reliable as any of the other three given above. By it fly-wheels,

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* *American Foundry Practice.*

sixteen feet in diameter, weighing over fifteen tons, cast separately in halves, are often made under the supervision of the writer, and when put together no difference is discernible in their diameter or contraction. Before commencing to cast a batch of wheels, enough iron of the same character is gathered on hand to run off the number of wheels wanted. In justice to foundry men it should be said that one thing which also greatly favors the results obtained is the large quantity of iron which has to be charged. If the quantity was small the same success could not be expected. It is to be understood that other mixtures are run through the cupola at the same heat, and to prevent their getting mixed with the fly-wheel iron more of the fly-wheel iron is charged than is wanted, and to use it up, some piece is made to follow the fly-wheel pouring, for which nothing definite in mixture is required.

It will of course appear from the remarks upon fuel in the previous part of this paper, that in all of the above four plans care must also be taken to see that the character of the fuel is as uniform as possible, as well as that of the iron.

Another point which has a bearing upon the question here at issue is the *temperature of the metal when poured.* This affects it by reason of the difference which exists in the contraction of hot and dull-poured metal; also by the power of liquid iron to strain a mould or to fuse its surface sand, by which a scale may be formed upon the surface of the casting, in massive work from $\frac{1}{32}$ to $\frac{1}{4}$ of an inch deep. The hotter the metal is when poured, the more it can increase strainage, and fuse the surface of the mould and affect the finished size. With reference to the difference in contraction of hot and dull-poured metal, as far as its own elements are concerned, several trials which have lately been made, using bars 5 feet long by $1\frac{1}{2}$ inches square, cast horizontal and carefully moulded by the writer, have showed that *hot-poured metal will contract more than dull-poured*, as the hot-poured bars contracted on an average about $\frac{1}{32}''$ more than the dull-poured bars. The bars when being moulded had the sand dug away from the ends of the patterns when the nowel was rolled over, and iron plates one-quarter of an inch thick by two inches square were rammed up hard and tight against them. The pattern before being drawn would not be rapped at all endwise. By these two means and by drawing up the pattern square, the moulds were all made of exactly the same length, a thing to accomplish which too much caution cannot be taken by any who might desire to give the matter a test. The bars

were poured by means of a ladle holding about 100 pounds of metal. The metal in the ladle, after pouring one bar, was allowed to cool down naturally until it was as dull as could be safely used for giving a good full-run bar. The gates for both bars, it might be well to say, were cut large so that the moulds could be quickly filled. From the above three facts, it will appear evident to all that duplicate castings should be poured with metal of like temperature.

This whole paper is of course based upon the supposition that the setting of sweeps, mouldings, and rapping of patterns, etc., are always performed alike by the moulder, but it is not intended that any one shall turn upon the author with the question—Do THEY?

DISCUSSION.

Mr. John E. Sweet.—One cause of unequal shrinkage in castings not mentioned in Mr. West's paper was met with in our experience, and owing to the attending circumstances, it proved to be of an interesting character. An account of it may be of some value.

In brief, we were casting a lot of flange-pipe 8 feet 2 inches long, and with others, six lengths of 5 inch pipe each day. As the pipe were cast with chilled ends, we were in the habit of testing them for length, and to our surprise we found that they varied from $\frac{5}{8}$ inch to $\frac{3}{4}$ inch; although cast from the same pattern and same ladle of metal, half were too long and half of the required length. As a second peculiarity, we noticed that about one-half were usually sound and one-half unusually poor. It was with this lot of pipe also that we discovered that the core bars grew shorter by use, and, too, that one-half changed more than the other half. With the above combined facts before us, it only needed the further fact, that one-half of the core bars were larger than the other half, to lead to a satisfactory and probably correct solution of the whole trouble. Three of the core bars were of $3\frac{1}{2}$ inch gas pipe and three of them 3 inch gas pipe perforated for vent.

My theory is, that when the melted metal first set, it bound firmly on the sand core and large core bars which resisted end shrinkage, and while this was taking place, the metal of the castings was partly torn asunder and rendered porous in the same manner as the center of a mass is torn or made porous by the outside cooling first. By the time the castings had cooled down and become rigid, the core bars had been raised to a red heat and become soft, so that the final cooling and shrinking of the pipe-casting actually upset

the core bars from day to day by its frictional grip upon them. These actions did not take place in the case of the small core bars, or at least not to so great an extent, but if to any extent, may we not look to the core bars, and flasks even, to account for unequal shrinkage and in some cases unsoundness of castings? In the case of steel castings it is a well-known trouble, that pieces of certain shapes pull themselves apart before the mould can be broken out of the way to permit shrinkage.

While it is not strictly within the scope of this discussion, as a useful fact it may be well to explain one successful method of arranging the dry sand mould to allow for this rapid contraction in steel castings. Take for example the casting for a connecting rod, where there is an enlargement at each end. In addition to the pattern itself, there is moulded in at the same time patterns in the form of what is known as "horn sprues," just inside of the projections of the enlarged ends, but not quite in contact with the connecting rod pattern. After the pattern and the patterns of the "horn sprues" are withdrawn and the mould dried and closed, it will be understood that as no metal is admitted into the horn sprue cavities, the mould is weak and the contracting metal can crush it at these points.

*Mr. Thomas D. West.**—From a study of Prof. Sweet's comments upon the irregularity of contraction in his flange pipes, I cannot fully agree with the theory which he advances. As far as I can judge from the remarks presented in his discussion, the trouble would seem to be largely due to the variation in the soundness of the castings, to which he calls attention when he says that "one-half were usually sound and one-half unusually poor." Under such a state of affairs there could not exist a uniformity in contraction, let the metal be never so uniform in mixture. The elements called out by Prof. Sweet's discussion are all good and have their effect to some degree, but such a difference in the contraction of the casting which he cites, was mostly caused, in my judgment, by the difference in the soundness of the metal.

*Contributed since adjournment, under the rules.

CCVII.

THE ENGINEER AS AN ECONOMIST.

BY HENRY R. TOWNE, STAMFORD, CONN.

THE monogram of our national initials, which is the symbol for our monetary unit, the dollar, is almost as frequently conjoined to the figures of an engineer's calculations as are the symbols indicating feet, minutes, pounds, or gallons. The final issue of his work, in probably a majority of cases, resolves itself into a question of dollars and cents, of relative or absolute values. This statement, while true in regard to the work of all engineers, applies particularly to that of the mechanical engineer, for the reason that his functions, more frequently than in the case of others, include the executive duties of organizing and superintending the operations of industrial establishments, and of directing the labor of the artisans whose organized efforts yield the fruition of his work.

To insure the best results, the organization of productive labor must be directed and controlled by persons having not only good executive ability, and possessing the practical familiarity of a mechanic or engineer with the goods produced and the processes employed, but having also, and equally, a practical knowledge of how to observe, record, analyze and compare essential facts in relation to wages, supplies, expense accounts, and all else that enters into or affects the economy of production and the cost of the product. There are many good mechanical engineers;—there are also many good "business men;"—but the two are rarely combined in one person. But this combination of qualities, together with at least some skill as an accountant, either in one person or more, is essential to the successful management of industrial works, and has its highest effectiveness if united in one person, who is thus qualified to supervise, either personally or through assistants, the operations of all departments of a business, and to subordinate each to the harmonious development of the whole.

Engineering has long been conceded a place as one of the modern arts, and has become a well-defined science, with a large and grow-

ing literature of its own, and of late years has subdivided itself into numerous and distinct divisions, one of which is that of mechanical engineering. It will probably not be disputed that the matter of shop management is of equal importance with that of engineering, as affecting the successful conduct of most, if not all, of our great industrial establishments, and that the *management of works* has become a matter of such great and far-reaching importance as perhaps to justify its classification also as one of the modern arts. The one is a well-defined science, with a distinct literature, with numerous journals and with many associations for the interchange of experience; the other is unorganized, is almost without literature, has no organ or medium for the interchange of experience, and is without association or organization of any kind. A vast amount of accumulated experience in the art of workshop management already exists, but there is no record of it available to the world in general, and each old enterprise is managed more or less in its own way, receiving little benefit from the parallel experience of other similar enterprises, and imparting as little of its own to them; while each new enterprise, starting *de novo* and with much labor, and usually at much cost for experience, gradually develops a more or less perfect system of its own, according to the ability of its managers, receiving little benefit or aid from all that may have been done previously by others in precisely the same field of work.

Surely this condition of things is wrong and should be remedied. But the remedy must not be looked for from those who are "business men" or clerks and accountants only; it should come from those whose training and experience has given them an understanding of both sides (viz.: the mechanical and the clerical) of the important questions involved. It should originate, therefore, from those who are also engineers, and, for the reasons above indicated, particularly from mechanical engineers. Granting this, why should it not originate from, and be promoted by The American Society of Mechanical Engineers?

To consider this proposition more definitely, let us state the work which requires to be done. The questions to be considered, and which need recording and publication as conducing to discussion and the dissemination of useful knowledge in this specialty, group themselves under two principal heads, namely: SHOP MANAGEMENT, and SHOP ACCOUNTING. A third head may be named which is subordinate to, and partly included in each of these, namely: SHOP FORMS and BLANKS. Under the head of Shop Management fall the

questions of organization, responsibility, reports, systems of contract and piece work, and all that relates to the executive management of works, mills and factories. Under the head of Shop Accounting fall the questions of time and wages systems, determination of costs, whether by piece or day-work, the distribution of the various expense accounts, the ascertainment of profits, methods of book-keeping, and all that enters into the system of accounts which relates to the manufacturing departments of a business, and to the determination and record of its results.

There already exists an enormous fund of information relating to such matters, based upon actual and most extensive experience. What is now needed is a medium for the interchange of this experience among those whom it interests and concerns. Probably no better way for this exists than that obtaining in other instances, namely, by the publication of papers and reports, and by meetings for the discussion of papers and interchange of opinions.

The subject thus outlined, however distinct and apart from the primary functions of this society, is, nevertheless, germane to the interests of most, if not all, of its members. Conceding this, why should not the functions of the society be so enlarged as to embrace this new field of usefulness? This work, if undertaken, may be kept separate and distinct from the present work of the society by organizing a new "section" (which might be designated the "Economic Section"), the scope of which would embrace all papers and discussions relating to the topics herein referred to. The meetings of this section could be held either separately from, or immediately following the regular meetings of the society, and its papers could appear as a supplement to the regular transactions. In this way all interference would be avoided with the primary and chief business of the society, and the attendance at the meetings of the new section would naturally resolve itself into such portion of the membership as is interested in the objects for which it would be organized.

As a single illustration of the class of subjects to be covered by the discussions and papers of the proposed new section, and of the benefit to be derived therefrom, there may be cited the case of a manufacturing establishment in which there is now in use, in connection with the manufacturing accounts and exclusive of the ordinary commercial accounts, some twenty various forms of special record and account books, and more than one hundred printed forms and blanks. The primary object to which all of these con-

tribute is the systematic recording of the operations of the different departments of the works, and the computation therefrom of such statistical information as is essential to the efficient management of the business, and especially to increased economy of production. All of these special books and forms have been the outgrowth of experience extending over many years, and represent a large amount of thoughtful planning and intelligent effort at constant development and improvement. The methods thus arrived at would un-

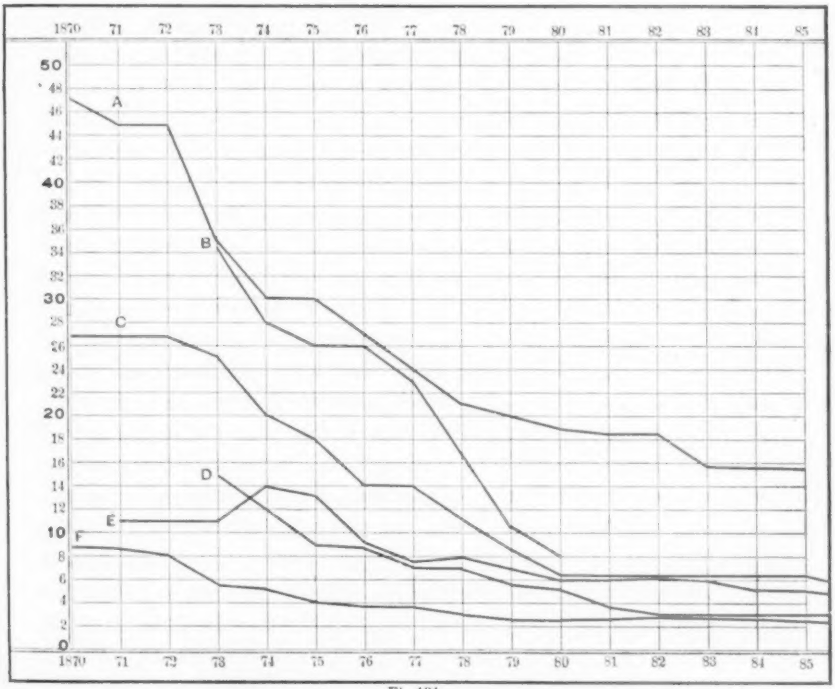

Fig.191

doubtedly be of great value to others engaged in similar operations, and particularly to persons engaged in organizing and starting new enterprises. It is probable that much, if not all, of the information and experience referred to would be willingly made public through such a channel as is herein suggested, particularly if such action on the part of one firm or corporation would be responded to in like manner by others, so that each member could reasonably expect to receive some equivalent for his contributions by the benefit which he would derive from the experience of others.

In the case of the establishment above referred to, a special system of contract and piece-work has been in operation for some fifteen years, the results from which, in reducing the labor cost on certain products without encroaching upon the earnings of the men engaged, have been quite striking. A few of these results, selected at random, are indicated by the accompanying diagram (Fig. 191), the diagonal lines on which represent the fluctuations in the labor cost of certain special products during the time covered by the table, the vertical scale representing values.

Undoubtedly a portion of the reductions thus indicated resulted from improved appliances, larger product, and increased experience, but after making due allowance for all of these, there remains a large portion of the reduction which, to the writer's knowledge, is fairly attributable to the operations of the peculiar piece-work system adopted. The details and operations of this system would probably be placed before the society, in due time, through the channel of the proposed new section, should the latter take definite form. Other, and probably much more valuable, information and experience relating to systems of contract and piece-work would doubtless be contributed by other members, and in the aggregate a great amount of information of a most valuable character would thus be made available to the whole membership of the society.

In conclusion, it is suggested that if the plan herein proposed commends itself favorably to the members present at the meeting at which it is presented, the subject had best be referred to a special committee, by whom it can be carefully considered, and by whom, if it seems expedient to proceed further, the whole matter can be matured and formulated in an orderly manner, and thus be so presented at a future meeting as to enable the society then intelligently to act upon the question, and to decide whether or not to adopt the recommendations made by such committee.

(This paper received discussion in connection with two others on germane topics. The discussion is printed at the end of the paper on "The Shop-Order System of Accounts.")

CCVIII.

INVENTORY VALUATION OF MACHINERY PLANT.

BY OBERLIN SMITH, BRIDGETON, N. J.

The keeping of *cost* and *valuation* accounts in connection with machinery has never been brought into so perfect a system as has ordinary commercial book-keeping. Recently, however, there has been a good deal of interest taken in the subject, and new light has been thrown upon keeping *cost* accounts by such valuable books as those of Captain Metcalfe and others. The matter of inventory valuations, however, with which it is proposed briefly to deal in this paper, is, to say the least, in a very mixed up condition, and although with some machinery owners it has received considerable attention, the average method contains a good deal of guesswork.

It is evident that at the very base of all account-keeping is the finding out the true value of the property kept account of; and that without this being correct, all else is useless.

Probably the most popular and frequently used method of doing this is by pure guessing. Another system is that of taking original cost at first, and then depreciating a given percentage each year, regardless of the several modifying conditions which will be mentioned later on. One large manufacturer, known to the writer, used to work upon this system with his machine tools, depreciating their value 10 per cent. each year. Although acknowledging that it brought the figures rather too low, he said that it kept him upon the safe side, in not letting his assets appear of greater value than they really were. However safe this method may be, it is worthless if the object is to show the real value of the property. This will be apparent if reference is made to the second line of the following table, wherein $100 is shown decreased at the end of each year 10 per cent. from the remainder belonging to the year previous:

YEAR.	0	1	2	3	4	5	6	7	8	9	10	15	20
	$	$	$	$	$	$	$	$	$	$	$	$	$
10% off......	100.00	90.00	81.00	72.90	65.61	59.05	53.15	47.83	43.05	38.74	34.87	20.59	12.16
5% off......	100.00	95.00	90.25	85.74	81.45	77.38	73.51	69.83	66.34	63.03	59.88	46.33	35.85

It will be noticed that at the end of 10 years the amount is only about 35 dollars, at the end of 15 years 20½ dollars, and at the end of 20 years about 12 dollars. In the third line is shown the respective amounts for \$100 as depreciated 5 per cent. each year instead of 10. This gives about \$60, \$46, and \$36 respectively, as the amounts at the end of 10, 15, and 20 years, and it is much more reasonable, for the valuation of machine tools, than is the first mentioned discount, if a system of this kind, with a constant ratio, is to be employed at all. The absurdity is, however, apparent, of using a tool costing \$100 when in such bad condition as to be worth but \$12, or even \$36. Such practice would be suicidal; and yet many tools need not be thrown away in 20 years.

Another method is to estimate the probable price which an article would bring at auction. This is a very indefinite way, as it is well known that there are auctions *and* auctions. In some of these the property brings more than it is really worth, while in others, where the proper bidders do not happen to be present, or where an article is bought for a purpose for which it is unfit, the prices are sometimes almost *nil*.

A striking illustration of the variable values which may be attached to a lot of plant may be seen by comparing the average insurance value and the average taxation value, the latter being usually a very different thing from the former, and the difference being something that frequently sadly puzzles the conscience of the owner to adjust, as it is a soothing balm to his pocket-book. The system now used of taxing machine-shop plant is very variable, and the average tax-assessor is often at his wits' end to know what value to put upon such articles as patterns and special tools, even if he arrives at any fair conclusion regarding the standard machinery. The result is usually a compromise between the high guesses of the assessor and the low guesses of the owner.

It will not be necessary in this paper to dwell upon the best methods of finding the cost of machinery or its productions, but taking it for granted that a machine shop's accountants have a complete record of the cost of all its plant, whether it shall have been purchased or made upon the premises, and supposing further that said plant is new and not deteriorated by wear, the question arises what is its *value?* The first and most important thing to do is for everybody concerned to get it out of their heads that value is necessarily dependent upon cost. There are many modifying conditions which prevent this being true. First among these variable condi-

tions is commercial *fluctuation* of value, and this applies perhaps more to purchased articles, such as standard machine tools bought in open market, than to patterns and other special tools, etc., made upon the premises; although the latter classes have of course certain fluctuations in cost, dependent upon the labor market and the current price of materials. Thus, if an engine lathe should have been purchased a year ago for $1,000, and remaining unused, should now be assessed, its value, of course, would depend upon the present price charged by its maker. A second factor in variation of values is *locality*. For instance, if the lathe above spoken of was to be used in a mountainous region away from railroads, it would be fair to add to its value at that place the freight and other expenses (perhaps also custom duties) paid to get it there, providing an equally good and cheap lathe could not be bought nearer at hand, where the freight would be less in amount. An appraiser must, however, be careful not to follow the rule of adding freight and custom charges to the cost of a machine, without proper discrimination as to whether it was necessary to bring the tool from a great distance or from a foreign country, instead of buying something nearer at hand. If the latter could have been done, the whole of such charges should of course not be added.

A third variable pertaining to values is *obsoleteness*, for it is evident that our hypothetical $1,000 lathe, even if it has been bought at the lowest market price, which has not yet fallen; and near at hand, where no freight of much amount was to be added; and is new and in perfect condition, is not worth $1,000 if a lathe can be bought for the same price which is of such superior design that twice as much work can be done with it in a given time. In this case, the first-mentioned lathe is practically obsolete, and its value might be less than nothing. This supposable case is, of course, an extreme one, but the fact is that in these days of intense inventive activity, machinery is constantly becoming more or less obsolete. In many cases, this is so only to a slight degree, especially in cases of machine tools, such as lathes, planers, drill-presses, etc. This fact is not much to the credit of mechanical engineers, but it is nevertheless a fact, that far less original design has been put into this class of tools than into many others outside of machine shops.

A fourth, and the most obvious cause of depreciation in machinery plant, is *wear and tear*, and there is perhaps more good judgment necessary in determining the exact amount of this depreciation than in any other part of the appraiser's work.

The grand principle which lies at the root of correct valuation, and which should govern the appraiser throughout all his work, is, that any article is worth not what it *did cost*, but *what it would cost to replace it to-day*, providing it is so useful that it would be desirable to so replace it were it destroyed. Thus, if a shop has a lot of machine tools which are built so near to the best modern practice that it would be desirable to duplicate them were they destroyed, they are worth exactly what said duplicates would now cost delivered and set up in the shop, less the depreciation due to the wear and tear. This rule also applies to boilers, engines, shafting, belting, shop fixtures, and small tools—anything, in fact, which can be bought in open market (and, for that matter, it can be applied also to buildings and ground, as well as plant). In the case of working drawings, patterns for castings, and other special tools, such as jigs, etc., all of which are usually made upon the premises, and whose chief cost consists in the items of labor and general expenses, together with a small amount for material, the method of obtaining the true value is of course somewhat more complicated. This is for the reason that the amount which such an article did cost is a very poor index of what it would cost to build a second one, it being usually the case that but one of a kind is needed, and no duplicate has been made. There must necessarily be some guess-work in getting the value of these articles, but it is usually from 10 to 30 per cent. less than the original cost spoken of—that is, if in full use. With this class of tools, the variation of market-price, locality, and wear and tear, do not occur to so great an extent as with machine tools proper, but the variation due to obsoleteness occurs in a very much greater degree than with almost any other class of property. It will be noticed above that working drawings and patterns are classed with jigs, as "special tools." They are not always regarded as such, but undoubtedly should be, as they have exactly the same general conditions governing their use. It may be here said that a marked distinction should always be made between an original drawing and a working drawing. One of the former class may cost $1,000 to make, on account of the designing which is incorporated with it, but as a drawing it is not worth more than $10, if it can be duplicated by an ordinary journeyman draughtsman for that amount. Whatever value the designing spoken of has in itself, must be found in some other part of the inventory, under the head of "patent rights," "good will," or something of that kind, rather than in the class "drawings." A working drawing, therefore, is

(and the same way with a jig) worth exactly what it would cost for a draughtsman to copy it off, plus the paper on which it is drawn. A pattern is worth exactly what the wages and general expenses would be for a pattern maker to duplicate it, plus cost of the wood, glue, varnish, or other material. The true value of a jig or templet may obviously be found in the same manner, always assuming that said articles are needed for frequent use in the regular production of the goods manufactured by the shop in question. It is evident that the value of all these classes of special tools depreciates enormously if said production is permanently decreased from regular and standard to occasional, or if the articles made are going out of fashion in the market, or are not able to compete in price with others of a similar nature. If they have become entirely unsalable from the above causes, or from having been superseded by improved articles of some other kind, then the value of the drawings, patterns, and other special tools with which they were produced is of course reduced to nothing. Great care should always be taken in appraising to rate such articles low enough so as not to show deceptively high assets, but at the same time, in justice to all concerned in the ownership of the property, they should not be put at a foolishly low figure, as were the patterns of a large manufacturing concern known to the writer, whose policy was to gradually depreciate all their patterns, until their value stood at "nothing" upon their books. This of course made them safe against showing false profits, and also had the merit of making their inventory worthless for this particular class of tools, as far as the legitimate functions of an inventory are concerned. The simplicity and ingenuity of this plan was more conspicuous than its common sense, especially after some time had elapsed, and the figures had gotten down very low. Of course if the system was right at this time, it must have been wrong at first, and to carry it out logically the patterns should all have been counted as worth nothing when they were first made.

In all jobbing machine shops, which do repairing and odd work rather than limiting themselves to standard manufacturing, there is a large accumulation of drawings and patterns (not usually, however, many jigs), which belong to what may be called "transient" jobs, and which will probably never be used again, or at any rate only occasionally. These should be valued at a very low figure, usually less than 10 per cent. of what they cost, the amount of this percentage depending upon the probabilities of their future use.

In estimating the depreciation due to wear and tear in engines, shafting, belting and machine tools, due regard should be had to the general system upon which they are run—whether they are allowed to wear themselves almost entirely out and are then replaced by new ones, of which a new inventory is taken, or whether they are kept up to a certain standard of goodness by the replacing of worn parts, etc. The latter is the system practiced by the writer for many years past, and is, in his opinion, undoubtedly the best one. Leaving out the question of *obsoleteness*, there is no reason why a lathe or a planer should not be run for twenty or thirty years and kept up to the standard (by frequent repairs and replacement of parts) to which it has attained in the third or fourth year of its age. Shafting and pulleys can be regarded in the same way, but can probably be kept nearer to a new standard, as they do not wear out so fast. Belting also can be treated upon the same principle, but kept at a lower standard, the average condition of a lot of belting throughout a shop usually being probably nearly half worn-out. The writer intends, for his own use, to establish for these classes of machinery, and also for small tools, such as twist drills, reamers, etc., a standard percentage of "worn-outness"—if such a word may be coined for the occasion. He has not yet made an accurate estimate of the proper percentage to be employed in each case, but probably a fair allowance for the percentage of present new value in a well equipped and properly taken care of machine shop (leaving out, as before intimated, the question of obsoleteness), would be, for shafting, etc., 80 to 90 per cent., engines and machine tools, 70 to 80 per cent., boilers and belting, 60 to 70 per cent., and small tools (which are constanly being ground away), 50 to 60 per cent. This estimate is, of course, only approximate, and its correctness would vary with the standard of condition which was adopted and the consequent thoroughness and frequency of repairs.

A properly kept inventory of the class of articles just mentioned would put them at new value the first year, and depreciate them from 5 to 10 per cent. annually, until the standard *constant* was reached, after which they would remain at about the same price each year, except as affected by violent fluctuations in the market, and by obsoleteness of design.

With regard to the special tools before mentioned, the depreciation for wear and tear need be but very little, as if they serve their purpose at all, they must be kept in such repair as to serve it perfectly; and they are not a marketable article in which a slight

deterioration in appearance would largely affect their value, as would be the case with standard articles. In the case of working-drawings, which are usually of trifling value, it is not worth while to take account of the wear and tear, as when worn too much for use they can be wholly replaced with duplicates, and the valuation can be kept, for convenience, at the same rate.

An excellent mental aid to an appraiser, in considering the value of doubtful articles, is to estimate what he would be willing to bid at auction for a duplicate, were the article destroyed. This amount, if correctly guessed at, is certainly a true index of the real value.

The writer has for several years past paid considerable attention to keeping a systematic inventory, in which all the property of the machine works with which he is connected is classified into "classes" and "sub-classes," so entered in tabular form that the names need not be re-written yearly except in case of additional articles entered. In this book there is a set of columns provided for each year, for a term of years to come, so that the value merely need be entered, together with the amount of depreciation since the last year. There are proper columns provided for cost, variation therefrom to obtain actual new value, subsequent depreciation for the various causes that have been mentioned in this paper, etc., etc. He will not, however, occupy the time of the Society now to describe this book in detail, though it may possibly furnish a theme for some future occasion. The object of this paper will be attained if it shall haply influence even a few among many engineers to us more systematic methods in estimating the true value of the property in their charge.

As a recapitulation of the foregoing, the rules governing an appraiser may be tersely stated thus: Rate all property that it would be desirable to reproduce, were it destroyed to-day, at the net cost of such reproduction, in its existing locality, minus its estimated damage by wear and tear. Rate partially obsolete articles the same way, but minus also a percentage of their apparent value equal to their estimated percentage of obsoleteness or of improbability of usefulness. Rate wholly obsolete articles at nothing.

(*This paper was read in connection with the paper on "The Shop-Order System of Accounts," and the discussion is printed at the close of the latter.*)

CCIX.

THE SHOP-ORDER SYSTEM OF ACCOUNTS.

BY HENRY METCALFE, WATERVLIET ARSENAL, TROY, N. Y.

I.

LET us imagine the art of music before its notation was devised. Think of the strains which might have been immortal, but which died as voices die, and were lost. Imagine the energy wasted in repetition: the effort beginning afresh with each new learner of each new tune, enlarging his own experience merely and leaving no vestige to guide another's way.

Look forward, on the other hand, and imagine it possible to catch and fix the vibrations of an untrammeled voice seeking expression in speech or song. Is not this the ideal toward which stenography and phonography are but instinctive and feeble approaches?

Now, administration without records is like music without notes—by ear. Good as far as it goes—which is but a little way—it bequeathes nothing to the future. Except in the very rudest industries, carried on as if from hand to mouth, all recognize that the present must prepare for the demands of the future, and hence records, more or less elaborate, are kept. Their elaboration depends on what their results are worth.

I used to think that only government workshops suffered from circumlocution, and took it for granted that private establishments had simple and direct methods of procuring supplies, of keeping track of work in progress and of determining its cost when done. I knew, of course, that no shop running to make money could afford to wait, as I have had to do, for the most necessary material, and assumed that in other respects their management was generally on a par with their facility in procuring supplies.

But in seeking better methods where the permanent personal responsibility of profit tends to whittle off excrescences of administration which lead to wasteful delay, I found that much had

been sacrificed to immediate advantage; that records were too often kept by memory, so that as the manager of an establishment employing 1,400 men once told me, "The trouble is, not in foreseeing necessities, nor in starting the work to meet them; but in constantly running over the back track to see that nothing ordered has been overlooked, and in settling disputes as to whether such and such an order was or was not actually given and received. Superintendence," said he, "would be very different work if I were sure that an order once given would go of itself through the works, leaving a permanent trail by which I could follow it and decide positively where and by whom it was stopped. As it is, I spend so much of my time in 'shooing' along my orders like a flock of sheep, that I have but little left for the serious duties of my position."

These were familiar words, and when I went further down, and saw how much foremen's time and memories were taxed for means of attending at once and finally to their daily wants, I became convinced that the government methods, though bad enough, were not the only ones to be criticised.

In the matter of costs, too, I found great uncertainty: I found one business which had been exposed to expensive litigation, involving $6,000,000, to determine what was the true cost of machinery sold by its agents at a commission based upon its cost of production. I found another entire trade based upon costs determined, as one of its members writes me, by "thumb-sailing:" large establishments suffering from the competition of ignorant free lances, who in ruining themselves also injured their neighbors.

II.

The proposed system of shop accounts is based on two compensating principles.

1. The radiating from a central source, let us say the office, of all authority for expenditure of labor or material. These being, however they may be disguised, the elemental forms of all internal expenditure.

2. The converging toward the office from all circumferential points, of independent records of work done and expenses made by virtue of that authority.

Upon the free play of the forces thus defined there is but one essential restriction; that every right to the means for executing

an order shall be qualified by a responsibility, which shall be recorded in as great particularity of detail as the scheme of management adopted may require. This leads to a comparison of managements depending upon the automatic record of their results. The following discussion will show how easily and cheaply this end may be attained.

In a broad sense a manufactory may be considered as an engine for transforming material, and its efficiency, like the duty of a pump, may be measured by the ratio of the effort exerted to the effect produced.

It will not be disputed that this ratio is best expressed by the true costs of its products, and that managements may be compared by their costs.

The object of the proposed system of accounts is to provide automatic, and therefore impartial means for determining the most probable cost of manufactures in gross, or in such detail as the expense of its determination may permit. This is no new necessity. It enters into the imminent questions of what we can afford to make at market prices; of what is the lowest selling price; and also into estimates relating to the differences caused by the addition or removal of parts and the substitution of processes.

The difficulty of analyzing the usual gross account of expenditures, the uncertainty of this analysis when made by clerks unfamiliar with the processes analyzed, and the evident objections to so employing the time of foremen, have generally led to more or less exact accurate estimates of cost by those whose management was more or less in question. I do not exclude the self-deception of absolute proprietors. These objections are increased as the product of an establishment is diversified, so that the more miscellaneous is the product, and hence the more necessary the knowledge of its difference in cost, the more difficult is this knowledge to obtain. A system which might serve a blast furnace would utterly fail in a repair shop, yet are not the accounts of repair shops often kept on blast-furnace principles?

It would seem that the practice of estimating costs would not be followed if a more positive method were available at a reasonable price.

The world has been working too long with existing methods for any one to hope to improve them; the change must be one of methods. I propose to replace the ordinary *ex post facto* analysis of expenses by a preliminary analysis of objects of expense to be

followed by a synthesis of items of expense, made mechanically by sorting cards on which the objects of expense have been indicated at the time of expenditure, by the persons who most probably knew them best, in symbols significant to the least experienced compiler.

By thus defining every charge for labor and material, our accounts are, so to speak, balanced in advance, and it only remains to distinguish between specific and general expenses and properly to apportion the general expenses among the specific, in order to obtain the most probable cost of any specific object.

III.

A manufactory may be functionally divided into two main portions, the workshops and the office.

In the shops are performed the processes with the records of which the office is principally concerned; on one side stands the foreman expending labor in transforming material; on the other sits the clerk recording the results of the other's acts. Taking these two as typical figures, I propose:

1. To require the highest local authority to define the objects on which its resources are to be expended. In other words, what accounts are to be opened.

2. To require the foreman to define the object most probably benefiting by the expenditure which he directs, as nearly as possible at the time that the expenditure is made.

3. To require a clerk, independent of the foreman, to compile the record of the foreman's acts.

4. To provide a simple symbolic language, common to both office and workshop, by which the same object of expenditure, whether it be a product, a component or an operation of manufacture, shall always be called by the same name and by which the foreman's symbols shall suffice the clerk, without requiring of either a knowledge of the other's work.

5. To make each act of record an independent unit by entering it on a separate card, certified by significant punch-marks.

6. To save clerk's work in combining similar entries by assorting mechanically cards containing similar symbols, only transcribing the summation of the charges they contain.

7. To provide that no claim for labor shall be allowed, nor any material put in the way of expenditure unless charged to its most

an order shall be qualified by a responsibility, which shall be recorded in as great particularity of detail as the scheme of management adopted may require. This leads to a comparison of managements depending upon the automatic record of their results. The following discussion will show how easily and cheaply this end may be attained.

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5. To make each act of record an independent unit by entering it on a separate card, certified by significant punch-marks.

6. To save clerk's work in combining similar entries by assorting mechanically cards containing similar symbols, only transcribing the summation of the charges they contain.

7. To provide that no claim for labor shall be allowed, nor any material put in the way of expenditure unless charged to its most

probable object; so that to every right there shall attach a responsibility of record.

8. To provide for the transfer between general and specific expenses, of charges more probably belonging to either.

9. While allowing free play to the foreman, to increase correspondingly his responsibility as measured by—

1. The cost of specific work.

2. The ratio of his general expenses to the causes of such expenses.*

10. To eschew the use of books, except for final records, because of—

1. Their inflexibility; they can be used by but one person at a time.

2. The labor of combining similar entries made in them at different times and places.

3. The certainty that when used for memoranda, the effete matter will soon obscure the important, so that the longer an entry has escaped attention, the more certainly will it be neglected.

11. To prefer natural methods to arbitrary, so that those who may use the system shall of themselves tend to conserve it.

IV.

THE SHOP-ORDER SYSTEM.

The system has three principal objects in view:

1. The prompt performance of work by the prominence given to unfinished orders.

2. The determination of the most probable cost of work and of management.

3. The keeping of an account of stock, in units of material as distinguished from their values.

It attains these objects by using three forms of cards, viz.:

1. Shop-order tickets, or warrants of expense, and records of expense reported on

2. Service cards.

3. Material cards.

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* It will be seen that relieving himself under one head increases his responsibility under the other, so that the line of least resistance will be the truth. The same result is reached by requiring daily records, the immediate bearing of which on ultimate costs can hardly be appreciated at the time.

V.

Taking the above objects in the order of their importance we have—

1. **Orders for work**: shop-orders.

These are of two kinds, viz.:

1. Special orders, requiring the performance of specific work.

2. Standing orders, requiring the maintenance of certain facilities for the execution of the special orders.

These facilities may be either in charge of certain foremen, the costs of whose management we wish to compare, or may be too general in their nature to be assigned to any one department.

The first are called departmental, and the second, general, standing orders.

Designation:

The special orders are designated by serial numbers, beginning at 100, according to their sequence in the shop-order book.

Each department of the manufactory is known by a number, preferably in the order of work, and the standing order relating to its maintenance has the same number as the department. The numbers below 100 may be reserved for these orders; *e.g.*, 1. for the pattern-shop. 2. for the foundry, etc.

General standing orders may run from 50 to 100. In deciding how many and what they shall be, we must remember that our first analysis may safely be detailed because details may always be combined by neglecting their differences, and it is easier so to combine them than to analyze results too grossly stated into their component parts. The more complete is our preliminary analysis, the more stable will be our synthesis. The history of chemistry and of mathematics teaches this. In another sense we say "we divide that we may rule."

The following general standing orders are suggested:

51. Office expenses relating to factory.
52. Office expenses relating to sales.
53. Office, and other expenses which cannot be classified.
54. Power.
55. Heat.
56. Light.
57. Transportation, in and about factory.
58. Repairs of buildings, not departmental.
59. Superintendence, general.

For rent, insurance, taxes and the subdivision of general expenses, see Cost of Work, page 454.

Authority to issue orders.

The authority for all orders is vested in the office; but, as is customary, is more or less extended to include transactions between foremen. With the free exercise of this right is combined the incidental responsibility of a written record, retained by the recipient, who is in turn restrained by the automatic record of the cost of his work. Both records coming finally to the office, one foreman is accountable for the necessity of the order and the other for the cost of executing it. This principle of liberty qualified by responsibility runs throughout the plan.

Form of order.

The shop-order book provides a place for the record of every order originating in the office. A special order here receives its serial number, and work of a general nature worth special entry takes the number of its proper standing order.

To distribute orders, and for other purposes, the order ticket is devised. See duplicate form separated by a perforated line, page 447. A punch-mark of special design in the "authority" space, indicates the giver.

Standing orders and their numbers are circulated in lists which are soon memorized.

Course of tickets.

I shall describe the simplest case first, as its principles apply in all others.

1. For short jobs on which only one kind of work is done at a time, single tickets serve.

They are displayed in a rack in each foreman's office, while the work is in his shop. When the work is done he punches out his number* in the marginal line headed "completion" and passes the work and the ticket with it to the next foreman in order. This is continued until the ticket reaches the office, where the date of its completion may be entered in the shop-order book.

2. When work is to begin or continue in more than one depart-

* Men are known by the numbers of their shops or of the departments in which they work. In each department they are ranged by invariable numbers according to their importance, seniority, etc. Thus of shop, or department 3, the foreman is No. 301, the next man 302, and so on. This allows for 100 men in each department. If this number should be exceeded, many expedients of correction are possible. M.A. below signifies Master Armorer in arsenals; in private shops S. might be used for Superintendent.

ment at a time, separate tickets must be made out for each department. These issue directly from, and are returnable directly to, the office, as soon as each department's work on the job is done.

Receipt.	M-A.	201.	205.	301.	401.	501.	601.	701.	801.

S-O. ; **C.** ; **O.** ; **N.** ; 188 .

Authority, . Completed, 188 .

Completion.	M-A.	201.	205.	301.	401.	501.	601.	701.	801.

Receipt.	M-A.	201.	205.	301.	401.	501.	601.	701.	801.

S-O. ; **C.** ; **O.** ; **N.** ; 188 .

Authority, . Completed, 188 .

Completion.	M-A.	201.	205.	301.	401.	501.	601.	701.	801.

3. Subordinate orders.

Foremen requiring the co-operation of others may originate tickets specifying the work to be done, by indicating the number of the

original order authorizing the work and punching the authority space with their special punches. The tickets are returned to the office by the recipient. They may well be white to distinguish them from office tickets, which may be of two or three different colors to indicate the relative urgency of the work they authorize.

Thus S-O. 789. *Build 6 double axle lathes,*

might be on a yellow ticket, indicating a staple manufacture, and—

S-O. 2, P. *Cut door, north side pattern-shop*

on a blue ticket, to indicate local work of an important nature. Such tickets emanating from the office would be in ink, and would refer to drawings, specifications, etc.; but a merely local order or foreman's request, such as

55, W. *Stop leak in steam coil,*

sent, say, by the master carpenter to the master machinist, might be in pencil on a white ticket.

It is desirable, but not essential, that subordinate orders be in writing. The advantage in definiteness, in responsibility, in the certainty of execution, and in the accuracy of the record which follow from writing them are so great as to outweigh the slight loss of time taken to fill them up and punch them. A package of tickets, a lead pencil and a ticket punch are all that a foreman needs for attending finally to any order which he is competent to give.

In complicated operations, where it is desirable to take heed of the receipt of orders on their delivery, duplicate tickets, such as shown in full, may be used to advantage. The duplicate ticket is also intended for a complete exhibit, say in the racks of the superintendent's office of unfinished work ordered by him or by his superiors. As the completed tickets come in from the foremen, he takes down his retained copy from the proper rack, punches it, and returns it to the main office, keeping that which he has received.

Advantages of order tickets.

Each foreman's unfinished work is always displayed before him, relieving his memory and permitting him to apply all his energy to active work. This applies in even greater measure to the superintendent.

For the orders in question the ticket represents both the work and the authority for doing it; and as no foreman receives work without its warrant, each one checks the other and prevents the loss

or neglect of either. The loss of a ticket, at the worst, would correspond only to an order forgotten under common methods; in practice they are never lost.

SERVICE CARD, Frankford Arsenal.

No. Name. Price per Unit.

Charge to-	Nature of service in detail.	No. of Units.	
S.O.-		Pieces.	Time.
C.-		AMOUNT.	
O.-		Dolls.	Cents.
N.-	N. B. Make but one entry on each card.		

UNITS. { Time. Pieces. } DATE.

The tickets in the racks may be classified as work in hand or as not in hand, by departments, and in many other convenient ways. When returned to the office after completion, they may be re-

sorted according to objects worked on, forming an indefinitely expansible index to the order book.

VI.

RECORDS OF EXPENDITURE.

Having shown how an order, started from the heart of the administration or from one of its intermediate points, finds its way out to the circumference and along it; and how, its work accomplished, it is by natural means brought back to its source, it remains to show how the records of the expense it has involved are similarly directed in their centripetal course.

The expenses of a workshop may be classified under two heads:

1. Services. { Internal. / External.
2. Material.

1. SERVICES.

Labor may be performed either within the establishment or without it. To both kinds of labor the name service is given, and their record is kept on uniform service cards. For internal services these are roughly bound into little books like check books, each page containing one card detachable like a bank check, and having a memorandum stub for the workman's private use. All cards are printed on one side of brown manilla paper.

Employment of service cards.

One of these cards, designed for arsenal use, and such as, with the addition of a space for "machine time," is used by one of our large locomotive factories, is reproduced full size on page 10.

R. G. & SON. APR 2 1886

No.	Name.	Rate.
235.	Lannigan,	0.25.

ORDER NO.	PIECE.	WORK.	HOURS.
789	Spindles.	Planer.	10

The above is a full size cut of a similar card in simpler form,

designed for day work in an establishment where no piece work is done. If greater simplicity were required, the two middle spaces might be omitted.

When a man is hired he receives a book in which, on each card his name and rate of wages are stamped. This certifies his employment to his foreman. He gets his book from his foreman in the morning and returns it at night with one page filled for every order on which he has worked during the day.

If he has worked all day on but one order, his writing may consist of its number and the number of time units in a working day. Thus, if the hour is taken as the time unit, he might write—

S-O. 789 Time units, 10

If time is kept by the half-hour it would read—

S-O. 789 Time units, 20 and so on.

The foreman looks over the books when handed in; if correct, stamps them with his dating stamp, tears out the leaves filled, and sends them to the office. The next morning he returns the books to the men.

The book serves a double purpose: it affords the workman an opportunity for making a definite charge for his labor, and it gives him the only opportunity of doing so. This makes certain a record of his employment during the time for which he is paid, and also affords original evidence from an impartial source as to the object on which that labor has been spent.

It takes the place of a roll call or time check. Early comers get their books at once and can go to work. Late comers are so marked on their own books. Those who leave early have their books verified at the time.

If a man has worked on several orders during the day he fills out a separate leaf for each order, the sum of the times equalling the total time, as before.

Piece work is similarly entered, the leaf being punched by the inspector. The time and piece records are independent of each other, so that if a batch of piece work should last a man for several days, he still makes his time record for those days at *no* price.

When the cards reach the office, daily, they are sorted, if need be, by names and the total time, pieces or wages entered in the time book. They are then re-sorted by orders and distributed in pigeon holes corresponding to the orders.

2. External services.

Outside services embrace freight, insurance, rent, taxes, telegraphing, attorney fees, etc., pertaining to factory. These cards, when approved, are filed like those first described.

Payment for such service must be similarly distributed among the shop orders benefiting by the expense. It is optional whether this shall be done for each bill before payment, or whether such charges shall be consolidated from the books monthly or oftener.

2. MATERIAL.

Material card.

This card, which is freely distributed in blank to the foremen, permits every transaction with material to be recorded. The accom-

MATERIAL CARD.

APR 3 1886

QUANTITY.		NAME.
ASSUMED. 6	UNIT. pcs.	N. B. Make but one entry on each card.
ACTUAL. 164	lbs.	Sanderson Steel $\frac{3}{4} \times 1\frac{1}{4}$ about 8 ft. long.
Price per Unit. Dolls.	Cts.	

CHARGE TO				CREDIT TO				AMOUNT.	
S-O.	C.	O.	N.	S-O.	C.	O.	N.	DOLLARS.	CENTS.
820	W								

Ordered from Corning & Co.

REQUIRED BY ● CERTIFIED BY ●

panying form is devised especially for private shops. If a foreman wants some steel he fills the card as shown, charging it to the order for which most probably needed. He makes a direct charge to a special order if possible; if not, then preferably to his depart-

mental standing order. The foreman makes his entry in pencil, entries of price and amount being added by clerks.

Punching out "required by" he throws the card into his messenger box and concerns himself no more about it. Without awaiting a special time or opportunity for making known his wants; without awaiting the return from the office of his "requisition book," he has, at the very moment that the need of the steel presented itself, asked definitely and finally for what he wanted. He has set rolling a ball which will be in somebody's way until it is finally disposed of. At the office it may be approved or sent back for explanation, or simply suspended, without interfering with immediate action on other articles asked for at the same time. A long list is like a large bank-note, easy to carry, but hard to change.

Suppose that the requisition is approved by the superintendent's also punching "required by," the card is sorted with other cards of the same kind, say for hardware, the name of the dealer from whom the material is to be ordered attached, or not, at pleasure, and the card sent with others to the foreman or storekeeper who is to receive it on arrival. If to the foreman, he knows what to expect.

When the steel has come the quantity actually received is filled in, the receiver punches "certified by" or "received by," or whatever special form of acknowledgment may be required by the management, and sends the card to the bill clerk, who, after comparing it with the bill, and may be adding prices or amount, sends it to the cost clerk for filing in the proper pigeon-hole.

Let us suppose again that the foreman, having no immediate use for the steel, has charged his departmental standing order with it. By and by he finds that he wants 10 lbs. of it for a special job. He makes out another card, charging it to the special job and crediting himself accordingly on the same card and punches "certified by" as before. The converse is possible if he finds that he has charged too much to the special order first mentioned.

If he lets another foreman have steel he charges and credits appropriately between departmental orders, certifies the entry, and gets the other foreman to do so before he gives up the steel. The issuer keeps the punched card as his equivalent and sends it to the office for entry.

The card may also be used for reporting each batch of work packed or shipped or sent to the store-room or warehouse, as the custom of the place may require. Such cards contain a credit to the order under which the material has been made. They take

theplace of all memoranda re-copied into lists for office use. Each card may start independently of the rest at the very time that the batch is done or inspected, so that there may be any number coming into the office during the day. Like the other cards they are movable memoranda, written once and for all by those responsible for their accuracy.

These are the simplest of many possible cases; I have so far been unable to imagine one in which the card fails to tell its story in the easiest and plainest way.

VII.

THE COST OF WORK.

This, second division of our subject, involves two elements:

1. The work done.
2. The cost of doing it.

The second of these divided by the first gives the price.

1. The work done.

An order having been completed, we may simply wish to know what it has produced.

This may be determined in any customary manner, subject to this precaution, that it is not always safe to assume that the exact number of articles ordered by the tickets has been made. The means described, page 14, are probably as easy and expeditious as any that can be devised.

The gross product is therefore easily determined, but, except in the crudest industries, this will hardly satisfy those in charge. There is scarcely any work which does not require some preparatory expense in the way of drawings, patterns, tools, etc., which may be useful for future work of the same kind.

We shall generally, therefore, require separate information as to—

1. What must be done again every time that such an order is repeated: what is made for sale.

2. What has been done in preparation, having, when the order is completed, a permanent value for future work of the same kind: what is made for the establishment.

In anticipation of such inquiries we provide in advance that all expenditures, besides being reported under the order authorizing them, shall be referred, under that order, either—

1. To "Work," symbol W., or
2. To "Plant," symbol P.

"Work" has been sufficiently defined above.

"Plant" includes drawings, patterns, machinery and special tools and fixtures not apt to be consumed during the execution of the order. Buildings, etc., are plant of standing orders, their extensive repairs and improvements are charged to P.; current repairs to W.

The simple analysis given suffices for miscellaneous products; but for the staple objects of manufacture for which a factory may be specially designed, such as guns, sewing, or other machines and appliances made on a large scale, we may also wish to be prepared to collect information relating to—

3. Their component parts.

4. The operations of manufacture through which these parts have passed.

These comprise all possible questions involving cost, which, to be truly answered, must be prepared for before the work to be analyzed is begun.

Therefore, although it is essential that only the number of the shop order appear on every record of expense, yet, for a full development of the system, it is desirable that every such record have room for four symbols, viz.:

S-O. The number of the *order* authorizing the expense.

C. The *character* (P. or W.) of the expense.

O. The component part or *object*, profiting by the expense.

N. The symbolic *number* of the mechanical operation performed on the object in question.

Room is therefore made for the symbols S-O.; C.; O.; and N. on the service and material cards already described. Only so many of these symbols need be used as the scheme of administration may require: some will be satisfied with gross costs, and will need only the first symbol; others will require plant to be separated from work; and others still, for staple manufactures, will want to know the cost of components and of the operations upon them. Such demands must be anticipated at a cost proportional to the benefit expected: as we would reap, so must we sow.

2. The cost of doing work.

The net cost consists of—

1. The specific expenses for labor.

2. The specific expenses for material.

These are also called the direct expenses, or those which can be charged directly to any particular job. Added to—

3. A proper proportion of the general annual, or indirect expenses, they make the gross cost.

It is comparatively easy to compute net costs by any of the usual methods. Their exactness depends upon the scale of trouble adopted, and, excepting errors of omission arising from unbalanced data, they may be assumed to be fairly accurate.

The main difficulty lies in apportioning those general or indirect expenses which cannot be referred to any special product. I therefore give special attention to this subject, as follows:

Apportioning the indirect expenses.

Factories are established for the profitable transformation of material by the organized employment of labor. How shall the indirect expenses be distributed? in ratio to the material or the labor? by quantity, or by value?

I believe that the incidental expenses are incurred for the purpose of making labor more effective, and that the more material enters as their divisor, the more does it vitiate the probability of the result.

For the more material costs, the more labor it has already had spent upon it; and the less, and not the more, does it need the facilities provided by the incidental expenses. On the other hand, the more men are employed, irrespective of their cost, the greater is the wear and tear, the waste, the cost for room light, heat and attendance, etc.

These and other similar considerations lead me to determine for each department a cost factor, as follows:

1. To distribute such general expenses as rent, insurance, taxes, etc., among departments profiting by them according to the most probable hypothesis.

2. To distribute last year's general standing orders or the unclassifiable current expenses among departments in proportion to the total day's work done in each department.

3. To add this amount for each department to the sum of its own expenses for the past year, as given by the cost of its departmental standing order.

4. To divide the gross amount thus obtained by the number of direct days' work done in each department during the past year, and so obtain a cost factor, say of $1.15 per day, by which the cost of every day's direct work in the present year must be increased in order to make it bear its most probable share of the cost of facilities provided for it.

Thus a man at $2.00 a day would be really costing $2.95, and a bill as follows:

15 days at	$4.00		$60 00
6 "	2.50		15 00
27 "	1.25		33 75
48 days			$108 75

would be increased by $45.60, representing 48 days × cost factor of $0.95 per day.

The variation of the factor measures the foreman's management during the past year. Its amount is the cost of facilities for doing a day's work which is chargeable to a particular job.

VIII.

COMPUTATION OF COST.

Simple case: gross cost.

Our accounts may be on so simple a scale that we shall require no more than a simple statement of the gross cost of executing a given order. To obtain this we add up the charges contained on the service and material cards found in the pigeon-hole corresponding to the order in question. This gives the net cost. This, increased by the sum of the products obtained by multiplying the number of direct day's work done on the order in question in each department by the cost factor for that department, and diminished by the sum of the credits, gives the gross cost. In such a case the cards need only contain room for the symbol S-O.; the symbols C., O. and N. being omitted. I would recommend this simple method to beginners, although I believe that all will find it to their advantage as they become familiar with the system to analyze more closely. To such the following method commends itself.

Continued analysis of cost.

Sort the service and material cards belonging to a completed order according to Plant and Work, and add together their amounts under each head. Then correct the net cost so obtained for indirect expenses as already described.

The appraised value for future uses of plant should then be charged to the most probable standing order and credited to the cost of work. The amount thus determined when divided by the output gives the factory cost per piece, lb., etc. The factory cost increased by its proportion of the selling expenses, and profit added, gives the selling price.

For example: we find the total cost of S-O. 789 corrected for indirect expenses to be—

Plant	$ 50 00
Work	175 00
Gross cost	$225 00

Suppose that by inspection of the cards we discover that no credit has been given for the contingent value of the patterns, which, let us say, is $25.00, and that they are kept in department No. 2. The omission of the foreman of No. 2, known as 201, should be supplied by making out a card as follows:

One set patterns.

CHARGE TO...2. CREDIT TO...789. AMOUNT, $25.00

This reduces the gross cost to $200.00, and increases correspondingly the liability of S-O. 2, subject to correction by inventory. (The annual inventory would correct the balance of 2, and hence affect distributively its charge for indirect expenses in future. This card should properly have been made out by 201 when the order was completed, thus clearing his mind of it, and leaving to higher authority only the task of revision.)

Detailed cost of components and of operations thereon.

If, as in staple products, the cost is needed in greater detail, we sort the cards by the object symbols, and those having like object symbols by the operation symbols, and service cards having like operation symbols by departments in which working, and those in each department by rates of wages. This being done, and the charges added together and labor increased by cost factor product, we may ascertain the most probable cost of every operation on every component. This is as far as any one would be apt to go.

Daily cost sheet.

By adding up daily the amounts in each pigeon-hole, and entering their net sum on the cost sheet, the office is kept informed how and where the money is going. The cards may then be sorted in continuous preparation for the analysis above described.

IX.

STOCK ACCOUNT.

By entering but one kind of material on each card we gain immensely in flexibility at a very small cost of trouble, for it takes but very little longer to fill, say three cards with one line each, than to

write three lines on one card, and when written the cards are independent of one another. (This applies to both service and material cards.)

This feature is particularly valuable in the accountability for Government property, which happens to be altogether by items, without regard to values. An instance of its immediate utility to private works will suffice. After the sortings, previously described, the material cards in each pigeon-hole may be re-sorted by the names of material upon them; this forms a convenient bill of material, the difference between which and even careful estimates will often prove surprising.

Space fails me to describe all the advantages following the independence of these units of record, which, like that of the printer's type, adapts them to an immense variety of uses. I have tried them in every supposable case of the affairs of an arsenal, trammeled by all the precautions imposed by a most jealous audit, and have yet to find a case in which they fail.

X.

APPLICATION.

The data for an illustrative case are derived from the analysis of the expenses of a hypothetical stove foundry which for the past two years has been the subject of discussion by the National Association of Stove Manufacturers.*

It had been estimated by one of the most experienced members of that association that the gross annual expenses of a foundry capable of turning out about 3,000 tons of a fair assortment of stoves per annum were about $321,000, divided as follows:

LABOR.

	Per ton.	Per 3,000 tons.
Moulding	$24 00	$72,000 00
Mounting	8 00	24,000 00
Pattern making	1 45	4,350 00
Pattern fitting and repairs	1 50	4,500 00
Pattern moulding	25	750 00
Carpenters	1 25	3,750 00
Cupola-men, breaking iron, etc	75	2,250 00
Cleaning and piling	2 00	6,000 00
Engineer	30	900 00
Shipping	1 00	3,000 00
General labor	2 00	6,000 00

* It is fully analyzed in the *New York Metal Worker*, February 6, and in the *Chicago Artisan* of February 6 and 13, 1886.

LABOR—*continued.*

	Per ton.	Per 3,000 tons.
Watchman	$ 25	$750 00
Foreman, moulding and melting	50	1,500 00
Clerks	50	1,500 00
Trucking	75	2,250 00
Miscellaneous and pilferings	50	1,500 00
	$45 00	$135,000 00

MATERIAL.

Foundry Costs.	Per ton.	Per 3,000 tons.
Iron	$20 00	$60,000 00
Mounting materials, not including nickel panels and rails, etc	8 00	24,000 00
Fuel for all purposes	2 75	8,250 00
Moulding sand and clay	40	1,200 00
Facing	25	750 00
Patterns, flasks and lumber material	75	2,250 00
Shipping material	10	300 00
Freight and expressage	1 25	3,750 00
Machinery and tools	1 75	5,250 00
Repairs	40	1,200 00
Gas and oil	20	600 00
Stationery and books	10	300 00
Rent	1 00	3,000 00
Insurance	40	1,200 00
Taxes	25	750 00
Miscellaneous and pilferings	40	1,200 00
Castings broken and discarded that have been paid for	1 00	3,000 00
	$39 00	$117,000 00

SELLING EXPENSES.

	Per ton.	Per 3,000 tons.
Allowances of various kinds	$1 25	$3,750 00
Attorneys' fees	25	750 00
Advertising, catalogues, etc	1 75	5,250 00
Bad debts	2 00	6,000 00
Clerks	1 60	4,800 00
Freight on stoves delivered	1 00	3,000 00
Gas and oil	10	300 00
Insurance	20	600 00
Interest	2 00	6,000 00
Discount for cash	2 50	7,500 00
Miscellaneous and pilferings	50	1,500 00
Postage, express and telegrams	1 00	3,000 00
Rent	1 00	3,000 00
Stationery	15	450 00
Traveler's wages	2 75	8,250 00

SELLING EXPENSES—*continued.*

	Per ton.	Per 3,000 tons.
Travelers' expenses and general traveling	3 25	9,750 00
Taxes	20	600 00
President and Secretary	1 50	4,500 00
	$23 00	$69,000 00

RECAPITULATION.

Total labor cost	$135,000 00
Total foundry cost material	117,000 00
Total selling expense	69,000 00
	$321,000 00

It must now appear that the essence of the system proposed is to afford means of making definite charges of expense in the following order of preference:

1. Special order, as plant or work.
2. Departmental standing order.
3. General standing order.

For the foundry let us call our departments and their standing orders as follows:

1. Pattern shop.
2. Moulding.
3. Melting.
4. Mounting.
5. Foundry, unclassified.

These comprise the manufactory proper. Now, let all the other departments be consolidated under one head,

10. The selling department.

The number of departments is limited for simplicity's sake. The more they are divided the more exact will be the resulting costs; but the more trouble will it take to keep the accounts separate.

Each of the items of expense named (pages 459–461) was distributed among the departments named, and for each item of labor the number of day's work corresponding to the amount distributed, at an assumed average rate of wages, was also stated.

Labor, which, like moulding, mounting, pattern and flask-making, is susceptible of being charged to special orders, was called direct work, and separated from that like engineers, cupola-men, and superintendence belonging to the standing orders, and a result obtained which represented, most probably, the actual results of one year's work under the system proposed.

TABLE NO. I

ANALYSIS OF LABOR CHARGES.

Department.	Direct. Chargeable to Special Shop Orders.		Indirect. Chargeable to Departmental or General Shop Orders. Standing.	
	Value.	No. of Days' Work.	Value.	No. of Days' Work.
1. Patterns	$10,500	4,500	$1,350	600
2. Moulding	72,000	24,000	8,000	6,980
3. Melting	....	...	4,050	2,270
4. Mounting	24,000	14,000	1,450	925
5. Foundry in general	....	...	6,400	3,615
Totals	$106,500	42,500	$21,250	14,390

Labor.	Value.	Days' Work.
Direct	$106,500	42,500
Indirect	21,250	14,390
	$127,750	56,890

TABLE NO. II.

ANALYSIS OF CHARGES FOR MATERIAL.

Departments.	Direct.	Indirect.
1. Patterns	$1,500	$583
2. Moulding	...	4,863
3. Melting	....	72,483
4. Mounting	20,000	5,681
5. Foundry in general	....	12,915
Totals	$21,500	$96,345

Combining Tables I. and II., we find the following total indirect charges :

TABLE NO. III.

TOTAL INDIRECT CHARGES PER ANNUM PER DEPARTMENT.

Departments.	Labor.	Material.	Total.
1. Pattern	$1,350	$483	$1,833
1. Moulding	8,000	4,863	12,863
3. Melting	4,050	72,403	76,453
4. Mounting	1,450	5,681	7,131
5. Foundry in general	6,400	12,915	19,315
Totals	$21,250	$96,345	$117,595

Expenses for shipping (labor and material), warehousemen, cartage, watchman, freight, and pilfering transferred from the foundry data, increased the sales' account to $75,405 which is about 30 per cent. of the balance of $245,595 devoted to manufacturing.

For the present we set aside the direct expenses and seek how best to apportion the indirect expenses among them. This we do by distributing the most general charges among those less so, until the cost factor for each department is obtained, as follows:

We first take the total general foundry expenses, $19,315, and divide them among 1, 2, 3, 4, according to the *total* days' work done in each department, as follows:

TABLE IV.

DISTRIBUTION OF GENERAL FOUNDRY EXPENSES AMONG DEPARTMENTS.

Departments.	Days' Work per Annum. From Table I.			Share per total d. w. of $19,315 in Table III.	Total Indirect Charges per Department, from Table III.	Gross total, Indirect, per Department.
	Direct.	Indirect.	Total.			
	d. w.	d. w.	d. w.	$	$	$
1. Patterns	4,500	600	5,100	1,850	1,833	3,683
2. Moulding	24,000	6,980	30,980	11,230	12,863	24,093
3. Melting	......	2,270	2,270	823	76,453	77,276
4. Mounting	14,000	925	14,925	5,412	7,131	12,543
Totals	42,500	10,775	53,275	19,315	98,280	117,595

Next, except for Melting, which will be treated later, we divide the gross total indirect expenses for each department by the number of days' *direct* work done in it during the past year, and get the Cost Factors per department as follows: *

1. Patterns	$0 82
2. Moulding	1 00
4. Mounting	0 90

It is supposed that under the same management the cost factors will not vary greatly from year to year. In this respect they will resemble to some extent the phenomena of life insurance; so that such variations as may be found may be attributed to causes the effects of which in future cases may be closely approached. But in gaining this experience the following discussion may serve.

The indirect expenses may be divided into two classes: those like rent, insurance, salaries, etc., which are fixed charges; and those which, like attendance, wear and waste, have a closer relation, say a direct ratio, to the number of men employed.

Calling the fixed charges for a given time F; the variable charges, V; the number of direct days' work in the same time, D; and the cost factor, either for the whole factory or for any one department, C, we have

$$C = \frac{F + V}{D}$$

If we change suddenly the number of men employed, then D will become D'; and V will become V', and

$$C' = \frac{F + V'}{D'}$$

For example, if $F = \$12,000$; $V = \$28,319$, and $D = 42,500$ d. w.

Then $C = \$0.95$.

If we double the number of men employed, on direct work only, then

$$C' = \pm \$0.80.$$

If we discharge half the force,

$$C_1 = \pm \$1.23, \text{etc.}$$

* We may simply take the quotient of the aggregate indirect expenses of the three departments by their aggregate of days' direct work ($0.95) as a gross cost factor for them all. This course will be more simple than the other, but its simplicity will be purchased at too great a cost if we lose the opportunity for keeping the foremen up to the mark by comparing the expenses of their respective managements.

In the Melting Department, as the cost of iron in good castings depends so much more upon the output than upon the number of men employed, I disregard the men and divide the cost by the weight of good castings produced, say 6,000,000 lbs.; this gives a cost per lb. (not for melted iron, as it is often called, but for good castings) of 1.288 cents per lb.; and this is taken as the cost factor of that department.

COST OF A STOVE.

Now, suppose that we have finished an order calling for 500 stoves of a special size and pattern, and that from overrunning or by direction 521 happen to be made with a lot of spare parts estimated to be worth, say, $200. Also suppose that the patterns are estimated to be worth half cost for future work. We may establish the cost per department as follows:

1. Patterns.

These have all been made by day labor, charged to the order from day to day. So has the material. We find that they have cost as follows:

Labor, direct, at average of $2.75	$1,500 00
Cost of facilities, 545 days' work at 82 cents	447 00
Value of material, estimated	513 00
Cost of patterns	$2,460 00

2. Moulding.

If this is done by the day and an account be kept, as with the patterns, the same course is followed, except that no special charge is made for materials, all of which comes out of the cost factor. But since in stove foundries, moulders work almost altogether by the piece, and owing to the great number of different parts of different stoves which they are apt to mould at the same time, it is almost impossible for them to keep their time on each order; the time may be approximated by dividing the total piece price per stove by the nearest average daily earnings. Thus, if the sum of the piece prices on the stove in question be $2.25, and the average earnings per day of moulders employed on this class of work be $3, each stove will take on the average three-fourths of a day's work to mould, and the cost of moulding may be expressed as follows:

Piece price	$2 25
Cost of facilities, viz.: 0.75 day's work at $1 per day	75
Cost of moulding each	$3 00
521 stoves at $3.00	$1,563 00

3. Melting.

Suppose that the stoves weigh 347 pounds each; 347 × 521 × 1.288 cents.......................................$2,328 00

4. Mounting.

Either of the plans described for the pattern shop or moulding floor may be followed according to circumstances; but a third case may present itself, where the mounting is done by a contractor who employs a number of men, the establishment furnishing power, tools and room, and paying the contractor by the piece. This presents special difficulties, for while we pay the contractor by the piece, he probably pays his men by the day, and makes no attempt to distribute their time, contenting himself with securing a profit on their aggregate wages.

In such a case two methods are possible. The first and most accurate requires knowledge of the average profit made by the contractor and of the average number of men he employs per day. Then the men's share of the piece price paid for mounting any stove, divided by their average daily wages, is equal to the number of days' work in mounting that stove.

For example: suppose that owing to ignorance on both sides of the actual amount of labor required to mount any particular stove, and to the concessions which in long business intercourse of this kind supply the place of competition, the prices paid the contractor are so fixed as to allow him in the long run a profit of about ten per cent. on his expenses for labor.

This will give the men about 90 per cent. of the piece price, which, when divided among them, gives, say, an average per man of $1.50 per day. Supposing the firm pays for mounting our stove $1.25; then it takes

$$\frac{90}{1.50} \text{ of } \$1.25 = 0.75 \text{ day's work to mount that stove.*}$$

The contractor's estimated profit should be charged to the general expense of mounting (S. O., No. 4), as he is virtually a foreman under a specially strong incentive to make his men work. It may seem rather inquisitorial to require the contractor to expose his pay roll; but this is justified by the circumstance that the

* (Proof. The contractor gets 521 × $1.25 =....................$651 25
Of which the men get 90 per cent., or.................. 586 12
Which, at $1.50 per day = 390.75 days' work,
Or for 521 stoves................ 0.75 day's work per stove.)

foundry furnishes the facilities which are occupied, worn and wasted more nearly in proportion to the number of men employed than to any other quantity.

Now, suppose the contractor keeps his profits to himself. We merge him with the men, and knowing, for police purposes if for no others, how many men are employed per day in a given time, the quotient of the contractor's gross receipts for that time, divided by the number of days' work done in his department during that time, gives the average cost of a day's work, which, divided into the piece price per stove, gives the day's work on that stove for mounting.

We can now sum up the cost of mounting per stove as follows:

Contract price	\$1 25
\$0.75 day's work × \$0.90 (cost factor, p. 25)	0 67
Material, per material cards, or estimated from list of material as shown by drawings	1 10
Total per stove	\$3 02
Total for 521 stoves	\$1,574 00

Omitting ornaments, nickel work, tiles, crating, etc., all of which can be charged directly, we may sum up as follows:

COST OF SHOP-ORDER, NO. 7,654, FOR 521 "O. K." STOVES.

Patterns		\$2,460 00
Moulding		1 563 00
Melting (iron in castings)		2,328 00
Mounting		1,574 00
Gross cost		\$7,925 00
Deduct ½ patterns	\$1,230 00	
Extra parts	200 00	\$1,430 00
521 stoves at \$12.46		\$6,495 00
Foundry cost, each		\$12 46
Selling expenses, at 30 per cent		3 74
Net cost		\$16 20
Profit, say at 10 per cent		1 62
Selling price		\$17 82

Let us suppose that another set of men are employed, who work so much faster that we can afford to increase their wages 50 per cent., the direct outlay for labor remaining unaltered.

The time on the job, and consequently its share of the indirect

expenses, will be $\frac{1}{3}$ less than before, and, the cost factor determined with correspondingly slow labor remaining unaltered, we shall save $395.00 in the cost of the stoves, or about 6 per cent.*

Of course, if we continue with this grade of labor for the same yearly product, our cost factor may increase; but this tendency will probably be diminished either by an increase in product for the total number of days' work per annum, or by a diminution in interest charges on invested and working capital, or by both causes and other causes also.

The example is offered to show in dollars and cents that cheap labor is not always profitable; and how the rate of work enters into the rate of wages.

It will be observed that as soon as the patterns are made and the piece prices for moulding and mounting established, the selling price of the stove may be known almost as well before it is made, as afterward. The advantage of this is apparent.

In a foundry such as has been described, the use of the service cards might be confined to pattern makers, and to a few other employees whose time is distributed among special orders.

General labor constantly engaged on standing orders might be cared for by the usual methods of time keeping, and piece workers by the means described.

Charges for material purchased might be made in bulk, on the principles set forth, from the bills received from dealers, and transfers of charges by foremen be also by values in bulk. Or charges for material entering into the cost factor might be made annually as shown by item (e) in the following statement:

	Dr.	Cr.
On hand per last inventory	a	..
Procured since last inventory	b	..
On hand per present inventory		c
Accounted for by direct charges to fabrications since last inventory, made out from drawings or specifications, p. 28		d
Balance charged to proper departmental standing order		e

Experience only could tell how much detail it would pay to neglect.

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* The saving will depend somewhat upon the departments in which an increase of earnings is allowed. The illustration supposes the increase to be uniform.

DISCUSSION.

Mr. W. E. Partridge.—I wish to direct attention to a point in Mr. Towne's paper which is usually overlooked by manufacturers. He says that in the establishment he mentions, the cost of labor has in certain instances been greatly reduced, while the earnings of the men have not been encroached upon. This is a vital point. The almost universal tendency of manufacturers is toward a reduction of wages as an easy and obvious method of reducing the cost of production. In this, as in most mechanical work, the easy and obvious method is the wrong one. Both manufacturer and workman have a common commercial interest in seeing the earnings of the employee very large.

It has been a notorious fact that in certain lines of manufactures, goods were most cheaply produced in that portion of New England where wages were highest, and were most expensive to make in certain parts of the West where wages were but one-third as large.

How to cut down the price of piecework is usually considered a problem for the manufacturer only, and one which, when solved, can only result in a reduction of earnings. This is by no means necessary. The employee may be as steadily and earnestly considering the methods for reducing the prices of piecework as the proprietor of the establishment himself. In an extensive manufactory, under the superintendence of one of our members, I believe, a system is in use which is worthy of consideration by all manufacturers and engineers. A large proportion of the men are on piecework. When one of the men devises any method or new machinery by which the time required to perform any operation can be shortened, the works at once proceed to establish a new price or schedule for that class of work, and the men receive one-half of the gain thus made. If the improved method, machine, or tool originates in the drawing-room or office the men get one-third and the establishment two-thirds of the saving. When the improved methods call for new tools, dies, "jigs," etc., they are made at the expense of the works in all cases.

Under such a system every man is as busily employed with the problem of reducing the price of piecework and the cost of production to his superintendent or employer. In this establishment it is no uncommon thing to have an application from a workman for a new and reduced price to be set on some kind of piecework,

the application being based on improved tools or methods of his own invention.

When the work of erection is done by gangs of men, the same rules are enforced. If a job is completed in less than the standard time, the overplus is paid for as overtime. When a new adjustment of work is made, the gangs get their share of the advantage. The practical application of this plan makes every man an interested overseer, and sets before him a premium for the improvement of every part of every process or operation, while it drives even the day laborer with the same spur to make every day's work as large as possible. In this way every man becomes a most efficient co-laborer with the manufacturer.

Very material advantages accrue from the employment of high-priced workmen. Large earnings employ valuable men with large producing powers. They become property owners. They are conservative and respectable members of society, and invariably throw their influence on the side of law and order. Such men can feel an intelligent interest in the success of the establishment. Under a system like that just outlined the loafer is either driven to work or fired out of the shop. Every man becomes an overseer with powers to act, which he does not fail to exercise, and he does not omit to keep up both standards of quality and quantity. He is a most vitally interested party, becoming virtually a silent partner upon whose co-operation implicit confidence may be placed.

The question is sometimes asked in this connection, How shall we keep up the standard of work as regards quality? Where machines are built, after the usual inspection is provided for, it is sufficient to guarantee the quality to the customer, and then make the erecting gang responsible for all defects in quality which they could have detected while erecting. A discoverable error in workmanship, or flaw in material, is charged to those who put the machine together. When those who erect find flaws or poor work, the last man who expended labor on the piece, and who had an opportunity to detect the trouble, sustains the loss. Every man who passes work forward becomes responsible for its quality to the man who next handles the work. This, at first sight, may appear a little unjust, but it is not so. A man, even in the inspection-room, may feel disinclined to turn a piece of poor work back upon some man with whom he is on friendly terms, but if he knows that the penalty will fall on his own shoulders, or rather his own pocket, he will not hesitate. Self-interest soon adjusts the whole matter.

Men refuse to accept poor stock to work on. Poorly finished or imperfect work is turned back at every step. Poor workmen are weeded out, and the quality once established is always kept up.

Mr. Chas. H. Fitch.—I would like to touch upon the question which is brought up in Mr. Towne's phrase, "without encroaching upon the earnings of the men engaged." It is beyond question that the ever narrowing tendency to consider and achieve wealth alone is a curse rather than a blessing. It is as truly economy to elevate the standard of manhood as it is to elevate proficiency in money-making, and it comes as near mechanical engineering. An economic section of this Society ought to consider the condition of mechanic labor and the means by which it may enjoy a more gratifying compensation. The manufacturer, however philanthrophic his disposition, is himself in the hands of iron-bound circumstances, and is often unable to do what he would in this direction. The economic section can, however, secure some concert of action and be the means of making it a matter of wholesome emulation to establish superior conditions for the performance of labor. I think that the author of the paper will certainly concur in this, that it would be laudable to set the mark a little higher than the mere avoidance of encroachment upon the earnings of the men engaged.

Mr. Jno. W. Anderson.—Mr. Metcalfe has placed the manufacturers of this country under obligations to him by the prominence which he is giving to the question of shop accounts. I can vouch for the value of the system, as I have used it with slight difference in form. While perhaps in most shops cards or order-slips would be just the thing, there are some kinds of manufacturing where, I think, books are preferable to cards for entering shop orders; that is, orders given to the several departments for work to be done. A little of my experience in this line may be useful in showing modifications of the system. About seven years ago I took charge of a large manufacturing establishment which embraced twelve different departments, each having a foreman. The plan of giving orders then in use was to write the order on a sheet with printed heading and send it to the foreman of the first department interested in the job. When he had finished his part of the work he handed it to the next foreman, and so on until the job was completed.

I soon found that in that factory there were objections to that plan. The sheets, or slips, would usually get soiled and torn, and

frequently literally worn out before reaching their destination. Sometimes they would get lost. There was no record left in the departments of the work done. With most of the orders it was necessary that the work should be in progress in several different departments at the same time. If the foremen depended on one slip, it was inconvenient and troublesome for them. If several slips were issued, it complicated the checking and filing them in the office.

After giving the matter some thought, I adopted a set of order-books to take the place of the order-slips. The books adopted were plain ruled records, with a wide margin on one side of the page. The departments were numbered from one upward.

The book containing the original orders is kept in the superintendent's office, and each department is supplied with a similar book, except in size. Each order is numbered, and the same number is u ed throughout the works. When an order is entered on the superintendent's book, he looks it over and marks on the margin the numbers of the departments that will take part in the work, as shown in Form 1.

FORM 1.

	No.
1.	
2.	
3.	
4.	
5.	

The messenger brings the department books to the office when instructed to do so, and the superintendent or his clerk enters the orders on them and returns them to the foremen.

When the foreman of a department finishes his part of an order he checks it off by recording the date on the margin. At the same time he reports it to the superintendent by signing and dating a blank like Form 2, and drops it into his letter-box.

FORM 2.

S. Manufg. Co.
....................188
Superintendent of Construction.
Sir: Order No......was completed in my department to-day.
....*Foreman.*

These reports, with other communications from the foremen, are collected by the messenger twice each day and delivered to the superintendent's office. The superintendent checks off the orders on his book as shown in Form 3.

FORM 3.

	No. 524.
2/8/84. 1.	
2.	
3.	
4.	
5.	

For example, foreman of department No. 1 reports order No. 524 completed in his department on February 8, 1884.

The superintendent turns to that order on his book and enters the date on the line numbered 1 on the margin.

At the same time he fills out a blank, like Form 4, and drops it into the letter-box, to be delivered by the messenger to the foreman who takes the job next.

FORM 4.

S. Manufg. Co.

Superintendent's Office,

........................188

Foreman, Dept. No....

Sir: Order No....is ready for your Dept.

.....................*Supt.*

Form 4 has two or three blank lines for any special instructions the superintendent may desire to give, as for example: Commence work on this order at once. Or, Give this order preference over all others, etc.

When the superintendent has checked his book in this manner from the reports of the foremen, he can see at a glance the progress each order has made, and each step toward completion has its date recorded. Should the superintendent find that an order is delayed in any department he fills out blank, Form 5, to which the foreman is bound to reply at once.

FORM 5.

S. Manufg. Co.

Superintendent's Office,

........................188

Foreman, Dept. No....

Sir: I have no report from your dept. on order No....

Please report progress.

.....................*Supt.*

By this system the time required by the superintendent to watch the progress of the work need not exceed one hour each day, even when there are a large number of orders going through the factory at one time, thus leaving a large amount of time for legiti-

mate duties, which in some shops is spent in "shooing" the orders through. Every foreman has a record of what he has done. If it is necessary or desirable for him to refer to a past order he has it before him. If anything is forgotten or mistake made the record is there to show for itself, and it is easy to fix it upon the right person.

As soon as an order is given each foreman knows his duty in the case, and he is enabled to provide for it in advance if necessary.

The system has worked so satisfactorily in the factory referred to that there has been no desire to change it.

Mr. F. W. Taylor.—I have read with very great interest Mr. Metcalfe's paper, as we at the Midvale Steel Co. have had the experience, during the past ten years, of organizing a system very similar to that of Mr. Metcalfe. The chief idea in our system, as in his, is, that the authority for doing all kinds of work should proceed from one central office to the various departments, and that there proper records should be kept of the work and reports made daily to the central office, so that the superintending department should be kept thoroughly informed as to what is taking place throughout the works, and at the same time no work could be done in the works without proper authority. The details of the system have been very largely modified as time went on, and a consecutive plan, such as Mr. Metcalfe proposed, would have been of great assistance to us in carrying out our system. There are certain points, however, in Mr. Metcalfe's plan, which I think our experience shows to be somewhat objectionable. He issues to each of the men a book, something like a check-book, containing sheets which they tear out, and return to the office after stating on them the work which they have done. We have found that any record which passes through the average workman's hands, and which he holds for any length of time, is apt either to be soiled or torn. We have, therefore, adopted the system of having our orders sent from the central office to the small offices in the various departments of the works, in each of which there is a clerk who takes charge of all orders received from, and records returned to, the central office, as well as of all records kept in the department.

The clerk or clerks in these department offices write, in all cases where it is practicable, under the direction of the foreman of the department, written orders stating what work is to be done, and how it is to be done; what order number to charge it to, and what drawings and tools are to be used, etc.

These orders are locked up in suitable bulletin-boards with glass doors in front, so that the men can see but not handle them. Each man in the shop receives from the shop clerk a note or a card for every job that he is to undertake, which refers him to the more elaborate order locked into the bulletin-board. The note which each workman receives gives him the proper authority for doing his work, and at the same time insures the concern to a certain extent against spoiled work, which so frequently results from misunderstanding verbal directions. These notes are also the means of conveying all desired information about the work to which they refer, both from the foreman and from the man who is doing the work, for keeping the records in the small offices as well as in the main office. We find that there are a great many records which it is desirable to keep close to the department in which the work is going on, for which there is comparatively little need in the central office.

For instance, it may be very desirable for each foreman to be able to place his hands at a few minutes' notice on the record of the piece of work last done, similar to that which he is about to do. For those records, of course, he could not afford the delay of sending to the main office, and it would be a very difficult matter, if they were kept there, for him to obtain the information which he desired without going himself and saying just what he wanted. If he, however, has a series of card records kept in his own office, close to where he works, and if those records are arranged, not chronologically, but on loose cards, which can be filed in such a way that the record of each job as it is finished will be placed next to that of the job which most nearly resembles it. If the records are kept on cards instead of books, the foreman can with great ease obtain any information about former jobs similar to the one he is about to start on, either in the way of mistakes made, or suggestions as to the best method of accomplishing the work, the cost, the time, or the man who did the work, etc. In our system only such information is sent to the central office as is there needed to keep them posted as to the cost and progress of the work and the men's time. While in the department office is kept much fuller information about the work, in fact everything which the foreman may find it useful to know.

Mr. W. F. Durfee.—I think this subject is one of the most important that has ever been brought to the notice of this Society, and while I fully concur in the opinion of Mr. Towne, that it is in the highest degree deserving of our consideration, I am somewhat in

doubt as to the advisability of organizing a separate section for that purpose, being fully persuaded that every engineer here who is interested in the management of works, or ever expects to be, will be a member of that section, and we should simply resolve ourselves into a "committee of the whole" to consider that subject. I think it is perfectly proper to bring it before the Society as a body, and that this discussion will demonstrate that it will have an interest for all of us.

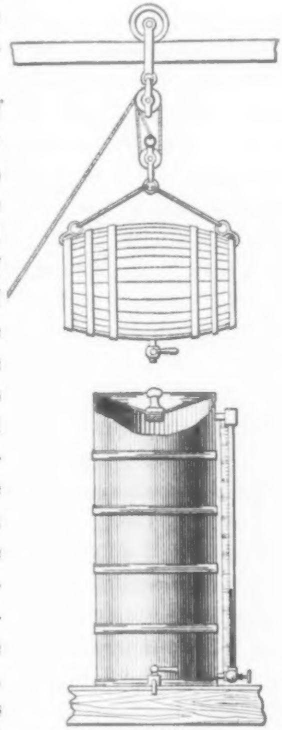

Fig.265

As an illustration of the importance of this subject, I will state some facts in my own experience. Some years ago, I was called to the supervision of a very large works, employing at times a thousand men, and I found an utter destitution of all system for determining the cost of work done. As an illustration of the state of the accounts, I would state that in the assets of the company and on its books there appeared a credit of six thousand tons of a certain kind of coal. As a matter of fact, there was no such coal on the premises. The coal at that time was worth six dollars a ton on the ground, and there was an asset that it would puzzle a bookkeeper to account for. There was no system whatever for distributing stores at the works, and no proper store-room account. One of the important items of supply was oil. The method of distributing that oil consisted in turning the barrels into an oil room in the charge of a man, and everybody who wanted oil came there and got it without reference to where it was to be used. There was no method of telling whether they carried it home, or into one department of the mill or another, or did anything with it.

This state of affairs was of course intolerable, and therefore in order to effect a radical change and to keep an accurate account of the receipt and consumption of oil, I had six cans made, each of which would hold a little more than two barrels. The illustration (Fig. 265) gives a good idea of the construction of one of the cans, the upper end being shown in section.

The upper head of each can was made in the form of an inverted cone, having a screw plug at its apex. On the right-hand side of each can was a glass gauge-tube whose scale was graduated by pouring oil into the can one gallon at a time, and marking on the scale its level in the gauge-tube, thus eliminating the effect of all errors of form or size from the scale. This work was done by the official sealer of weights and measures of the locality, and when complete he placed his official seal upon each can.

Over the range of six cans there was an over-head railway so located that a barrel of oil could be hoisted and run into either of the cans. Thus the store-keeper had a means of checking the receipts of oil. Previous to the adoption of this arrangement the oil was always paid for as per invoice. No one knew whether the barrels were full or half full, and soon after the construction of the measuring cans, their use detected a shortage of about 25 per cent. of the oil that was charged. We soon straightened that up, and though we continued to purchase of the same parties, their invoices always agreed with our receipts of oil as measured in these cans, and we had no further trouble with shortage. For distributing the oil to the several departments of the works I adopted the following system: I had a number of cards printed of different colors, each color designating a different department of the works. On these different colored cards were printed the words "One gallon," "A quart," or "A pint" of oil; a blank giving space to specify in pencil the quality of the oil wanted. The store-keeper kept an account with each department, and distributed at the beginning of the month a certain number of these tickets to the superintendent of each department of the works, and when there was any oil wanted in a department, its superintendent would send a man to buy it of the store-keeper with one of those tickets. As the oil was sold, the tickets received for it were deposited in a locked tin box, and on the first of every month this box was opened, the tickets assorted, and the value represented by each color charged up to its proper department.

One month's trial of the above described system demonstrated a saving sufficient (in the matter of oil alone) to pay all the clerical force necessary to the carrying out of a complete system of cost accounts for the whole works. One of the principal features of the system of cost accounts was this, viz.: All supplies which were consumed in the establishment were when purchased charged to store-room account, and the store-keeper kept an account with each

department of the works as rigidly as if he were a private merchant dealing with the same department. In this way waste, and what was far worse, petty thievery, was prevented and a degree of responsibility fixed upon the heads of the several departments which resulted in increased efficiency to a marked degree.

Mr. Oberlin Smith.—In giving a little discussion to the first two papers, I want to say that, like Mr. Durfee, I have some doubts about the establishment of a separate section in this Society for the consideration of economic subjects. I heartily believe in considering these things, and I suppose after awhile our Society will be so large that we may want to divide into sections as does the "British Association" and the "American Association," but whether the time has yet come to split into two as a preliminary thing to splitting into three, or four, or five eventually, I don't know; perhaps it has. If we do not do that, I hope that this subject will be agitated at future meetings—that papers will come in, and that the whole thing will be thoroughly discussed, for it needs discussion very badly indeed. The waste in ordinary shops, especially in small machine-shops throughout the country, is very great, and more than a casual observer would be apt to suspect. One thing that needs looking up a great deal more is shop organization—the officers of the shop and their relations to each other. Now, in cotton-mills there is a regular organization, just as much as there is in the army. Everybody knows his duty and his responsibility, and to whom he is responsible and what for. But in machine-shops there is no regular definite system. In some cases the foreman is the head officer outside of the office, and the gang bosses come next under him. In some cases there is a superintendent and several foremen under him. In some cases gang bosses are absent entirely. The comparison of experience in these matters, and a careful thinking out of the best organization, will no doubt result in a great deal of saving of money to the country. Whatever the Society shall decide to do about another section, they will be working in the right direction in taking cognizance of this subject.

In regard to Captain Metcalfe's paper, I am inclined to think from what I have studied of his very valuable book and of the paper, that his system is an excellent one. Of course, it needs modification to suit particular cases. There is some question in my mind whether or not the plan of using a separate ticket for every little transaction is always the best. I have no doubt that it is, in cases where each man does a good deal of one kind of work. But

in a machine-shop where there is a production of a great variety of articles, especially in a small shop where each man works on from one to ten separate jobs each day, I do not know whether it is best. I had recently a little experience in this matter myself, and found that my associates were somewhat averse to carrying out the ticket system, because in our shop, employing seventy or eighty men, each man having from one to ten jobs a day (averaging perhaps five), if separate tickets were used for all the recording of time, it averaged five tickets to each man per day, or thirty per week, which made the total very large. If there were a hundred men in the shop, there would be three thousand tickets. I have not any question in my mind that it *does* pay to handle that number of tickets, for the ease with which they can be assorted, compared with posting and rewriting in separate books; but when it comes to the amount of paper and necessary printing, I do not know about it. Of course, in the card system we only use ten tickets where a man does ten jobs, and only one where a man does one job in a day, and we waste no paper so far as that part of the ticket is concerned where the writing goes; but each one must have a certain space for a printed heading, and there must be the waste of paper due to that heading, and also the cost of the printing. If, however, you take one paper, or a larger card, for the recording of *one man's time* for a *week*, you have only one hundred such papers, instead of three thousand. A good deal of paper is of course wasted in cases where a man fills only one space out of ten provided. It is now a serious practical question in my mind which of the systems is best for my own particular case. I would like to ask Captain Metcalfe's opinion on this point—as to how great an evil is the necessary cost of extra paper and printing on the numerous tickets used where a small shop does a great variety of work and where each of the men has a great variety of jobs.

Capt. Metcalfe.—I will answer Mr. Smith by a reference to the saying about the honor which prophets receive in their own country. I have had to depend largely for experiments on what recognition my system might meet from private individuals and corporations. I got it up in the government works of which I had charge, but I have not had a full opportunity of trying it as I should like, and so cannot answer him explicitly. Of the general truth of the principles on which it is based I have no possible question. I began the trial of it at Frankford Arsenal, where we had a hundred and fifty or two hundred people. I generally had

about a hundred orders under way, of different kinds, some little jobs and some quite important ones. There, instead of the unit card proposed, we had a card with ten horizontal lines on it, allowing for the reporting of ten jobs, if necessary, one for every hour in the day. The saving of labor there was very great. I was to hire a time clerk. He had two little boys to assist him in posting the cards. This kind of card made a very great change and helped very much. But still I did not get my reports in at the end of the month as quickly as I expected. I went out West. The selfish element entered still more largely into my facilities, for I had to do almost all the work myself. I was allowed a soldier, however, and by the use of these single card tickets he did everything in about an hour a day. We did not have as many men, but I had about sixty or eighty, and this soldier did all of the sorting and all of the computing, and I had everything ready at the first day of the month, a full account of everything done the month before, the cost of every order analyzed and balanced with the pay-roll. I made a computation the other day at the Watervliet Arsenal, West Troy, where I am stationed now, but where I have had nothing whatever to do with the management of the system. That is done by an officer who is my superior in rank, and who learned it from me at the Frankford Arsenal, and who has introduced it with some modifications. The card system is not followed there. The commanding officer there does not believe in having the workmen write on cards. I found that an average of a hundred and fifty men in a great many various capacities were making cotton duck equipments, harness, canteens, straps, steel and wooden gun carriages and a great many other parts of military furnishing. I found that about $1\frac{2}{10}$ to $1\frac{3}{10}$ orders per day were worked on per man. Some of the men went up to four, or five or six jobs a day—general utility men. Others work on the same jobs steadily day after day. I am very confident in saying that anybody who tries it will be very well satisfied with the great saving and the great readiness with which any desired result can be immediately attained. I think that answers Mr. Smith's question.

Mr. Smith.—Regarding the relative amount of paper and printing in the two systems I wanted to hear, if you please.

Capt. Metcalfe.—Of course in the independent card system there would be more paper and printing. As to the statement of Mr. Taylor, who is connected, I believe, with Messrs. William Sellers &

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Co., to whom I am under many obligations, I think he somewhat confuses the order-tickets and the time-cards. The order-tickets are substantially such as he represents as being used in his works, although almost any convenient way of making the orders known to the workmen may be used. A bulletin-board will answer as well as anything, if nothing better can be found. Verbal transmission is the readiest, but of course it loses the character of a distinctive record. The order-tickets are not torn; they are simply passed out and then returned and the transaction is canceled. The only things which are torn are the labor cards, or service cards as I call them, which are torn off from the top of a book, so that, with the exception of the top one, they will always be reasonably clean. I have found no trouble with that. In my experience at the Benicia Arsenal the men kept them in little tin boxes outside their benches and filled them out as their work went on.

Then as to Mr. Anderson's remarks about the sheets getting dilapidated and the difficulty of keeping track of them; I found no trouble of that kind. I never found one of these cards to be lost. You do not lose them any more than you lose money. They are used as if to buy things with and go on from hand to hand until they get into the office, where they are all settled into their proper places. The receipt of the order ticket is indicated by each foreman's punch-marks on the duplicate retained by the superintendent, so that, having in his rack that ticket, the superintendent may see from a glance at the punch-marks upon it who has received this order, and in time that it has been completed by those whose numbers in the "completion" line he has punched out as their own cards come in completed.

All the record necessary is comprised on the original ticket.

Mr. Wm. Kent.—I have read a good deal about this subject the last six months, and also paid a great deal of attention to what has been brought before us here. No satisfactory solution of the question can be made by a desultory debate; there is so much difference of opinion as to its various details. I think it had better be referred to a committee who can report at a subsequent meeting of the Society. We might perhaps have a permanent committee on this subject instead of an economic section. I think such a committee should be composed of members who are in charge of manufacturing establishments, such as the three writers of the three papers presented to-day.

Mr. S. A. Hand.—In my business I have found it very useful

in getting at the cost of work to use a blank such as is shown below:

COST OF...

Date.	Number of hours worked during the week.	Shop expenses for the week.	Average rate of expenses per hour.	Number of hours worked on this job during the week.	Amount of wages for the week's work on this job.	Cost of the week's work on this job.
Total......						
Average...						

Cost of materials used—

Cost of machine work...

Total cost...

In the first column is placed the date of the ending of the shop week. In the second is the total number of hours worked by all hands (excepting engineer and laborers) during the week. In the third column are put the shop expenses for one week. The shop expenses for one week, divided by the total number of hours worked, gives the average rate of expenses per hour per man employed. This is put in the fourth column. In the fifth column is put the number of hours worked on "this job" during the week, and in the sixth column is put the amount of wages paid for the week's work on this job. The average rate of expenses per hour per man employed multiplied by the number of hours worked on this job during the week, and the product added to the wages paid for the week's work on this job, gives us the total cost of the week's work on this job. This is put in the seventh or last column. Below, the sheet is ruled for the totals and averages of all the columns, which will in the course of time show the percentage of full time made by the men, the average expenses per hour, the

average of wages paid to each hour worked, and the average value of worked turned out per dollar paid in wages.

Below the average space is a blank left for cost of material used in construction, which, added to the total cost of machine work, gives the total cost of the job. If a shop owner has a certain charge per hour for work he can soon tell whether that amount is paying him or not.

Mr. W. H. Doane.—I merely rise to ask the Captain to very kindly tell us how he arrived at the basis of cost; after he got the number of hours of labor how he arrived at the cost of the product.

Mr. Hawkins.—I quite agree with Mr. Towne in his advocacy of a committee to consider this subject; but I should hope that if a committee was appointed that they would not be confined strictly to the subjects of the papers as given. I am very much interested in that branch of the question which was touched upon by Mr. Partridge—making the workmen interested in the product—and my opinion is, from the experience I have had in the working of men, that there is no part of the question of economics of shop management that can begin to approach that, and if that can be extended to ordinary shops where piece work is impracticable, as it is in many cases, I think that is where we will accomplish the greatest saving that can be done by any means. We all know very well that the average mechanic, particularly such as have the care of automatic machines, planers, lathes, gear cutters, and last, though not least, the self-feeding drill, that they will waste time most abominably, and with the latter tool it is almost impossible to tell just how much they are wasting; and if you remonstrate with them, it is astonishing what tender solicitude they will have for the drill, and the dislike they have to render themselves liable to be considered as jeopardizing their future salvation in doing more than they ought to.

It is always the tendency of the average man, according to my observation, to do just as little as possible, particularly with automatic tools, and when you come to the question in a lathe or any self-feeding tool, of using one tooth on the feed-ratchet, or two, it is a very important question, and there is no part of the economics of the machine shop that can approach it in my opinion. I should like to see all that taken in by a committee.

Mr. Taylor.—I think Mr. Metcalfe has misunderstood me if he is of the opinion that I do not approve of the card system. I

thoroughly approve of the card system. We have tried it practically in our works for nearly ten years. It is simply the working out of one part of the details of his system that I do not approve of.

His suggestion is, that each workman should have a book containing ten, or twenty, or a hundred or more cards, something like a check book, and that each day he shall return one of those cards to the office punched by the foreman of the shop, and my objection was to that part of the system.

I think that the same card, the same check which he suggests as being useful for conveying the time and the work done, and the authority and so forth, to the central office, can be used to record a great variety of other facts which are exceedingly interesting and valuable. In point of fact in our works we use a great variety of time cards, which proceed in our case first from the clerk to the workman, and then from the workman back to the clerk.

We have at least, I should think, two hundred varieties of printed cards, differing according to the information desired to be conveyed from the workman to the office, all of them, however, containing to a certain extent the same information; that is, each card conveys the same information and other information besides as is recorded on Mr. Metcalfe's blanks.

My criticism was that the information conveyed by his cards was not sufficient. I fail also to see the advantage of using a punch as described by Mr. Metcalfe. The initial of the foreman, or the workman or the clerk is more rapidly made with a pen or pencil at the same time as the writing is done on the card than it can be with a punch, and it retains a certain amount of individuality.

Any one who gets hold of a punch can punch the authority for doing work of any extent or variety that he chooses, but hand-writing is much more difficult to counterfeit.

Mr. Oberlin Smith.—I thoroughly believe in Capt. Metcalfe's theory of the subject, and in his system as a whole, but I believe with the other gentlemen here that some modifications may be necessary for different shops. I confess I am a little alarmed at the two or three thousand tickets a week which I might have to use if I carried it out strictly, but I believe in a great many shops the system exactly as it stands is just the thing. There may be other shops where a modification making a card last a week instead of a day for one workman would be better. There may be cases where it would be better to put more than one job on the card, and there are cases, as I said, where the Metcalfe system could be used in its

entirety. But all of this wants looking up, and in the happy future I hope that some committee or commission (perhaps of this Society) will have a chance to devise some systems of shop organization—not *one* system only, because we cannot apply one to all kinds of shops. The shops of this country want classifying into so many classes, and the best possible kind of organization for each will be ascertained only by careful study and by the collation of the experience of a great many persons.

In regard to the dirtying of the shop cards, I do not think it amounts to anything. In the concern with which I am connected we have allowed workmen to write directly on cards for a long time. We have never had any trouble with their being lost or dirtied or too much torn for practical use.

The point that Mr. Hawkins mentioned is an exceedingly important one, and one that occurs in shop economics more often than almost anything else. I really believe that in this country the lathes, planers, shapers, and drills are not running, on an average, over half way up to the capacity that they ought to in the matter of speeds and feeds. As he says, the universal tendency of the workmen is to run at too slow a speed, being afraid that the point of the tool will grind off. In our best shops you will see tools creeping along with a cut one one-hundredth of an inch deep and one one-hundredth of an inch wide. This waste of time is utterly unnecessary, and the only limit on depth or width of cutting ought to be the strength of the work. Where the work is weak, and therefore apt to break or bend, we cannot do thus, but ordinary work that is strong enough to resist the stresses ought to be worked up to the full belt power of the machine—that is, as far as *roughing* cuts are concerned. Finishing cuts must be more delicate.

Capt. Metcalfe.—So many suggestions have been made, it is rather hard to take them all up in order, and I may omit some.

Mr. Hawkins made a point about men. That suggests to me one point which I had not dwelt upon, which is that by making a workman start a record of what work he is doing, it gives him an interest in doing it rapidly from a feeling that it will be recorded somewhere in the office.

A gentleman asked me about the cost of production. The workman has to charge his time every day to some job so as to get paid for it. He is presumably disinterested in the cost of any particular job, so he tends to put it to the most probable one. We check his record by its verification by the foreman, and we make the probability

greater by recording everything as nearly as possible at the time when it was done. I believe that the tendency will be to charge things,—in fact, I have noticed it myself,—to charge things as nearly as possible at the time when they are done. Then the record is shoved away and passed on to another person.

The general scheme is this, you stand in the center of your works, give your orders, and echoes come back to you telling what is going on. Now, these being physical items having individual numbers, to which labor and material are charged, they may be assorted in pigeon holes corresponding to the orders. They can be sorted from time to time during the progress of the work, so that, for example, the number of hours' work by operatives of the same class of wages being noted to get the cost of labor to date, you will merely have to add in those which have not been assorted. The differentiation can be carried still further, if necessary, so that by providing in the beginning for a more complete analysis, the number of days' or hours' work by each class of men on each component in that job may also be ascertained.

Mr. W. H. Doane.—My query had reference to the bases on which Capt. Metcalfe proposes to apportion the incidental expenses of an establishment.

Capt. Metcalfe.—The cost of work is made up, I believe, of the cost of labor, the cost of material, and its fair share of the incidental expenses of the establishment. I believe that the incidental expenses of an establishment should be distributed on the basis of the quantity of labor which the establishment holds, by what I call a cost factor. I find it is used in several shops, Mr. Smith's among others. I divide the incidental expenses of each department plus their fair share of the general expenses of the whole factory by the number of days' labor done in that department during the past year. That gives us, say, $1.25 per day. Rent, insurance, taxes, salaries, motive power, lighting, are all in the nature of facilities for the performance of labor. I once applied this method to the case of a stove factory, and with some satisfaction to those who heard me, I believe. If the price of iron goes up your incidental expenses do not increase. If your change of material were to be very great, say from an iron stove to a brass stove, your cost for motive power, etc., would not be any greater, so I leave material entirely out of the question and put these expenses with the cost of labor. But I distribute it according to the actual number of days' work irrespective of their cost. Poor labor

costs more to watch it than dear labor. But if you attempted to divide your incidental expenses according to the cost of labor the difference would be the other way. You would have to charge more for dear labor than for cheap labor.

That is about all. As to Home Rule in the departments, that would be a question of locality. It is not necessary that everything should be run from the centralized power.

CCX.

ANOTHER NEW STEAM ENGINE INDICATOR.

BY CHAS. W. BARNABY, SALEM, O.

THE instrument which is here considered is the result of an endeavor, on the part of the writer, to devise an indicator which will admit of having its piston placed in direct communication with the interior of the engine cylinder without the intervention of the usual pipes, cocks, etc. To be used in this manner, the indicator should meet the following conditions:

1st. The form should be such as will admit of the insertion of its cylinder into a pipe passing through the jacketing of the engine.

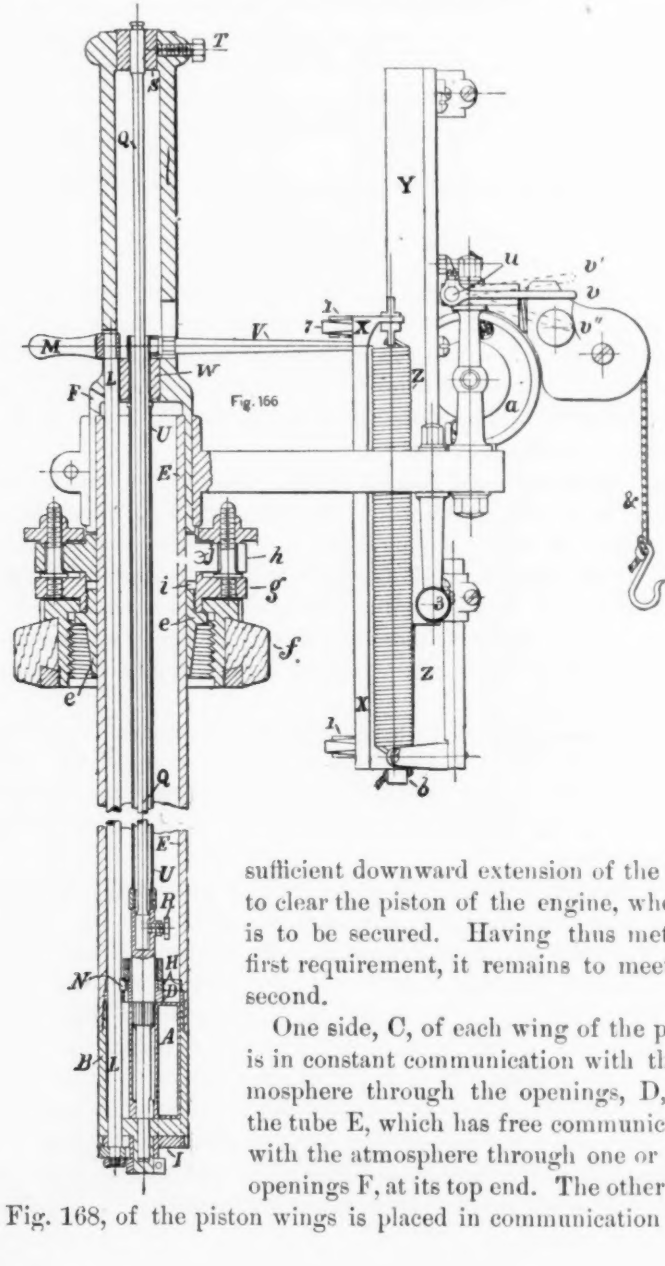
2d. One side of the piston should be in constant communication with the atmosphere, while provision should be made for putting the other side in communication with either the atmosphere or the interior of the engine cylinder at pleasure, so that the atmospheric line or the diagram of steam pressure in the engine cylinder may be traced at will. And,

3d. In complying with these conditions the instrument must, in these days of high speed engines, equal, if not excel, the several forms now in use in the direction of minimizing the disturbing effect due to the inertia of the parts which give motion to the marking point.

The most feasible form of piston which presented itself to the writer was an oscillating or vibratory one, such as is designated in Figs. 166, 168, etc., by the letter A. The cylinder B was made of such diameter ($\frac{31}{32}''$) that it, with connected tube E, would slip freely into a one-inch pipe. To apply the indicator to an engine a one-inch pipe, *c*, Fig. 170, is connected with the cylinder.

A fitting, *d*, Figs. 170 and 171, is provided of suitable form to screw on the outer end of the pipe and to receive the taper coupling sleeve and nut, *e* and *f*, Figs. 166 and 170. These latter pieces are attached to a combination stuffing-box and clamping device, *g* and *h*. A small space, *i*, is provided for packing. The gland, *h*,

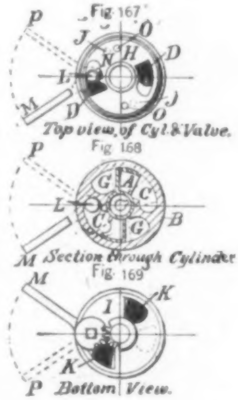
is split on one side and provided with a clamping screw, *j*. By loosening this screw the instrument may be adjusted to give just



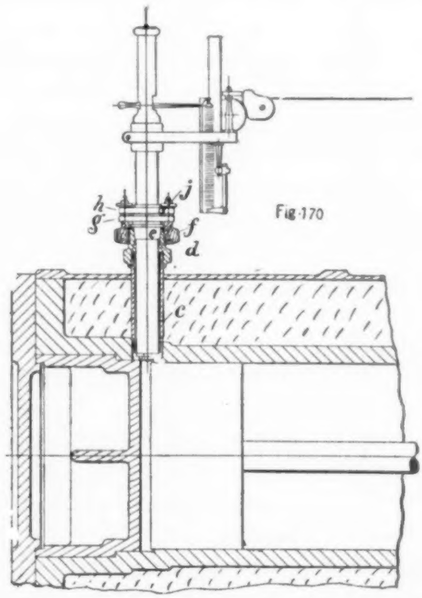
sufficient downward extension of the stem to clear the piston of the engine, where it is to be secured. Having thus met the first requirement, it remains to meet the second.

One side, C, of each wing of the piston is in constant communication with the atmosphere through the openings, D, into the tube E, which has free communication with the atmosphere through one or more openings F, at its top end. The other side, G, Fig. 168, of the piston wings is placed in communication with

either the atmosphere or the interior of the engine cylinder by the valves H and I, the former controlling the openings J at the top, and the latter the openings K at the bottom of the cylinder. A shaft, L, having a handle, M, at its upper end, operates these valves simultaneously, a small feather or arm, N, acting on the top valve so as to open and close J, as the handle, is moved back and forth between M and P, provision being made that the openings D shall not be interfered with. A segment of a gear wheel on the bottom of the shaft, in connection with a similar segment on the valve I, imparts to the latter valve sufficient movement to open and close K. The second condition is now fully complied with.



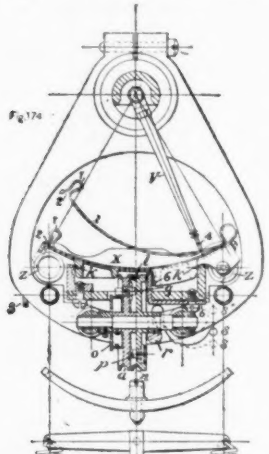
It is now obvious that the piston motion and long tubular body



are admirably adapted to the use of Prof. Sweet's torsion spring, Q, Fig. 166, which is secured in the end of the piston rod or stem by the screw R. The top of the spring is held by a split hard-

ened steel sleeve, S, which clamps the end of the spring by the action of the screw T.

The piston cannot now rotate in either direction without twisting this hardened steel rod, and the angular movement of the rod subject to torsional stress being proportional to the force applied, we will have an angular movement of the piston as the steam pressure acts on its opposite wings in the spaces G, in which equal degrees of movement will represent equal degrees of pressure. The piston motion is most conveniently carried to the upper part of the instrument by means of a light tube, U. Here the motion may be imparted to some form of parallel movement so as to use the ordinary paper drum, but the axis of the drum would in this case take a horizontal position.



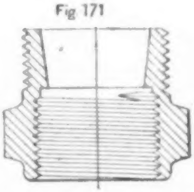
Here again Prof. Sweet comes to the rescue with his concave paper carrier. Now, it must be confessed that there are disadvantages connected with this form of paper carrier. In point of simplicity, compactness, neatness, freedom from friction, and strength for a given capacity and weight, it cannot compare with the cylindrical form; but if ever there was an indicator which, from its fundamental principles, called for a concave sliding paper carrier, this must be the one. It would seem to be a necessity, and whether or not its disadvantages are fully counterbalanced by the several advantages gained in other points will not be discussed at present.

Having adopted the concave paper carrier concentric with the cylinder, tube, spring, etc., Fig. 166 (see Fig. 174 for plan view), the parallel movement is replaced by a simple lever, V, secured to the top of the tube U. The movement of the pencil is now in perfect unison with the movement of the piston, the pencil

lever being attached rigidly to the piston without the intervention of any moving joints. The only chance for disturbance by lost motion is in the bearing, W, and this would be reproduced at the pencil point unmodified, being neither greater nor less at that point than the actual play in the bearing.

The concave paper carrier X slides in the frame Y, receiving its downward movement from the two springs Z, Fig. 166, and its upward movement through the cord &, which may pass over a grooved sheave, *a*, and be attached to the bottom of the paper carrier at *b*. This manner of attachment has been departed from in letter, but not in principle, as will be explained further on.

Fig 171



The fact that the cylinder of this instrument is capable of being inserted into a pipe through the jacket of an engine when desired does not interfere with its being used above board in the usual manner; for this purpose a fitting, Fig. 172, may be used, the lower end being threaded to ½″ pipe thread to suit the indicator attachments with which the engines are usually provided, the upper end being the same as that of Fig. 171 to couple with the indicator, the coupling device *e f*, Fig. 166, being moved to the lower end of the indicator stem.

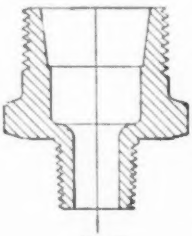


Fig.172

The question arises now as to whether, since an indicator has been designed which will accomplish the object sought, there is not more loss due to the disturbing effect of the inertia of the long tube U than there would be in the friction and inertia of the steam in indicator pipes of a reasonable length; it being obvious that in case the instrument is designed for use above board only, the long tube U can be dispensed with, and the pencil lever attached directly to the top of the piston stem, as shown in Fig. 173. This question has not, as yet, been fully determined by experiment, but it must be confessed that the probabilities would seem to be in favor of the steam rather than the tube connections with the interior of the engine cylinder.

Let us now critically examine the different members of the moving parts, from piston to pencil point. It will be noticed at once that the mass of the heaviest parts, *i. e.*, the hub of the piston, the piston stem, and the spring, is located very close to the center of motion, where the movement is very slight, while the pencil lever

and the piston wings, which are farther from the center where the movement is greater, are very light. Thus the inertia is mini-

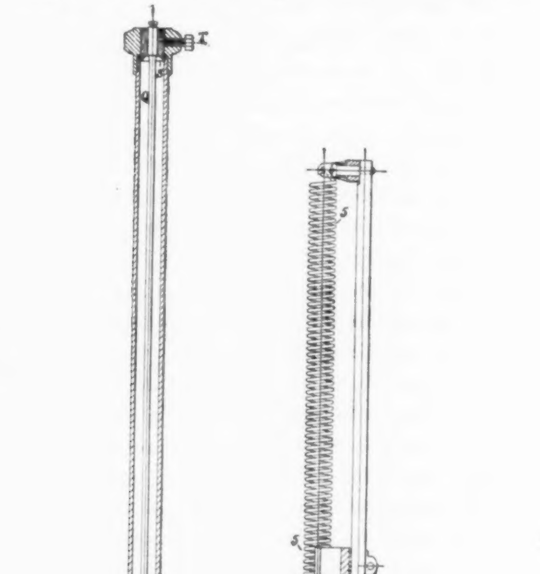


Fig. 173

Fig. 176

mized by the slight movement of the heavier parts, and by the light weight of the parts having the greatest movement.

A careful examination of the moving parts and a mental comparison between them, especially those of the form of Fig. 173, and

the moving parts of the well-known instruments, should lead us to expect better results from this combination than any now in general use. This expectation has been fully realized on putting the instrument to test.

Having determined to submit to the disadvantages of a concave paper carrier in order to gain considerable advantage in the pencil movement, it is desirable to put it in the best possible shape and overcome the disadvantages as far as possible. The friction is minimized by placing the springs Z, the guides *k*, and the operating cord *l*, Fig. 174, on an approximately straight line and as near as possible to the center of weight of the paper carrier, so as to give as little lateral strain on the guides as possible. The paper carrier is ribbed on the back, partly to give greater strength for a given weight, and partly to throw the center of gravity back as near the center line of spring and cord attachment as practicable. A sheet brass paper carrier was first tried which weighed only 4 ozs. The one now under consideration is cast brass, weighing when finished $6\frac{1}{4}$ ozs., which is much more satisfactory than the first, notwithstanding the extra weight.

The vertical movement of the paper carrier is $4\frac{1}{4}''$, while the pencil has a lateral movement of $3''$, giving a diagram capacity of about $4''$ by $3''$.

Those who have had occasion to take diagrams from high speed engines have experienced the difficulty of managing the drum cord while changing the papers on the paper drum. It is rather a difficult matter to hook and unhook the cord at high speeds, and when unhooked the end must be held or secured to keep it from being caught by some part of the engine or indicator rig. Indeed, to take diagrams from even a moderately high speed engine with any degree of satisfaction and despatch, it is generally necessary to have an assistant to manage the cord while the papers are being changed. To obviate this difficulty, an arrangement similar to that of Rosenkranz has been applied to the instrument under consideration. The cord &, Fig. 173, passes around the groove in the sheave *a*, and its end is attached to it at *m*. The hub of this sheave engages with a spring, in the barrel *o*, Figs. 174 and 175, of sufficient strength to take up the slack of the cord. By the side of this sheave on the same shaft is a second one, *n*, Figs. 174 and 175. One end of a second cord is attached to this sheave, the other end being attached to the bottom of the paper carrier at *b*. The action of the springs Z will keep the stop screw *p* in contact with the stop screw *q*, in

the auxiliary sheave *a*; so, as the cord & is drawn out and let back, the movement of the paper carrier will be the same as though the cord was attached directly to it, as was assumed in description of Fig. 166.

The paper is held in position by the spring clips, 1. When unlatched these clips spring out to the position in which one of them is shown in Fig. 174, so that the paper may readily be slipped under them. By putting the finger on the latch loop, 7, and pressing it to the paper carrier, the clip will develop its length upon the concave surface, pressing the paper closely to the carrier at all points. The latches 2 punch through the paper into holes through the paper carrier when they become engaged, holding the paper securely in place.

Instead of pressing the pencil against the paper to trace the diagram, the paper is thrown against the pencil, the guide frame Y being for this purpose pivoted at its lower end upon the pointed screws 3. About $\frac{1}{32}''$ movement at the level of the pencil is sufficient swing for the guide frame. The pencil holder 4 has a slight end movement in the pencil lever, to allow it to follow up slight wear of the pencil point, but not sufficient to allow the pencil to reach the paper when the carrier is swung back. A delicate spring inside of the pencil lever controls the pressure of the pencil point on the paper. The paper carrier is held in the position farthest away from the pencil by the spring *x* on the stud *y* in the back of the guide frame, Figs. 173 and 175, and hence the pencil comes in contact with the paper only when the carrier is swung toward it, a small arm, *w*, on the shaft *u* being provided for this purpose. This shaft is operated by the lever *v*, Figs. 166 and 175. The spring *x* coming in contact with the stop *z*, limits the movement of the paper carrier toward the pencil. This stop is adjusted to bring the carrier in line with the axis of the instrument, it being in this position when the diagram is traced.

On the shaft *u*, Fig. 175, is also an arm, *t*, which, through the medium of the weak spring *s*, operates the pawl *r*, which is placed in such position as to engage with a notch in the side of the rim of the sheave *n*. This notch is made at a point in the rim which will allow it to engage with the pawl when the paper carrier is near its uppermost position. The pawl spring *s* is so shaped as to throw the pawl into engagement when the spring is thrown to the position *s''*, Fig. 174, by the arm *t*, and to throw it out of engage-

ment when in the positions s and s'. The positions v', v, v'' of the lever v, Fig. 166, correspond respectively with the positions s', s, s'' of the spring s, Fig. 174. Hence, when the lever is in the position v the paper carrier will not be thrown in contact with the pencil or the pawl in contact with the sheave, and the carrier will thus be free to respond to the action of the cord and springs Z. When the lever is thrown to the position v' the paper carrier will be thrown in contact with the pencil, and when thrown to v'' the pawl r will be thrown into engagement with the sheave.

Suppose we now apply the indicator of Fig. 166 to an engine, as in Fig. 170, and proceed to take a diagram. The cord should be adjusted to such length as will carry the notch in the sheave just past the point of pawl engagement at the end of each out stroke. The lever M, Fig. 166, should be placed in the position P, Figs. 167 and 169, thus closing the openings K and opening the vent holes J. The lever v may be placed in the position v'', then, when the engine first passes over the out center thereafter, the pawl will drop into engagement with the sheave to which the paper carrier cord is attached, and hold the carrier at the upper point of its movement, and, as the engine runs on, the cord &c will be kept taut by the spring acting on the sheave a, which winds in the cord at each return stroke. The only effect on the paper carrier will be a slight movement or jerk at the end of each out stroke when the stop screw q is brought in contact with the stop screw p, Fig. 174.

The lever M may now be thrown to the position shown in Figs. 167 and 169, thus closing the vent holes J, and placing the piston in communication with the interior of the engine cylinder through the openings K. The pencil will now have a lateral movement, corresponding with the variations of pressure acting on the piston. The handle v is now to be thrown upward; when it reaches the central position v the pawl will fly out of engagement the next time the pressure upon it is relieved at the outmost pull of the cord, and the paper carrier being released will descend with the return stroke and continue to reciprocate in unison with the piston of the engine. Throwing the handle on upward to the position v' will bring the reciprocating paper in contact with the moving pencil, thus tracing a diagram of the pressure acting in the cylinder of the engine. The lever is now to be thrown downward to the position v'', when the pawl will be thrown against the sheave and will again

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engage with the notch and retain the carrier at its upper position. By placing the lever M in the position P, and again throwing the lever *v* up and down, the atmospheric line will be traced, the paper carrier being started and stopped as before. The diagram may now be removed and another blank paper substituted. The operations are the same when taking diagrams with the indicator of Fig. 173, except that in place of manipulating the lever M, as in Fig. 166, an indicator cock is used in the usual manner.

At the higher speeds the paper carrier is put in motion, the diagram traced, and the carrier stopped again as quickly as the lever *v* can be thrown up and down. This operation is performed without serious shock, as the pawl only acts to release and arrest the movement of the carrier at the end of the stroke, while all the reciprocating bodies are practically stationary. The cord is in full tension at the time of release, it being necessary that it should give the carrier a slight jerk or movement before the pawl can become disengaged.

The originator of the detent attachment frequently applied to indicators probably designed it to act in the same manner, but, in the hands of makers who copy the device in form without understanding the principle, it has degenerated; all those which have come under the notice of the writer being provided with springs of such force as to disengage the pawl the instant the operator throws the slide or lever which reverses the action of the pawl spring. The result is that the cord is almost always more or less slack at the time the pawl is released, and the paper drum is at the mercy of the drum spring which rotates it at an accelerating velocity until the slack is taken up with a shock which is frequently sufficient to break the cord, if the operator has not learned from sad experience to hold it until the drum has been released.

The detent spring should not have sufficient force to throw the pawl out of action until the pressure of engagement is removed by the slight pull of the cord at the out end of the stroke. With a properly constructed detent device and Rosenkranz auxiliary spring and sheave to take up the slack cord while the drum is not in motion, other indicators can be as satisfactorily operated as the one we now have under consideration, which, as already explained, admits of the paper carrier being started and stopped at pleasure, while the cord is left to take care of itself, or, rather, to be taken care of by the auxiliary sheave and spring.

There has been something done and more said in the direction of positive connections between the reciprocating parts of the engine and the paper drum or carrier of the indicator. A cord does seem like a slender thread upon which to hang the results of an instrument of precision, especially after examining a specimen of that usually sent out with the instrument by the makers, much of it being about as suitable for the purpose as india rubber.

Theoretically, however, ignoring friction and the disturbance due to the angular vibration of the connecting rod, it is possible so to design a paper drum or carrier and its operating spring or springs that it can be perfectly operated with a rubber cord, but the writer does not propose to recommend such a one for that purpose; at the same time it might be well in designing an indicator to approach as nearly as possible to the india rubber cord conditions and then use the most unyielding cord that can be obtained. Then it will be practicable to operate an indicator, even at the highest speeds, with a cord and indicator rig of ordinary proportions, as accurately as could be done with rigid connections of such rigidity as any one would be likely to attain in their construction. The convenience and popularity of the cord for this purpose make it desirable to examine into the conditions most favorable for its use and to see if they wiil admit of practical application.

The condition which will admit of the use of a rubber cord will, obviously, be that which will give a uniform strain on the cord throughout the movement. The strain being uniform the rubber cord will not vary in length, and the movement of the paper carrier will consequently coincide with that of the device from which the cord receives its motion. Indeed, when certain perfect conditions were reached, perfect regulation of the engine being one of the requirements, the cord might be removed entirely and the paper carrier would continue to reciprocate indefinitely in unison with the piston of the engine.

The movement of a paper drum or carrier being in unison with the movement of the reciprocating parts of the engine, it will obviously be subject to the same forces, and the familiar diagonal line representing the varying forces of acceleration and retardation applies also to the paper carrier, and shows what forces we have to contend with.

The weight of the paper carrier under consideration is $6\frac{1}{4}$ ozs., and the force required to reverse this weight at the end of each stroke for a 3″ diagram at 300 revolutions per minute is $1\frac{1}{2}$ lbs. Let

the line $A\ B$, Fig. 177, represent the three-inch movement of the carrier, and $A\ c$ the $1\frac{1}{2}$ lbs. on a scale of $\frac{1}{4}''$ to the pound, required to reverse it at the end of the stroke. Then the vertical distance of the line $c\ d$ from $A\ B$ will represent the forces of acceleration and retardation at the various points in the stroke or movement of the carrier.

Suppose now that the paper carrier X, Fig. 166, is at its extreme or dead center position at the downward end of its stroke, and the springs Z so adjusted as to be just relieved of their tension without hanging slack, then as the cord & is drawn out, the spring resistance, starting from zero, will increase uniformly to the upper end of the stroke and decrease along the same line on the return or

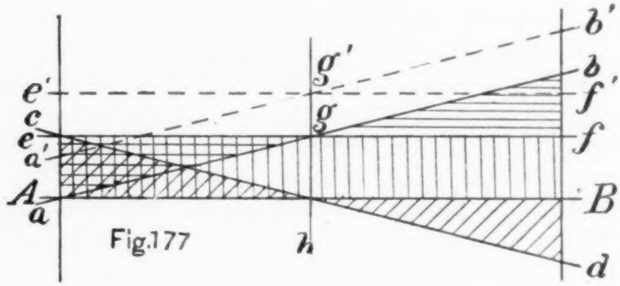


Fig. 177

down stroke. Suppose the spring to have a force of 1 lb. per inch of extension, then the resistance of the spring to the pull of the cord will be represented by the line $a\ b$ (Fig. 177), starting at zero and ending with 3 lbs. force $B\ b$; but as the cord starts outward it must start and accelerate the motion of the carrier up to the point h of maximum velocity at midstroke, hence this resistance must be added to that of the spring. This is done in the diagram by adding, above the line $a\ b$ of spring force, the diagonally shaded area $A\ h\ c$, as represented by the horizontally shaded portion $a\ g\ e$. As the carrier passes through the latter half of the stroke, the energy absorbed during the first half is given out as represented by the diagonally shaded area $d\ h\ B$, and assists the cord in overcoming the resistance of the spring, thus neutralizing the horizontally shaded portion $b\ g\ f$. Hence the tension on the cord is represented by the line of equal force $e\ f$, as shown by the vertical shading. On the return stroke the spring force acting on the cord is again represented by the line $b\ a$, but the energy $d\ h\ B$ absorbed during the first half of the return stroke and given out during the latter half,

as represented by the area $A\,h\,c$ is again subtracted at $f\,g\,b$ and added at $e\,g\,a$. The cord is therefore subject to a constant force of $1\frac{1}{2}$ lbs. on the return stroke. We now have a constant force of $1\frac{1}{2}$ lbs. acting on the cord at all points of its movement, fulfilling the conditions necessary for the use of a rubber cord. Some one may ask, what will happen if the cord is now removed? Inasmuch as the paper carrier would go straight to the lower end of the stroke and stay there, it is obvious that we are not yet ready to dispense with the cord.

Having found a proportion of carrier weight and spring force which will give a uniform tension on the card at 300 revolutions for a 3″ diagram, we also have the proper conditions for any length of diagram at that number of revolutions, as both the spring and reciprocating forces are proportioned to the length of the stroke or movement. If we put tension on the spring Z at the starting position, the tension on the cord will still be uniform, but it will be greater in degree. Suppose an initial tension of 1 lb., $A\,a'$, Fig. 177, is given to the spring, the line of spring force is represented by $a'\,b'$. This raises the line of uniform cord tension to $e'\,f'$, or to $2\frac{1}{2}$ lbs.

Let the speed now be doubled, and let us see what will be the result. We find that at 600 revolutions it will take 6 lbs. to reverse the $6\frac{1}{4}$ oz. paper carrier, or four times as great a force as is required at 300 revolutions. The line $c\,d$, Fig. 178, shows what we now have to contend with. It looks rather steep, but we will try to screw our 1 lb. per inch spring up to the work. We will try 3 lbs. initial tension which gives us the line of spring force, $a\,b$, now add $A\,h\,c$, and subtract $B\,h\,d$, as before. Instead of having a constant cord tension we now have a force $e\,f$, varying from 9 lbs. at A to 0 at B. Neither a rubber nor any other elastic cord has any business about a paper carrier under these conditions; nothing short of a good healthy steel wire should now satisfy us.

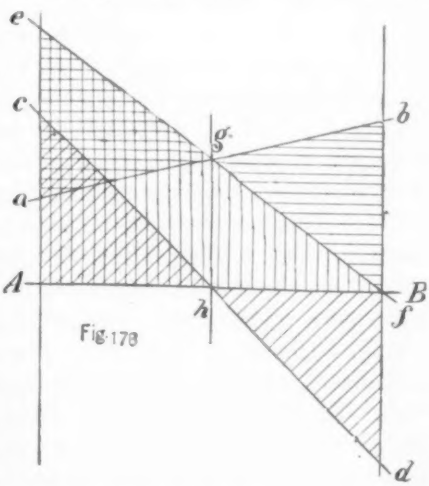


Fig. 178

It is very obvious that a spring which is right for one speed is

not correct for another, and, in order to keep up with the times, the unfortunate engineer will soon be compelled to carry around in his indicator box an assortment of springs to use with his paper drum for the various speeds, as well as an assortment to use in connection with his piston for the various pressures.

To have a constant cord tension the spring force should increase and decrease at a rate corresponding with the forces of acceleration and retardation; hence, the line *a b*, Fig. 179, should be of the same angle as *c d*. We now have a spring force of 4 lbs. per 1″, with a constant force *e f* of 6 lbs. It may be thought that 6 lbs. constant cord tension is excessive. Suppose we conclude that 1½ lbs. is sufficient. It will be noticed that the line of constant cord tension passes through the point *g* at which the line of spring force crosses the center line of the diagram. Having fixed upon a cord tension of 1½ lbs., we draw the line *e′ f′*, and also the line *a b′* through its point *g′* of intersection with the center line. We now find that we have a negative force from *a′* to *i* below the zero line and a positive force from *i* to *b′* above, with a neutral point at *i*. To secure these conditions in practice, it is necessary that the paper carrier should stand neutral in an intermediate position, and that the springs be so arranged as to resist movement in either direction, tending at all times to bring the carrier to this same neutral center. This has been accomplished by the arrangement of springs shown in Figs. 173 and 175. The lower springs Z are so adjusted that, when the paper carrier is at its extreme lower position, they still have a slight tension, enough to keep them in place. The top springs 5, shown broken off in Fig. 175, but full length in Fig. 173, are adjusted in the same manner for the extreme upper position. When the two pairs of springs are thus attached and the carrier left free to take its position, it will remain at an intermediate point, as shown in Fig. 173. It stands enough below the center to put a pretty fair tension on the cord if it is drawn out until the carrier is brought up central with the pencil lever.

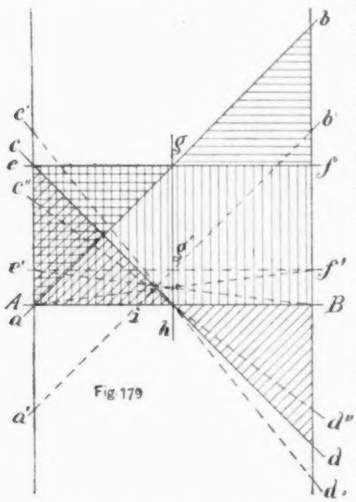


Fig 179

When attaching the instrument as above adjusted, the engine is to be placed on its out center and the cord & drawn out and secured to the indicator rig, care being taken to have the cord adjusted to such a length as will bring the notch in the sheave slightly past the retaining pawl. The lever v operating the pawl is to be thrown down and left down until the engine attains full speed, when, by throwing the lever up and down, the carrier will be started and stopped just as when the one pair of springs is used. With the double set of springs applied as above, the speed of the engine cannot be increased or diminished to any great extent. With the adjustment shown in Fig. 179 for 600 revolutions, the cord tension being $1\frac{1}{2}$ lbs. $e' f'$, the speed cannot vary more than from 525 to 675 revolutions, which varies the inertia line from $c'' d''$ to $c' d'$, and the line of cord tension from $A f'$ to $e' B$.

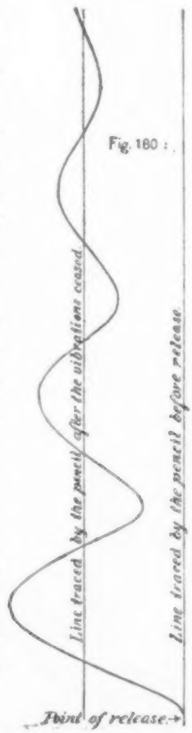


Fig. 180.

Neither end of the line of cord tension $e' f'$ must dip below the zero line $A B$, as the tension is negative below that line and the carrier "pushes on the line," so to speak, the consequence being that the cord has more or less slack throughout that portion of the movement at which the tension is negative, which is taken up with a jerk as the tension becomes positive on again crossing the zero line. This action is not allowable, as the movement of the carrier does not under these circumstances perfectly coincide throughout all parts of the stroke with that of the piston, and, furthermore, the cord is liable to be broken by the shock occasioned by the taking up of the slack.

We are now ready to consider the theoretical conditions under which the cord may be removed, and the paper carrier, under the control of properly proportioned and adjusted springs, depended upon to keep up the correct motion. Take, for instance, the case just considered in which the springs are properly proportioned and adjusted for 600 revolutions per minute; now, the reason there is a uniform tension on the cord under these conditions is that the parts and spring forces are so proportioned that the paper carrier has a natural vibration corresponding in time with the speed of the engine, so that, ignoring friction and the effect of the angular vibration of the connecting rod on the movement of

the piston, after the carrier is once made to vibrate in unison with the motion of the piston, the cord may be dispensed with and the paper carrier will continue to vibrate at the same rate indefinitely. In practice, however, the vibrations grow beautifully and quickly less, as shown in half-size in the diagram of vibrations, Fig. 180, taken by attaching a pencil to the paper carrier and allowing it to press upon a paper wrapped around a drum making 128 revolutions per minute. The circumference of the drum was 22.18″, and the distance between each double vibration 2.4″, hence the number of double vibrations per minute was equal to $22.18 \times 128 \div 2.4 = 1183$. The combination springs used in this experiment had a spring force of 19 lbs. per inch of extension. This force would produce over 1,300 vibrations if the inertia of the $6\frac{1}{4}$ ozs. paper carrier was the only resistance to be overcome, but the inertia of the springs themselves, and friction, reduce the actual number of double vibrations to 1,183, which is the number of revolutions which our engine should make per minute when the 19 lb. per inch springs are used.

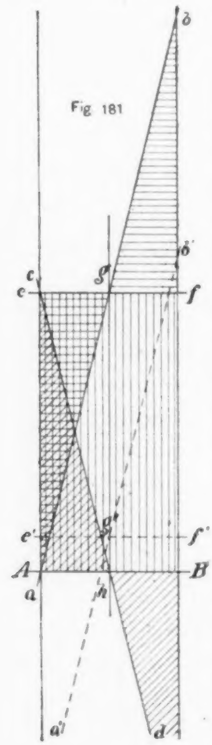


Fig. 181 is a diagram of the forces at 1,200 revolutions reduced to a smaller scale than the other diagrams. It is seen by this diagram that with an all tension spring of 16 lbs. per inch the cord has a constant tension of 24 lbs. as represented by the line *e f*, while with the combination springs acting both ways from a neutral point and represented by the dotted line *a′ b′*, it is practicable to keep the carrier in correct motion with a uniform cord tension *e′ f′* of 3 lbs. or less. In this diagram, and also Figs. 177, 178 and 179, only the inertia of the paper carrier is taken into account; the springs should, therefore, have a force somewhat in excess of what is considered in connection with these diagrams to overcome friction and their own inertia.

It may be interesting to note in this connection that the force per inch of paper carrier movement, with springs attached as in Fig. 173, is equal to the sum of the forces of the four springs; that is to say, the force per inch is the same as though all four springs were attached to pull in the same direction instead of being put in

opposition pairs. Suppose we desire a combination spring-force of 3 lbs. per inch; four springs of $\frac{3}{4}$ lbs. each; two of 1 lb. and two of $\frac{1}{2}$ lb. each; two of $1\frac{1}{4}$ lb., and two of $\frac{1}{4}$ lb. each, etc., may be used, the sum of the forces of the four springs being 3 lbs. in each case. It is preferable in connection with this instrument to have the force of the springs unequally divided, giving the lower pair a greater force per inch than the upper ones, so that the paper carrier will stand neutral at a point somewhat below the center of its movement, as shown in Fig. 173; then the constant cord tension will be equal to the force exerted by the springs when the paper carrier is held in a position central with the pencil lever.

No opportunity for taking diagrams at speeds above 450 revolutions has as yet presented itself, but the paper carrier was subjected to a pretty severe test by a crank attached to an emery wheel arbor. In this manner the carrier was operated with fair success at 1,200 revolutions, the length of its movement being $2\frac{1}{2}''$. The principal difficulty experienced was in the breakage of the cord, that on hand being of an inferior quality, and the strain put on the cord when giving the carrier the slight jerk necessary to relieve the pawl being considerable at this speed; but as long as the cord held out the pawl would catch and release the paper carrier without fail, as the lever v was thrown down and up. From 750 revolutions down it was operated with perfect success.

There is practically no limit to the speed at which the carrier can be operated, if in connection with the combination springs an indicator rig similar to Prof. Sweet's is used, the cord leading to the instrument being attached to a sliding block which is fitted to the sweeping arm in such a manner that the point of cord attachment may be raised to coincide with the axis of the arm, or dropped to a point giving the desired length of carrier movement. Stops should be provided to limit the movement of the sliding block between these two points. Gravity will keep the block in the lower position except when it is drawn up by a cord attached to it and passing through a hole through the axis of the stud which forms the pivot of the arm. The cord leading to the indicator should be adjusted to such length as will at the upper or stationary position of the sliding blocks draw the carrier slightly above its neutral position so that there may be a moderate tension. This initial tension on the cord is equal in amount to the constant strain while the carrier is in operation. The carrier will obviously remain stationary as long as the point of cord attachment coincides with the axis of

the arm, but when the block is slowly lowered, the carrier will be given a gradually increasing movement which will be as gradually decreased as the block is drawn back to the neutral point. Thus, with properly proportioned springs, speed, and carrier weight, the carrier may be put in correct motion at exceedingly high speeds by a slender cord subjected to an almost uniform tension of but a few pounds. The detent pawl should not be brought into action when the carrier is operated as above.

This subject has been entered into at considerable length, as it has frequently been stated that the speed at which an indicator can be operated is limited more by the paper drum than by the pencil movement. The foregoing is submitted as a possible, if not a practicable means of putting the paper carrying device far ahead of the point which the mechanical pencil motion is likely ever to reach. The principle is applicable to the usual drum motion as well as to a sliding paper carrier.

For all ordinary speeds the usual full-stroke tension-springs answer every purpose, but the same spring should not be used through a wide range of speeds. The spring force per inch should

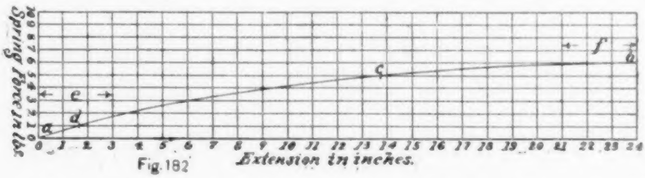
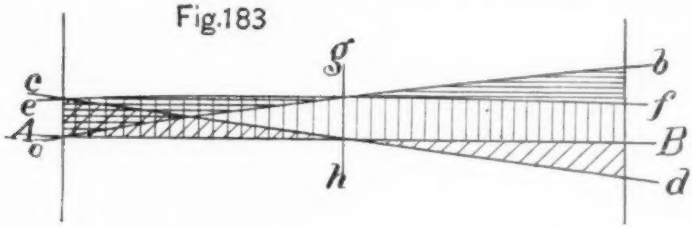


Fig. 182

increase as the square of the speed, but as it is hardly practicable to have a separate spring for each speed, and is difficult to get an adjustable spring of any considerable range, the proper conditions may be approximated with sufficient closeness by using several, each one covering a limited range of speeds. For instance, with the $6\frac{1}{4}$ oz. paper carrier, a spring force of $\frac{3}{4}$ lb. per inch of extension will be correct for 260 revolutions, but may be used for speeds from 0 to 375 revolutions without deviating more than $\frac{3}{4}$ of a pound per inch from the spring force theoretically required. In like manner a $2\frac{1}{4}$ lb. spring may be used at speeds from 375 to 525 revolutions with only $\frac{3}{4}$ lb. deviation: a 4 lb. spring at speeds from 525 to 675 revolutions with only 1 lb. deviation. From 675 to 850 revolutions, a $6\frac{1}{2}$ lb. spring may be used with $1\frac{1}{2}$ lb. deviation, but for these speeds, and especially for higher ones, the combination springs are desirable. One objection to the full stroke tension springs at high speeds is the difficulty of devising a stop, 6, Figs.

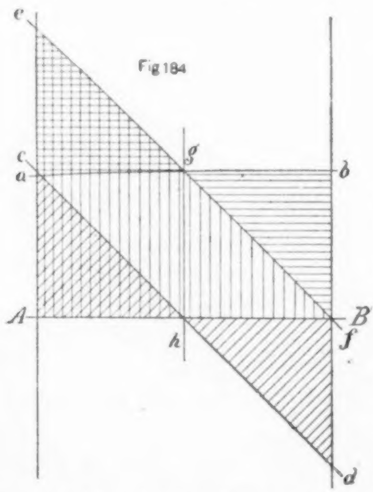
173 and 175, through which the paper carrier will not go if the cord breaks when the carrier is toward the top of its movement.

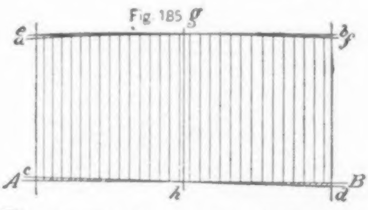
The clock spring, ordinarily used in connection with indicator drums, is very unsuitable for that purpose. Fig. 182 is a diagram of the force and extension of a drum spring of this form, the ex-



tension being in inches of cord unwound from a 2" paper drum. The curve *a c b*, representing the spring force and the corresponding extension at the various points, shows that the spring force is far from proportional to the extension, the first pound only producing 1.7" extension from *a* to *d*, while the sixth produces an extension of over 10" from *c* to *b*.

We will now apply this spring to a 3" length of diagram taken with the lowest spring tension possible, as represented by *e*, Fig. 182. This curve (*e*) is reproduced full length at *a b*, Fig. 183.





The inertia line *c d* is that which gives the line *e f* of approximately uniform tension on the cord. This line, retaining the $6\frac{1}{4}$ oz. for the weight of the drum, corresponds to a speed of 230 revolutions per minute. Suppose we desire to try 600 revolutions. The line *c d*, Fig. 184, represents on a half-scale the effect of the inertia of the drum. It is now necessary to put the tension of the spring up to the extreme limit, using the part *f*, Fig. 182. This line is represented in Fig. 184 by *a b*.

The line $e f$ shows the character of the cord force, a variation from $11\frac{3}{4}$ lbs. at one end of the stroke to 0 at the other. The spring force is evidently not at all suitable for this speed, and so we will see to what speed it is adapted. The line $a\ b$ in Fig. 185 is a reproduction of the line $a\ b$ in preceding figure. We now find that the inertia line must be lowered to $c\ d$, which represents the accelerating and retarding forces due to about 110 revolutions. This

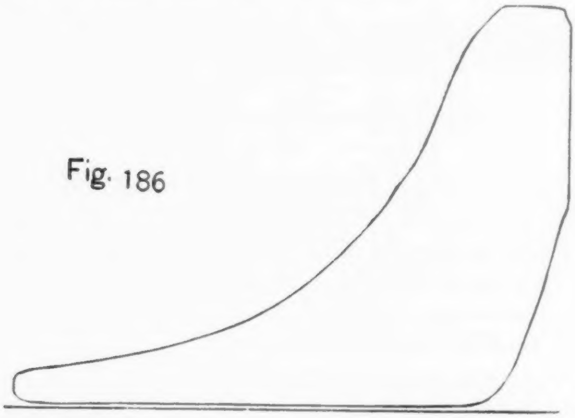


Fig. 186

Fig 187

demonstrates that the speed must be *reduced* instead of *increased* as additional tension is put on the spring, if we have any regard for the tension on the cord.

The practical utility of the instrument presented has not as yet been demonstrated by very extensive use, but whatever disadvantages are apparent on inspection or may be developed by future experience there would seem to be a few redeeming points. On referring to Fig. 173, it will be noticed that the spring Q is for the

greater part so far removed from the cylinder that the heat can affect it but slightly. To remove the spring it is only necessary to loosen the screws T and R, which secure its ends, both screws being accessible without removing or disturbing any other part of the instrument. The spring is lifted out through the bush at the top, when another may be dropped into place and secured. The greater portion of the tube 7 being so far removed from the steam is always cool, and forms a convenient handle by which to hold the in-

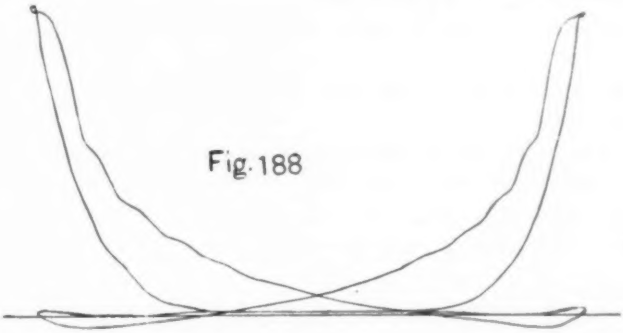


Fig. 188

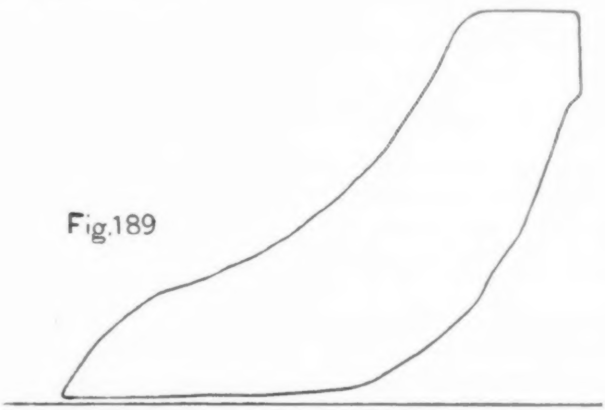


Fig. 189

strument while connecting or disconnecting it when it is hot. With the tube for a handle and the wood-covered coupling f to connect or disconnect the instrument, it may be handled without any inconvenience from the heat. The top end of the spring being cylindrical where clamped, the pencil lever (see Fig. 174) may be set in any initial position desired. If an engine with little or no vacuum is to be indicated, the pencil may be placed close to the edge of the paper carrier before clamping the spring, so as to use the full

capacity of the instrument above the atmospheric line. The velocity ratio of piston and pencil is necessarily constant, which is not the case practically or theoretically with the parallel movements in general use. The error due to the disturbing effect of the inertia of the moving parts from piston to pencil-point is reduced to a minimum by their reduced movement and weight. With the exception of the inaccessibility of the screw *R*, and that a greater portion of the spring is exposed to the steam, the above applies also to the instrument of Fig. 166. To change the springs in this instrument it is necessary to screw the pencil lever out of its socket at the top of the tube *U*, and to loosen the screw *T* at the top of the instrument, when the spring may be drawn out through the top, bringing with it the bushes *S* and *W*, the tube *U*, and the piston stem. The screw *R* may then be loosened and the spring replaced by the one desired, when the parts may be returned to place.

A few diagrams, taken with the instrument of Fig. 173, are submitted without comment for what they are worth. Fig. 186 is from a 16″ × 36″ Cummer, 100 revs., 36 lb. scale; Fig. 187 a 4″ × 5″ single valve automatic, 400 revs., 36 lb. scale; Fig. 188 a 10″ × 14″ Buckeye, 244 revs., 39½ lb. scale; and Fig. 189 an 8″ × 10″ Payne, 275 revs., 36 lb. scale.

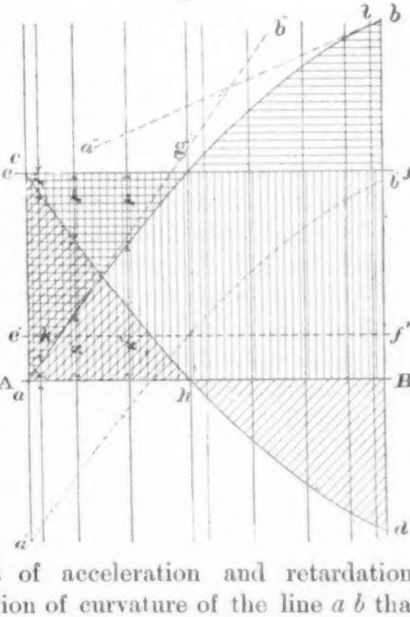
This new indicator, in common with most new inventions, is principally old. Several features were known to be old when adopted, others were supposed to be new, but that irrepressible previous fellow has already laid claim to several of them, and it may be but a question of time until all of the remaining features will have been eliminated which were supposed to be new.

APPENDIX V.*

Since writing the foregoing the writer has endeavored to obtain in the spring forces acting on the paper carrier, a reproduction of the forces of acceleration and retardation, as modified by the disturbance due to the angular vibration of the connecting rod.

Supposing the engine to be indicated to have a connecting rod length of six cranks, other conditions remaining the same as represented in Fig. 179, *i. e.*, speed, 600 revolutions per minute; weight of paper carrier, $6\frac{1}{4}$ oz.; travel, 3″, we have from ordinates given in Porter's Steam Engine Indicator, the line $c\,d$, Fig. 253, representing the forces of acceleration and retardation throughout the movement of the carrier. The problem before us is to obtain a spring force which will follow the line $a\,b$ so as to give the line $e\,f$ of constant cord tension. To accomplish this the ordinates s, s', s'', etc., from the line $A\,B$ to the line of spring force must be made to equal the ordinates r, r', r'', etc., from the line $e\,f$ to the line $c\,d$, representing the forces of acceleration and retardation. It is obvious from the direction of curvature of the line $a\,b$ that the spring force per inch of carrier movement should decrease as the stroke progresses from A to B. If the force continued throughout the stroke in accordance with the increase from a to k it would follow the line $a\,b''$, equal to about 5.65 lbs. per inch; while at the latter end of the stroke from l to b the line of spring force takes the direction of $a''\,b$, equal to about 1.75 lbs. per inch of carrier movement. Thus it is seen that the spring force must vary from over 5.65 lbs. per inch of carrier movement at the commencement of the stroke to less than 1.75 lbs. at the end. Follow-

Fig. 253

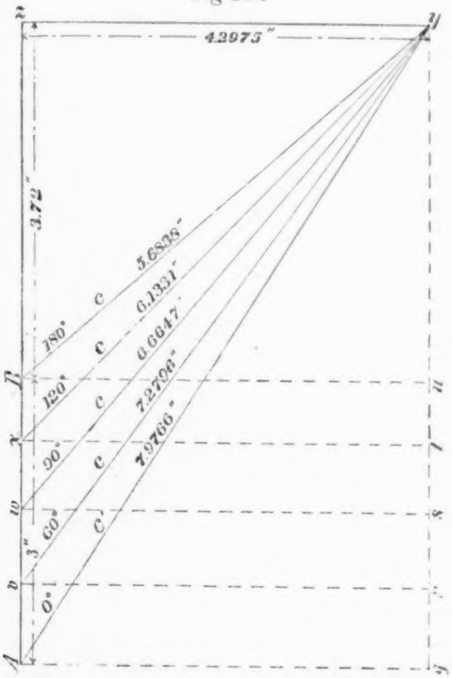


* Contributed since adjournment of the meeting.

ing out the first idea which presented itself, the following solution of the problem was obtained :

Referring to Fig. 254, let *A B* represent the 3″ movement of the carrier. Now, instead of attaching the stationary end of the spring at a point *Z* in line with the direction of movement, let it be attached at a point *y* some distance to one side. Suppose the carrier to stand at the lower point *A* of its stroke, and this line *A y* to represent a compression spring standing neutral at this point, but brought into compression the instant the carrier starts on the upward movement toward *B*. It is clear that the compression of the spring per unit of carrier movement near the lower end *A* is greater than at the upper end *B*. This is all the more obvious if we imagine the carrier to be moved on upward past *B*, when it will be seen that as it approaches *z* considerable movement along the line *Az* produces but little compression on the spring.

Fig-254



The spring forces acting on the lines *c′*, *c*, etc., when the carrier is at the various points *A*, *v*, *w*, etc., may each be supposed to be divided into two component forces, *A z* and *A y*, *v z* and *v r*, *w z* and *w s*, etc., the one acting in the direction of motion and the other at a right angle to it, and therefore neutral, so far as producing motion is concerned. It has been ascertained that the distances *B z*, *z y*, *B y*, etc., Fig. 254, are the ones which give a very close approximation to the results required.

Referring to Fig. 253, it will be seen that the spring force at the upper end *B* of the carrier's movement is 12 lbs., *B b*, the scale being $\frac{1}{8}''$ to the pound. Hence the force exerted by the spring, on the line *B y*, Fig. 254, must be such as will give a force of 12

lbs. in the direction Bz. By an application of the principle of the parallelogram of forces it is determined that the spring force acting on By should be 18.335 lbs., which gives the required 12 lbs. force in the direction zB, and a force of 13.8629 lbs. in the direction uB.

Table I. gives in column c the lengths of the lines c', c, etc., Fig. 254, for each 10° revolution of the crank during the out-stroke of the engine. Column Z gives the amount of compression of the spring at the same periods, and likewise f gives the spring forces due to the compression Z, and acting in the direction of the axis of the spring; U, the amount of movement of the carrier from A toward B; V, the ordinates of the curve representing the forces of acceleration and retardation. In column W, for comparison with V, are given the effective spring forces acting in the direction of motion. Column V gives the ordinates r, r', r'', etc., of Fig. 253, and column W the ordinates s, s', s'', etc. It will be seen on comparing the figures in the two columns that even as they stand they run near enough parallel for all practical purposes, and there is little doubt but that if both columns were calculated with greater accuracy by carrying the figures out to a greater number of decimals, they would correspond exactly.

The closeness with which these columns correspond is more clearly seen by referring to their differences given in column m. Column n gives the distances zA, zv, zW, etc., Fig. 254.

TABLE I.

COMPRESSION SPRINGS.

Degrees.	Carrier movement, Out-stroke. (A to B)	$b + (3 - U)$ ($b = 3.72$).	$\sqrt{n^2 + a^2}$ ($a = 4.2975$).	Compression of Springs. $= c' - c$	Spring force on lines c. $= Z \times 7.997$	Effective spring force in direction of carrier movement. $\frac{fn}{c}$	Forces of acceleration and retardation.	$W - V$.
	U	n	c	Z	f	W	V	m
0	.0000000	6.7200000	7.9766	.000000	.00000	.00000	.00000	.00000
10	.0265584	6.6934416	7.9542	.0224	.17928	.15085	.1470	.00385
20	.1050954	6.6149046	7.8882	.0884	.70763	.59332	.5934	−.0001
30	.232266	6.487734	7.7820	.1946	1.5560	1.2971	1.2942	.0029
40	.4027299	6.3172701	7.6405	.3361	2.6887	2.2230	2.2182	.0048
50	.6094732	6.1105268	7.4703	.5063	4.0483	3.3111	3.3096	.0015
60	.8442436	5 8757564	7.2796	.6970	5.5736	4.4982	4.4994	−.0014
70	1.0980327	5.6219673	7.0763	.9003	7.1993	5.7196	5.7186	.0010
80	1.3615861	5.3584139	6.8688	1.1078	8.8596	6.9108	6.9066	.0042
90	1.6258807	5.0941193	6.6647	1.3119	10.491	8.0184	8.0154	.0030
100	1.8825307	4.8374693	6.4707	1.5059	12.043	9.0024	8.9922	.0102

TABLE I.—COMPRESSION SPRINGS—*Continued.*

Degrees.	Carrier movement, Out-stroke. (A to B) U	$b + (3 - U)$ ($b = 3.72$). n	$\sqrt{n^2 + a^2}$ ($a = 4.2975$). c	Compression of Springs. $= c' - c$ Z	Spring force on lines c. $= Z \times 7.997$ f	Effective spring force in direction of carrier movement. $\frac{fn}{c}$ W	Forces of acceleration and retardation. V	$W - V$. m
110	2.1240933	4.5959067	6.2921	1.6845	13.471	9.8378	9.8238	.0140
120	2.3442435	5.3757565	6.1331	1.8435	14.740	10.515	10.4994	.0156
130	2.5378360	4.1821640	5.9966	1.9800	15.801	11.045	11.0250	0200
140	2.7008634	4.0191366	5.8840	2.0926	16.734	11.430	11.4156	.0144
150	2.8303428	3.8896572	5.7964	2.18025	17.436	11.698	11.6892	.0088
160	2.9241733	3.7958267	5.7339	2.2427	17.934	11.899	11.868	.0010
170	2.9809819	3.7390181	5.6964	2 2802	18.236	11.970	11.9688	.0012
180	3.0000000	3.7200000	5.6838	2.2978	18.335	12.000	12.000	.0000

TABLE II.

EXTENSION SPRINGS.

Degrees.	Carrier movement, In-stroke. (B to A) U	$b + U$ ($b = 2.1975$). n	$\sqrt{n^2 + a^2}$ ($a = 4.23$). c	Extension of Springs. 180° 170° $= c_1c$ & $c - c0°$ Z	Spring force on lines c. $Z \times 7.9985$ f	Effective spring force in direction of carrier movement. $\frac{fn}{c}$ W	Forces of acceleration and retardation. V	$W - V$ m
0	.0000000	2.1975000	4.7667	.00000	.000000	.000000	.000000	.00000
10	.0190181	2.2165181	4.7754	.00867	.069381	.032203	.0312	.001003
20	.0758267	2.2733267	4.8021	.03540	.28264	.13380	.1320	.00180
30	.1696572	2.3671572	4.8473	.08057	.64452	.31475	.3108	.00395
40	.2991366	2.4966366	4.9118	.14508	1.1604	.58985	.5844	.00545
50	.4621640	2.6596640	4.9967	.2300	1.8392	1.0018	.9750	.0268
60	.655757	2.853357	5.1023	.33568	2.6850	1.5014	1.5006	.0008
70	.8759067	3.0734067	5.2285	.46177	3.6934	2.17106	2.1662	.00486
80	1.1174693	3.3149693	5.3741	.60746	4.8588	2.9971	3.0078	—.0107
90	1.3741193	3.5716193	5.5361	.70934	6.1536	3.96995	3.9846	—.0146
100	1.6384139	3.8359139	5.7101	.94345	7.5463	5.0694	5.0934	—.0240
110	1.9019673	4.0994673	5.8904	1.1237	8.9905	6.2569	6.2814	—.0245
120	2.1557564	4.3532564	6.0699	1.3037	10.424	7.4760	7.5006	—.02359
130	2.3905268	4.5880268	6.2404	1.4737	11.787	8.6656	8.6904	—.0248
140	2.5972701	4.7947701	6.3940	1.6273	13.015	9.7595	9.7818	—.0223
150	2.7677340	4.9652340	6.5227	1.7560	14.045	10.6915	10.7050	—.0035
160	2.8949046	5.0924046	6.6199	1.8532	14.823	11.403	11.4066	—.0036
170	2.9734416	5.1709416	6.6807	1.9140	15.308	11.849	11.8530	—.0013
180	3.0000000	5.1975000	6.7011	1.9344	15.471	12.000	12.0000	—.0000

Fig. 255 represents one application of the above principle to a paper carrier. Two springs of like forces and dimensions are used, one inclined to the left and the other to the right. Thus the

force which each spring exerts in a direction *perpendicular* to the line of carrier motion is neutralized by that of the other, while the two springs act together in the direction of motion. Two rods, *a g* and *d h*, are pivoted to the carrier *a b c d* at *e* and *f*, the other ends passing through guiding sleeves pivoted at *h* and *g*; one end of each spring is fastened to one of the guiding sleeves, the other end being attached to the rods at *k* and *l*. When the carrier is drawn to the top of its movement, the pivots *e f* are in the positions *i j*, and the springs are in the position indicated by the dotted lines.

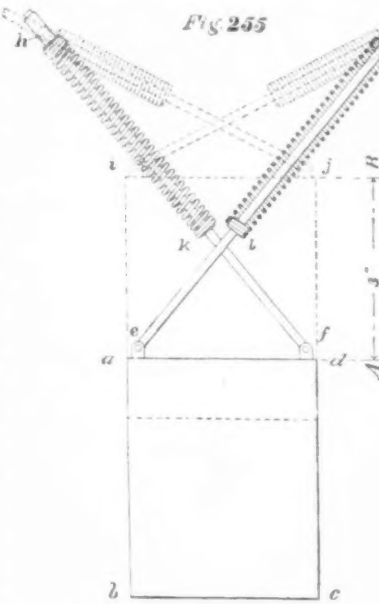


Fig 255

Thus far single acting springs have been considered which, in accordance with the usual practice, are applied so that the pull of the cord throughout the out stroke gives the carrier its upward movement, and at the same time stores in the compressed springs the force to give it its downward movement. These springs correspond with the springs *Z* of Figs. 166, 173 and 175, but instead of being attached below the carrier and acting in extension, they are attached above and act in compression.

We are now ready to consider substitutes for the upper springs, 5, Figs. 173 and 175, in which, as in the preceding case, the spring force is modified to correspond with the inertia curve. When in their neutral position these springs must place the carrier at its uppermost position, as indicated by the dotted lines *i g*, *h j*, Fig. 255, so as to act in extension as the carrier moves downward. As this is the manner in which springs would have to operate in case the paper carrier was to be operated by pulling the cord on the *in* instead of the *out* stroke, we will consider for the moment that the cord acts in that manner. In this case the movement due to the pull of the cord will be from *B* to *A*, Fig. 256, the line *c d* representing the action of the inertia forces, and *a b* the path which

the spring force must follow to give the constant cord tension along

Fig 256

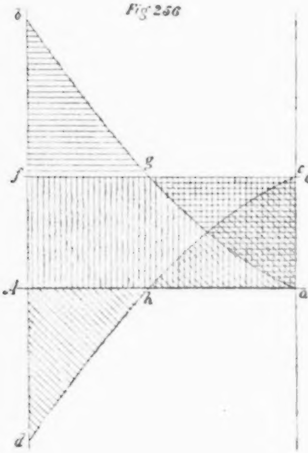
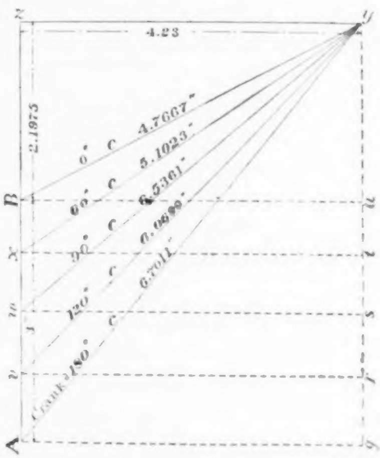
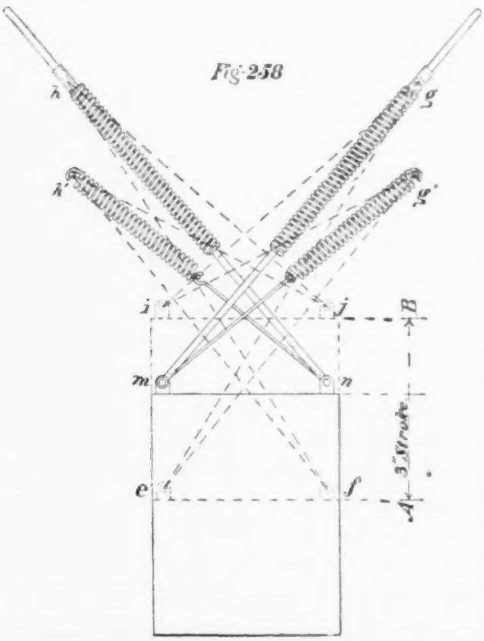


Fig 257



the line ef. Springs starting neutral at B, Fig. 255, and placed in extension as the carrier moves downward, require a different location of the point y from that required in the first case when they started neutral at A and acted in compression as the carrier moved upward, as will be seen on comparing Fig. 257 and Table II. with Fig. 254 and Table I.

Fig 258



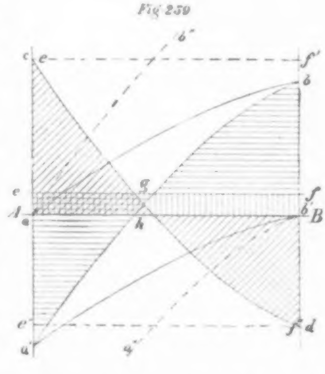
Having now secured in each of these two sets of springs an action corresponding to the inertia forces as modified by the angular vibration of the connecting rod, we are prepared to substitute for Z and 5, Figs. 173 and 175, springs paired

against each other between which the carrier may be hung as in that case, and under whose action the carrier will, when put in motion, have a natural vibration corresponding in every part of its movement to that of a reciprocating body which receives its motion through a connecting rod of six cranks' length, from a crank revolving uniformly on its axis.

Fig. 258 shows the manner in which the springs may be attached. Under the action of the lower or extension springs alone the carrier would be held at its uppermost position, while with the upper or compression springs it would be held at the lowest position. Under the influence of both it will take an intermediate position as shown, and if drawn to either *A* or *B* and released, it will vibrate between these points.

The action of the springs under these conditions is represented by Fig. 259, the positive* forces in this, as in all previous diagrams, being above the line *A B* and the negative below. Let the conditions of speed, etc., be the same as represented by Fig. 253, then *c d*, Fig. 259, will represent the forces of acceleration and retardation. The force per unit of movement of these springs is the sum of the forces of the springs, the same as explained in relation to Fig. 173. So we will employ springs of one-half the strength represented in Figs. 253 and 256.

Fig 259



Starting on the out stroke from *A*, Fig. 259, we have the line *a b*, representing the positive force due to the compression spring, commencing with 0 lbs. and ending with 6 lbs., *B b* at *B*, and *a' b'*, the negative force due to the extension spring starting with—6 lbs., *Aa'* at *A* and ending with 0 lbs. at *B*. By adding these two forces together, which may be done graphically by dropping from the line *a b* the ordinates of the curve *a' b'* measured from the line *A B*, we have as a result of the combination of the spring forces, the resultant force *a' b*.

We now have at the beginning of the out stroke a negative spring force pushing on the cord, as it were, whose effect is repre-

* The term positive is used in this connection in relation to those forces which act in a direction to pull on the cord by which the carrier is operated, and the term negative applies to those which act in a contrary direction.

sented by the area $a' h A$ and which neutralizes as much of the energy required to start and accelerate the carrier as is represented by the equal area $c g e$, and throughout the latter part of the stroke, due to the retardation of the carrier, a negative inertia force, whose effect is represented by the area $h d B$, which neutralizes as much of the spring force at that end of the stroke as is represented by the equal area $b g f$, thus leaving a constant force acting on the cord along $e f$. During the return stroke the forces all follow the same lines, as regards the effect on the cord, and we consequently have a constant cord tension of about 1 lb. throughout both strokes.

By substituting compression springs of greater and extension springs of correspondingly less strength than above, the inertia curve may be raised to any point between $a' b$ and $a b''$, bringing the line of cord tension at any position from $e f$ and $e' f'$. On the other hand, by reducing the compression springs and increasing the extension springs in the same manner, the inertia curve $a' b$ may be dropped as low as $a'' b'$, bringing the line of cord tension at any desired position from $e f$ to $e'' f''$. When the line $e f$ lies below $A B$, however, the cord tension becomes negative, so that, practically, the carrier could only be operated under such conditions by attaching the cord so as to pull on the *in* instead of the *out* stroke, thus making the tension positive.

Although calculations have not been completed for connecting rods of other proportions, the results as far as obtained seem to indicate that with a slight variation in the location of the point y, Figs. 254 and 257, and the corresponding variations in the lengths of $B z$, $z y$, $B y$, etc., the desired results can be arrived at as closely under other conditions as in the example presented.

DISCUSSION.

Mr. John Walker.—I would like to inquire of Mr. Barnaby, how a 6¼ ounce paper carrier answers better than a four ounce paper carrier?

Mr. Barnaby.—I did not mean that a heavy carrier was better. I meant that of the two tried, the heavier proved to be the more satisfactory. The gain was not in the additional weight obtained, but in the greater rigidity secured by the slight additional weight. The 4 oz. carrier was made of a single piece of sheet brass with the four edges turned over for flanges; the 6¼ oz. carrier was a brass

casting ribbed on the back. These carriers are in the instruments exhibited. With the sheet-brass carrier it is impossible to get all of the lost motion out of the guides without binding the carrier at points, as it is not sufficiently rigid to maintain its shape. The cast one is much stiffer, having, say two or three hundred per cent. greater rigidity, and only fifty per cent. greater weight, allowing a very close adjustment of the guides without binding.

The Secretary.—In the discussion of this indicator of Mr. Barnaby's, I have a letter here which was put into my hands by Mr. Charles T. Porter, in which reference is made to the fact that in the discussion on November 17, 1885, following the reading of papers on the Steam Engine Indicator, before the Institution of Civil Engineers of London, Eng., by Messrs. Reynolds and Brightmore, Mr. J. G. Mair, in explanation of the effect of the inertia and spring forces on the cord, introduces a diagram almost identical with those used by Mr. Barnaby in this paper. As those who have seen the English papers and discussion referred to would naturally infer from the fact that Mr. Barnaby's paper comes out some months later, that he had got his idea from that source, Mr. Porter sends this letter to bring out the fact that Mr. Barnaby's paper was really put into the Society's hands early in October, 1885, and that his researches in that direction extend back to a much earlier date ; and, as the idea of using drum springs accurately proportioned to the inertia appears to be original with Mr. Barnaby, it seems to be one of those cases where more than one person has hit upon the same idea independently.

*Mr. Geo. H. Barrus.**—I have not had an opportunity to compare the new indictor which Mr. Barnaby has brought out, with those already in use, and cannot, therefore, speak of it intelligently.

There are some features which appear worthy of commendation, and others which may be criticised adversely. These may be referred to.

The use of a torsion rod in place of a spring with rotary instead of rectilinear motion of piston, should secure a reduced amount of reciprocation, less weight of moving parts, and consequently less momentum upon the pencil mechanism. In this feature, the new instrument compares favorably with the best of those heretofore used.

The location of the piston at a point much nearer the cylinder than is common in the standard form hitherto used may be desira-

* Contributed since adjournment of the meeting.

ble, but I do not think much is gained by this change. There can be little reduction of pressure due to the steam passing through the form of indicator cock and short pipe connection ordinarily used in good practice.

The mechanism for carrying the paper appears to me clumsy and objectionable. Nothing can be simpler or better than the light, rotating paper drum with which every one is familiar.

CCXI.

THE PRODUCTION OF TRUE CRANK SHAFTS AND BEARINGS.

BY HORACE SEE, PHILADELPHIA, PA.

It has been said, "of all parts of marine engines that which requires to be renewed most frequently is the crank shaft." *

This is no doubt principally due to the shaft being injured or broken by the strains † which come from either the want of truth in itself, or from the bearings being out of line.

In order to eliminate these strains, and leave none but what are derived directly from the piston, a standard of workmanship has been adopted which excludes all measurable errors.

To effect the desired improvement, it is proposed to use a cylindrical truing mandrel or face plate, if it may be so called, of a length somewhat greater than that over the extreme ends of the main bearings, and of the same diameter as the journals of the shaft. In addition to this, for the built-up shaft, there is a set of special bearings corresponding in number and diameter with the journals of the crank shaft, and secured, with the capacity of longitudinal adjustment, upon a stout and truly level bed.

The fitting and finishing of a built-up crank shaft is as follows: the special bearings, having been bored out and properly secured upon the bed, are made true and in line axially by applying the mandrel to them, and correcting any irregularities which the mandrel as a face plate may develop.

The several sections of the shaft with the cranks shrunk on and keys forced in are next dropped into and secured in their correct position in the adjusting bearings. A portable boring bar and gear is then set parallel with the shaft, in bearings upon the bed, and

* On some of the Causes of Failure of Crank Shafts in Marine Engines, by J. T. Milton, Trans. Inst. Nav. Architects, 1879.

† Address to the Mechanical Science Section of the British Asso., by B. Baker, M. I. C. E., Pres. of Section, Aberdeen, 1885.

the eyes of each pair of cranks are bored to receive the pin, which is then forced or shrunk in and keyed.

The main bearings of the engine are bored out and the mandrel dropped into them to detect errors, which are corrected, after which the shaft is tested and made true, if necessary, by dropping it into the main bearings, which are now used as an external cylindrical face plate, after which the couplings are corrected by facing them off with power or by hand, while the shaft is revolved in the main bearings.

As a final assurance against the possibility of the shaft and bearings being out of line when in operative position, the mandrel is again applied to the bearings after the line has been run through them and the bed plate securely fastened to the foundations. In the event of either of the bearings having been forced out of shape or position in screwing down, it is corrected by raising or lowering before the shaft is finally dropped into place.

The several operations before described refer more particularly to the case of a built-up crank shaft. When a solid shaft is to be dealt with, it is, after being turned, tested and made true with its own bearings after they have been perfected.

The practical result of following the above has been not only the ease with which the serious errors have been detected and kept out, but also the facility and certainty with which large, as well as small shafts, composed of one, two, and three pair of cranks, together with their bearings, have been produced of such perfect form and excellence that the surface of both journal and bearing is made up of numerous bearing points, equally distributed,* and lying close together.

Moreover, it has been possible with such a condition of truth, to run with very close adjustments, the clearances ranging from the one hundredth to the one hundred and twenty-fifth of an inch. There being no idle surface, this, also, has been done without heating or the use of water during even the first trial of the engine, and that with the full pressure of steam upon the piston. The wear, also, has been kept down to quite a small amount. Some 25,000 nautical miles have been run before re-adjustment has been necessary, and 135,000 before the wear of journal and bottom box combined has amounted to the one fiftieth of an inch.

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* On producing True Planes or Surfaces in Metals, by Jos. Whitworth, Trans. British Asso. for the Advancement of Science, Glasgow, 1840.

The following, it may not be without interest to mention, are the names of some of the vessels fitted with shafts produced in this way: S. S. *Mariposa*, *Alameda*, *San Pablo*, *H. F. Dimock*, *Eureka*, *El Paso*, *El Dorado*, *Philadelphia*, and yacht *Atalanta*.

DISCUSSION.

Mr. Wm. Kent.—I observed in a paper recently published, which was read before the naval architects of Great Britain in reference to flexible shafts, that they seem to think over there the way to keep a shaft in line is to have it flexible, using a flexible joint in two or three points throughout its length.

Mr. See.—In order not to take up too much time I have purposely left out of the paper much interesting matter in my possession written on the subject of the crank shaft, and confined myself as closely as possible to the main facts of the improvement.

Flexible shafting has been proposed as a means to relieve crank and propeller shafting from the strains which bring about breakdowns. In the discussion which followed the presentation before the last meeting of the British Institution of Naval Architects of a paper describing a shaft of this kind, some considered it very doubtful whether such a shaft would work properly and without back lash. It was stated in the paper that ships altered their forms, but Mr. Martell, Chief Surveyor of Lloyds, said "He did not believe, and was unable to find any evidence that ships altered their forms. Steel ships were said to be very elastic, but such vessels, even when of the lightest scantlings permitted by Lloyds, had no difficulty with their shafts when the machinery was of first-class workmanship." Our experience under the new system has not only demonstrated this, but has shown how a new engine with shafts and bearings of undoubted truth can be driven to the utmost without the fear of hot journals or the need of water.

Mr. Kent.—Has Mr. See found any benefit from the use of hollow shafts such as are made by Whitworth?

Mr. See.—We have not tried them in this country, but they are being largely used in Great Britain, that form being particularly applicable to the after length of propeller shafting of high speed screw vessels, such as the new English cruisers, where the fineness of the run necessitates a greater length of shafting outside the ship. Intermediate bearings or hangers have heretofore been used with the solid shaft, but as they produce considerable resist-

ance it is desirable to avoid their use, which has been possible with the hollow shaft by making it of enlarged diameter, with but little extra weight, and yet of sufficient stiffness to run unsupported between the stern bracket and stern tube. They have been made in lengths as great as 60 feet.

Mr. F. W. Taylor.—I might state in relation to hollow shafts that there is without doubt a decided advantage in making shafts hollow if they are in short lengths, and if they are made of steel, for the reason that the best grades of steel, the toughest grades of steel, can be made only by oil tempering. Almost all parts of guns are made in this way, first being bored and then oil tempered, and annealed afterward. The quality of gun steel is very materially improved by oil tempering and annealing. If, however, the shaft is solid, the effect of the oil tempering extends but a short distance from the surface, and the improvement in the steel is almost inappreciable. As a matter of course, in order to get the benefit of oil tempering, the shaft must be divided into short sections, so that it can be dipped into oil. The length of the sections will depend upon the facilities of the maker for oil tempering, but I do not think that in any case it exceeds 40 to 45 feet.

Mr. Kent.—I think that the reason for casting the shafts hollow, is a much more important one than either the lightening of the shaft or the facility of oil tempering. In Whitworth's, at Manchester, they cast the ingot hollow first, then they forge it on a mandrel by hydraulic pressure. In casting a large solid ingot the tendency is to form very large crystals in the middle, and the tendency of the hammer work is not to improve the steel but frequently to damage it. In some cases it is believed that the hammer really ruins the steel, unless you have an enormously heavy hammer. I was told at Whitworth's four years ago that they had made seven hundred hollow shafts down to that time, and not one had broken.

Mr. J. W. Cole.—I think the demand in Europe for flexible shafts arises from their having flexible vessels. There was a case came to my knowledge in December, 1863, of a very strong vessel, an Italian iron-clad, *Re de Italia*, which was built by William H. Webb, armor-plated three inches, I think, and the ribs, as I remember, were twelve inches square and set solid from stem to stern, side by side, making a very rigid, strong vessel. This vessel having stood the maker's test at the dock and a short one in New York harbor, was taken out to sea by the Italian admiral, who, not understand-

ing the coast, and not taking the pilot's advice, ran ashore at Squan Beach. Very fortunately it landed in the sand, and was bedded there several hours, and after being hauled off by wrecking tugs, assisted by her own steam capstan, the vessel was able, when once in deep water, with her own engines, to run up the Jersey coast and into New York harbor, without any hot journals, tending to prove that the extreme strength of the vessel brought the bearings again into line and they worked sufficiently well to avoid hot journals.

Mr. Taylor.—I think that Mr. Kent is mistaken as to Whitworth's method of making hollow shafts. I understand from the Ordnance officers that his method is to cast the ingot solid, bore a hole through the center, which removes the pipe and any impurity which may have collected there, and then forge the ingot on a mandrel. It is a well-known fact that the center of any ingot will invariably be "piped" to a certain extent, and that if you make an annular ingot, with a core in its center, the pipe will then go to the center of the annular section, that is, it will be half way between the surface of the core and the outside of the ingot. Therefore, the practice at Whitworth's at present is to cast a solid ingot, bore the center out in a lathe, which removes pipe and any impurities that may have collected there, and then draw it out on a mandrel. I think Mr. Kent is perfectly right in stating that the quality of the steel is greatly improved by forging on a mandrel. The metal receives much more thorough working than if it is forged from the outside alone.

Mr. J. F. Holloway.—The question of flexible shafts, and particularly of flexible vessels, has a significance on these Western lakes, perhaps to a greater degree than elsewhere, as we have here vessels which are particularly flexible. In the earlier propellers on the lakes the engines had single cranks, and the shafts were joined by flange couplings bolted together. The inequalities in the alignment of the vessel due to load or otherwise were transmitted to that shaft, and through the single crank without any great difficulty, although we cannot say as Mr. See says, that we never had any hot journals; but with the introduction of larger engines and double crank shafts, there came a difficulty about keeping the couplings on the shaft, and a difficulty in keeping the crank shafts from breaking; and in order to obviate that, the first plan was to put in clutch couplings between the inboard and the outboard shaft. They answered very well, except that after being used awhile, there was a

good deal of lost motion in them, and they got to be very noisy and very troublesome. The next advance in that respect was to put in a universal joint coupling, and I think they are now universally used in all large engines. This coupling allows for the bending of the vessel while loading and unloading, which amounts to considerable, as well as the torsion and bending of the vessel in a sea-way while at the same time it obviates any difficulty of back lashing, as we use a wedge with which to take up the lost motion in the couplings. This has been found in Western practice to be the very best thing we can do in marine engines which have double crank-shafts. I believe there is now very little difficulty except from imperfect manufacturing of the crank-shafts and from the torsion of the shaft, and the bending back and forward to which all double cranks are liable, and which, after long use, usually proves destructive.

The Chairman.—What is the location of the coupling?

Mr. Holloway.—You will understand in the marine engines on the lakes the outboard shaft is much the longest shaft, being usually two-thirds of the whole length of the shaft, and our practice is, to put the coupling just aft of the main journal, leaving room only for the eccentrics, and all the bending of the shaft takes place at that point.

Mr. Walker.—In reference to what Mr. Kent has said, and for a yielding vessel, flexibility should be allowed in the couplings, so that the yielding of vessel may be accommodated without detriment to the bearings or shafts.

This illustration (Fig. 260) shows a recent invention of Mr. Peter

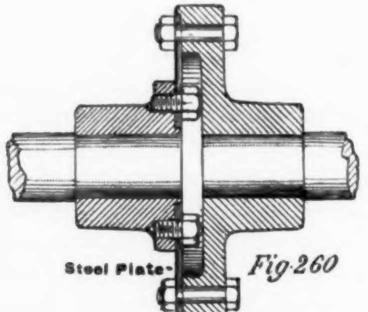


Fig 260

Brotherhood, London, England, which I understand has already been used on vessels. It would be proper to have all the bearings

in line when the vessel is at rest, but when the vessel yields at sea this flexible coupling will no doubt allow for such yielding without detriment to the bearings. To get the full benefit of this coupling there should not be any intermediate bearings.

Mr. See.—All of these appliances are wholly to allow for irregularities in the bearings, and not to help errors in the main journals or crank-pin. If any slackness is allowed in the main bearings, the shaft by a flexible coupling is allowed to accommodate itself to a high or low bearing, and the crank pin will be thrown out of line. Also, if the shaft after being turned should, when released from the lathe, either open or close in the gap between the cranks at the center of the shaft so as to destroy the axial coincidence, the pin will change its relationship to its box at each revolution of the shaft. In all of these cases the surfaces presented for wear will be untrue. The main bearings if not true will give trouble, but the untruth of the crank-pin gives greater and is more difficult to reach. The principle aimed at in the improved method is to start out with perfect form and alignment of both shafting and bearings. If this is done they will remain in such a condition for a great while. This we have ascertained through the use of the gauges which, before the vessel left the yard, were fitted to each journal. These gauges are made so as to be guided on the top and between the inside faces of the pedestals and to touch the top and sides of the journals. At stated periods the caps are lifted, the gauges applied, and the amount of wear correctly measured. It was in this way that the wear in the journals of the vessel which had run one hundred and thirty-five thousand miles was ascertained.

The Chairman.—Mr. See's paper is another illustration of the fact which we are seeing all the time, that the tendency of modern practice is to high-class workmanship cures many of the troubles that we have had to contend with. Apparently the simple statement of Mr. See's paper is that perfect workmanship will give perfect results and no heating.

Mr. G. H. Babcock.—There is a method of determining whether a crank-pin is true or not which I think may not be perfectly familiar to all of you. The point is how we shall determine after the crank is made whether the pin is in line with the bearings or not. A gauge to lay over and measure is necessarily inadequate, for the reason that it is liable to spring unless it is extremely large, and also because its bearing upon the crank pin itself is so very short. A way which has been used more or less is to place the connecting

rod upon the pin, key it up snugly and mark at the further end of the connecting rod its position. Then turning the crank pin a half revolution, let the connecting rod stand in the same direction as before, key up snug and mark again. If these marks coincide or are parallel the crank pin is necessarily in line with the shaft. The difference of the alignment is multiplied many times by the length of the connecting rod, and then doubled by turning the crank over, so that it is possible to ascertain very accurately by that means how true the crank pin is with the journals.

Mr. J. E. Sweet.—The device described by Mr. Babcock would not answer our purpose. If the shoulder where the box comes in contact with the crank is not true then the result will not be true, or if the face of the box is not kept to one side of the crank, then we will not have a true result.

Mr. Babcock.—I do no quite understand the criticism.

Mr. Sweet.—If the connecting rod is carried tight up to one shoulder, and that shoulder is spiral or at an angle with the crank, the result will not be true.

Mr. Babcock.—If the box is accurately fitted to the pin and snugly keyed, the shoulder has no influence on the result except to move the rod bodily to one side. This is shown by the marks being parallel but not coincident.

Mr. H. P. Minot.—If the pin is not true, what then?

Mr. Babcock.—I did not propose to describe a method of making a crank, but of testing it after it was made.

Mr. Oberlin Smith.—I want to ask why crank pins of that kind have not been made spherical, swelling up to a bulbous form in the center. Is there any practical difficulty in the way?

Mr. See.—The next thing to find out is whether the globe is true.

Mr. Babcock.—That was done a great many years ago upon the Lakes by Erastus W. Smith. One time I thought that a matter of importance, and I patented a globular box in a spherically bored connecting rod in such a way that it would give to the inequalities of the crank pin. But I laid it aside as one of the foolish notions of youth long ago.

Mr. Minot.—I do not see any difficulty in making a crank of that kind and making it true. It seems to me that a good workman can make a large crank just as well as a small one. Many cranks are bored under a drill, perhaps a hanging drill—something of that kind—and there is nothing true about it. Now, if you have a good lathe

which is true, and have a man that knows how to handle it, I see no trouble in making a crank reasonably true. I do not mean perfect, because I do not suppose we have got to that yet. I do not see the use of using the connecting rod to find out if it is true, when it doesn't do us any good when we do find it out. If your centers are properly located and the right distance apart, I see no trouble in making a good job in this way and getting the pin true.

Mr. Nagle.—Will Mr. Babcock state what serious errors he has discovered in that way?

Mr. Babcock.—I cannot say how serious errors have been discovered in that way, but of sufficient importance to require a new construction. For instance, in a case where a crank had been furnished by another party, we wished to determine whether it was sufficiently accurate for use.

Mr. Schuhmann.—The method described by Mr. Babcock not only shows whether the crank pin is in line but it also shows whether the brass boxes are bored square. In making this test on a large vertical engine, the crank pin of which ran hot, I disconnected the cross-head end and keyed up the crank end to take up lost motion, and found the connecting rod about one inch and a half out of line with the cross-head brass; as it leaned to the same side on top and bottom center it proved that the crank pin was in line but that the brass box had not been bored square; after correcting the error in the brass box the crank pin ran cold.

Mr. G. M. Bond.—I might say that it is generally customary, if the work requires even ordinary accuracy, to have some means of inspecting it, and I should think that Mr. Babcock's plan would be a good one. It is customary to use gauges for the purpose of testing finished work, however carefully carried out in detail, and I think it might be well to subject such work to the test referred to by Mr. Babcock, as a guaranty that it had been properly done.

Mr. Oberlin Smith.—I do not know how foolish Mr. Babcock's youth was, but he evidently misunderstood me about that spherical arrangement. I should not think it would be very sensible to have the box wabble around in the connecting rod when it could be put firmly in in the usual way and do *all* its moving upon the crank pin. I meant to have the crank pins spherical. The trouble of alignment is by such a device left out entirely. It would align itself all the time. I think the difficulty of such crank shafts getting made out of line is probably due in many cases to the deflection of the shaft by its own weight. In such heavy long work, even if it

is laid out accurately, the flexure of the shaft by its own weight when in the lathe will sometimes make a marked difference in the final result.

Mr. See.—I might add in closing, that Messrs. Sir J. Whitworth & Co. have in their display at the Liverpool Exhibition the longitudinal section of a hollow ingot.

CCXII.

ON THE RELATIVE ECONOMY OF VENTILATION BY HEATED CHIMNEYS AND VENTILATION BY FANS.

BY W. P. TROWBRIDGE, NEW YORK CITY

Of the various modes of producing the air-currents by which vitiated air is removed from chambers, halls, or working places, and fresh or pure air simultaneously introduced, involving the processes of ventilation, the heated chimney is the most common; although it is generally recognized that where large volumes of air are to be moved against considerable passive or frictional resistances, the use of the fan or blower is theoretically the most economical.

The following investigation has been undertaken with a view of establishing the exact theoretical relation between these two modes of ventilation, as far as economy of heat is concerned, and incidentally to determine, as far as the uncertain elements of the question may permit, the circumstances under which either of these methods may advantageously be employed in preference to the other; it being understood that the ventilation of public buildings, mines, suites of rooms, or single large rooms are all included in the problem.

It is assumed that since air is everywhere present at the earth's surface, ordinary ventilation is accomplished by a simple movement of air, all portions of which exist under the same pressure before motion begins: and that therefore the problem does not involve the lifting of the air through a determined height. The resistance to motion, or the forces to be overcome, are then the frictional resistances of the passages through which the air flows, and the inertia of the air put in motion. The expression "frictional resistances" is to be understood as implying all those resistances which oppose or obstruct the motions of fluids through conduits or channels, and which are usually expressed in terms of the height due to the actual velocity of flow, or are proportional to the square of the actual velocity of flow.

The work per second necessary to overcome these resistances may be expressed by the weight which flows per second multiplied by the head or height of a column of fluid which, expressed in terms of the velocity of flow, represents the total resistances. The frictional head for a given condition of things—a given conduit and passages—is usually expressed by a constant depending on the length, form, and dimensions of the conduit and passages, multiplied by the head due to the velocity of flow; or by an expression having the form

$$F\frac{v^2}{2g}.$$

The work performed in putting a weight of air represented by w in motion with a velocity v per second will be, therefore,

$$W = w\frac{v^2}{2g}(1+F).$$

If there are no resistances except those due to the inertia of the moving masses of air the constant quantity F disappears, and the work will be that represented by $w\frac{v^2}{2g}$ — the living force or actual energy imparted to the air per second.

In any investigation having for its object the relative economy of the methods of putting the same quantity of air in movement per second, through the same channels, and with the same velocity, it will be sufficient, therefore, to consider the work $w\frac{v^2}{2g}$, since the work performed per second in both cases must be the same whether the frictional resistances are considered or not.

It is further to be remarked, that by whatever means air is put in motion under the circumstances which we are considering, the process consists in a reduction of pressure at one point, whether a fan or a heated chimney be used, which creates an unbalanced head in the surrounding air, and a consequent flow to the point of reduced pressure.

This unbalanced pressure per square foot of section produced by a heated chimney is represented by the expression:

(1.) $$p = H.(D_a - D_c),$$

in which H represents the height of the chimney, D_a the weight

per cubic foot of the external air, and D_c the weight per cubic foot of chimney air.

The height of a column of fluid, whether of chimney air, external air, water, or mercury, which would represent this pressure is found by dividing the above value of p by the density of the fluid; thus,

$$(2.)\ \frac{p}{D_c} = H\left(\frac{D_a - D_c}{D_c}\right) = H\left(\frac{T_c - T_a}{T_a}\right), \text{ because } \frac{D_a}{D_c} = \frac{T_c}{T_a}.$$

T_c and T_a representing the absolute temperatures of the chimney air and the external air respectively.

In this last expression $H\left(\frac{T_c - T_a}{T_a}\right)$ represents the height of a column of air of a uniform density D_c, which by its weight would give a pressure per square foot represented by P.

The velocity with which air would flow into a space under this pressure is

$$(3.)\qquad v = \sqrt{2\,gH.\left(\frac{T_c - T_a}{T_a}\right)}.$$

The work per second produced by the chimney for each square foot of cross-section will be

$$(4.)\qquad W = pv = D_c\,v.\,H\left(\frac{T_c - T_a}{T_a}\right).$$

Substituting the value of v from (3) we have

$$(5.)\qquad W = D_c\sqrt{2g\,H^3.\left(\frac{T_c - T_a}{T_a}\right)^3}\quad . \quad . \quad . \quad \text{foot lbs.}$$

This is the work per second in foot pounds accomplished by the expenditure of heat in heating the air of the chimney, and thus producing motion.

The quantity of heat thus expended is represented by the expression

$$(6.)\qquad Q = D_c\,v.\,c_p\,(T_c - T_a),$$

in which Q is expressed in units of heat, $D_c\,v$ represents the weight of air which passes through each square foot of cross-section per second, and $(T_c - T_a)$ the number of degrees through which this air has been heated, and c_p the specific heat of air under constant pressure.

If we substitute again in this expression the value of v taken from (3) we have

$$(7.)\qquad Q = D_c\, c_p \sqrt{2g\, H \frac{(T_c - T_a)^3}{T_a}}.$$

This expression represents the heat units expended in heating the air of the chimney to produce the velocity v in the chimney.

The heat furnished may be supplied by a furnace at the base of the chimney, the heated products of combustion from which mingle with the air which enters the base of the chimney; by a system of steam pipes which heat the air by contact as it passes through or among them, or by any other mode which will accomplish the result. If a fire or furnace be employed, as in mines, in such a way that the dissipation or loss of heat from the furnace is prevented, the efficiency of the furnace may be considered unity.

Under this, the most favorable circumstance for the efficiency of the chimney, equation (7) gives the total heat generated and available.

In ventilation by a fan or blower driven by a steam engine, the heat expended to produce the same velocity, or the same discharge and renewal of air, will depend on the efficiency of the steam boiler and engine, the efficiency of the fan or blower, and the loss by friction in the apparatus.

If we consider the efficiency of the boiler and engine to be one-tenth, the efficiency of the fan five-tenths, and the loss from friction two-tenths, or the efficiency as regards friction eight-tenths, the resulting efficiency of the whole apparatus will be

$$E = .1 \times .5 \times .8 = .04 \text{ or } \tfrac{1}{25}.$$

The work performed by the heated chimney to produce the velocity v, and for each square foot of cross-section was found to be equation (5)

$$(8.)\qquad W = D_c \sqrt{2g\, H^3 \left(\frac{T_c - T_a}{T_a}\right)^3} \quad . \; . \; . \quad \text{in foot lbs.}$$

To produce the same work by a fan whose efficiency is $\frac{1}{25}$, twenty-five times this amount of work must be expended in equivalent heat units. Hence the number of heat units to be expended will be

$$(9.)\qquad Q^1 = \frac{25}{772}\, D_c \sqrt{2g\, H^3 \left(\frac{T_c - T_a}{T_a}\right)^3},$$

the second member being divided by 772 to transform its value in foot lbs. to its value in heat units. The relative quantities of heat expended by the chimney and fan, or the relative efficiency under the conditions assumed, will then be

$$(10.) \qquad \frac{Q'}{Q} = \frac{\frac{25}{772} D_c \sqrt{2g\, H^3 \left(\frac{T_c - T_a}{T_a}\right)^3}}{D_c\, c_p\, \sqrt{2g\, H. \frac{(T_c - T_a)^3}{T_a}}}.$$

Or

$$\frac{Q'}{Q} = \frac{25\; H}{772.\; c_p\, T_a} = \frac{H}{7.35\; T_a},$$

the value of c_p being 0.238.

If we suppose the temperature of the external air to be 60° F., the value of T_a will be 519.4, and

$$(11.) \qquad \frac{Q'}{Q} = \frac{H}{3817.59}.$$

This expression shows that the relative efficiency depends only on the height of the chimney, and in no way on the differences of temperatures within and without the chimney. For a chimney one hundred feet high the efficiencies will be as 1 to 38.17; or,

$$Q' = \frac{Q}{38.17},$$

showing that the chimney requires an expenditure of heat thirty-eight times greater than the fan. For a chimney 500 feet high, the fan will be 7.6 more efficient.

If the chimney be heated by steam pipes at its base the efficiency of the boiler and pipes must be taken into consideration, making a result still more unfavorable for the chimney.

On the other hand, where small quantities of air are moved, requiring only a fraction of a horse-power, or one or two horse-powers, to drive a fan, these powers being produced by a small engine and boiler employed solely for this purpose, the efficiency of the mechanical apparatus would probably be much less than $\frac{1}{25}$, a condition of things unfavorable to the fan.

We may now inquire under what circumstances the chimney might be advantageously employed instead of the fan.

In all cases of moderate ventilation of rooms or buildings where

as a condition of health or comfort the air must be heated before it enters the rooms, and spontaneous ventilation is produced by the passage of this heated air upwards through vertical flues, the efficiency of this mode of ventilation is evidently unity; that is to say, no special heat is required for ventilation; and *if such ventilation be sufficient*, the process is faultless as far as cost is concerned. This is a condition of things which may be realized in most dwelling-houses, and in many halls, school-rooms, and public buildings, provided inlet and outlet flues of ample cross-section be provided, and the heated air be properly distributed.

If, starting from this condition of things, we suppose a more active ventilation to be demanded, but such as requires the smallest amount of power, the cost of this power, when the wages of a skilled mechanic are taken into account, may quite outweigh the advantages of the fan in fuel. There are many cases in which steam pipes in the base of a chimney, requiring absolutely no care or attention, may be preferable to mechanical ventilation, on the ground of cost, and trouble of attendance, repairs, and maintenance. There is quite a wide field for the employment of heated chimneys for ventilation before a limit is reached when the fan becomes indispensable, even when economy alone is considered; and this field becomes more extended, when convenience, saving of time, and personal care and attention influence a choice.

Ventilation by chimneys is disadvantageous under one point of view in any case, viz.: the difficulty of accelerating the ventilation at will when larger quantities of air are needed in emergencies.

The fan or blower possesses the advantage in this respect that by increasing the number of revolutions of the fan the head or pressure is increased, the law being that the total head produced is equal (in centrifugal fans) to twice the height due to the velocity of the extremities of the blades, or

$$H = \frac{v'^2}{g} \text{ approximately in practice.}$$

In mines it is evident that to produce by a chimney the same ventilation as that produced by a fan with the same economy of fuel the up-cast shaft must be very deep. Taking into consideration the wages of an engineer employed to run a large fan and the cost of maintenance and repairs, it might happen, however, that a mine of moderate depth, where the galleries are large and the resistances consequently small, could be efficiently ventilated by a

furnace and chimney, at no greater expense than is required for the fan.

It is worth while to consider in this connection the rate at which the expenditure of heat increases in chimney ventilation when for the same channels of flow it is desirable to accelerate the velocity by increasing the heat of the chimney. Equation (3) gives the volume of flow per unit of section of the chimney, in terms of the height of the chimney and the interior and exterior temperatures. For the same height H, the volume of flow per second is proportional to the square root of the difference of temperatures.

Equation (7) gives the expenditure of heat for the same height H, and for the same difference of temperatures. The height H remaining constant, the expenditure of heat is proportional to the square root of the cube of the difference of temperatures.

The first formula is equivalent to the following:

$$v = C\sqrt{T_c - T_a},$$

and the second to

$$Q = C'\sqrt{(T_c - T_a)^3}$$

C and C' being constants.

If in these formulas we make $(T_c - T_a)$ successively 9, 16, 25, 36, 49, 64, 81, we have the following results:

Differences of Temperature.	Volumes.	Heat expended.
9°	C × 3	C′ × 27
16°	C × 4	C′ × 64
25°	C × 5	C′ × 125
36°	C × 6	C′ × 216
49°	C × 7	C′ × 343
64	C × 8	C′ × 512
81	C × 9	C′ × 729

This shows that as the volume (or velocity) is increased by increasing the difference of temperature, the expenditure of heat increases as the cubes of the volumes.

Economy of heat requires, therefore, that the *velocity* shall be

kept small and increase of *volume* obtained by enlarging the chimney and the channels or conduits through which the air passes. Moreover, since the resistances from friction diminish in rapid proportion as the channels are enlarged, and more of the total head produced by the chimney becomes available to create the velocity of flow, an additional advantage in large cross-sections for the chimney and conduits is secured.

The same laws of expenditure of heat hold for the fan or blower, the expenditures of heat increasing for the same conduit as the cube of the velocity of flow. This is, in fact, a general law for all cases where work is performed under such circumstances that the resistances are proportional to the square of the velocity of motion. In such cases the resistance being ($R = C.v^2$) a constant multiplied by the square of the velocity, the work performed per second will be proportional to the cube of the velocity

$$\overline{W} = Rv = Cv^3.$$

It often happens that for a particular chimney and channels of flow the ventilation becomes insufficient, and instead of increasing the heat in the chimney with a large additional expenditure of fuel, a fan is introduced to take the place of the chimney ventilation. The relative efficiency $= Q \dfrac{Q'H}{3817.59}$, and the application of this law of the proportion of heat expended to the velocity of discharge, enables us to ascertain to what limit such a substitution of a fan for a chimney may be carried before the cost of the fan exceeds the cost of the furnace ventilation.

In the above equation of efficiency, if the chimney is 100 feet high the fan will be 38 times more efficient than the chimney, and the table shows that the velocity of flow by the fan may be quadrupled before the cost exceeds that of the chimney. If the chimney is 200 feet high the fan will be 19 times more efficient than the chimney, and the velocity of flow may be increased to a little more than two and a half times that which was produced by the chimney before the cost by the fan exceeds that by the chimney. For a chimney 500 feet high the velocity by a substituted fan could hardly be made twice that produced by the chimney before the cost of the fan with increased ventilation should exceed that of the chimney. The question might then turn upon the advisability of getting the increase by additional heat in the chimney even with a

large proportional additional expenditure of fuel; the cost of attending and maintaining the fan becoming an important element in the problem.

It is quite evident that for the fan as well as for the chimney low velocities and large conduits are favorable to economy.

The following records of experiments are furnished in connection with this paper as a contribution from Mr. Geo. A. Suter, M. E., a graduate of the School of Mines, junior member of the Society, and now engineer for the New York Exhaust Ventilator Company.

RECORD OF EXPERIMENTS MADE WITH THE BLACKMAN FAN BY MR. GEO. A. SUTER, M. E., TO DETERMINE THE VOLUMES OF AIR DELIVERED UNDER VARIOUS CONDITIONS, AND THE POWER REQUIRED.

Revolutions per Minute.	Cubic feet of Air delivered per Minute.	Horse-power.	Water-gauge. Inches.	Nature of the Experiments.
350	25797	0.65		Drawing air through 30 feet of 48-inch diameter pipe on inlet side of the fan.
440	32575	2.29		
534	41929	4.42		
612	47756	7.41		
340	20372	0.76		Forcing air through 30 feet of 48-inch diameter pipe on outlet side of the fan.
453	26660	1.99		
536	31649	3.86		
627	36543	6.47		
340	9983	1.12	0.28	Drawing air through 30 feet of 48-inch pipe on inlet side of the fan—the pipe being obstructed by a diaphragm of cheese-cloth.
430	13017	3.17	0.47	
534	17018	6.07	0.75	
570	18649	8.46	0.87	
330	8399	1.31	0.26	Forcing air through 30 feet of 48-inch pipe on outlet side of fan—the pipe being obstructed by a diaphragm of cheese-cloth.
437	10071	3.27	0.45	
516	11157	6.00	0.75	

The experiments were made by him with great care, the power of the engine driving the fan having been determined by the steam-engine indicator, and the volumes of air delivered having been carefully determined at the same time by an anemometer.

The fan employed was a Blackman fan, belonging to the class of disc fans, four feet diameter, and the experimental apparatus was so arranged that the air was drawn and forced alternately through a metallic tube 30 feet long and four feet diameter; the fan being mounted at one end of the tube.

In the first two sets of experiments the only resistance to the

flow of the air was the frictional resistance of the tube and fan passages, and the resistance caused by the contracted vein at the entrance. In the second two sets the passage of the air was obstructed by a diaphragm of cheese-cloth placed within the tube; and this additional resistance was ascertained by a water-gauge in the usual way. The table of experiments is useful for determining the horse-power required for given volumes of air discharged with this class of fans, under free delivery and against resistances represented by the water-gauge readings of the table. For large volumes with free delivery, or with very small water-gauges, the efficiency and the small power required are worthy of attention. Estimating four and a half pounds of coal per horse-power per hour in common cases, with coal at five dollars per ton, a horse-power will cost, as far as fuel is concerned, about one cent per hour; the hire of a man to take care of and manage the apparatus, including other expenses, perhaps twenty-five cents per hour.

For such cases, and especially where the power required is only a *small fraction* of a horse-power as in ventilating single large rooms or small buildings, it is evident that as regards cost of fuel and the care and attention required, ventilation by heated chimneys is to be preferred. Where a fan is driven by machinery employed for other purposes than ventilation, the cost of attendance chargeable to ventilation being therefore trifling, the fan would evidently in all such cases be more appropriate. A variety of circumstances and conditions enter into these problems of ventilating single rooms, or halls, and a choice can only be made through the exercise of the best judgment.

Under circumstances where hospitals or public buildings of considerable magnitude are to be ventilated, and especially where the activity of the ventilation must be varied occasionally, the fan is no doubt to be preferred. And this is quite sure to be the case when the vitiated air is drawn through several systems of collecting ducts from a series of large rooms, into one main outlet; the friction of such collecting conduits, and the resistances of bends and changes of direction in them becoming principal factors in the power consumed. In such cases a comparatively high velocity at the outlet is indispensable. A system of ventilation by means of heated chimneys in such cases involving no greater cost would require numerous and ample vertical heated flues so arranged in the construction of the building that the velocity in each flue should be the smallest possible, and the frictional resistances avoided by the most direct pas-

sages of the vitiated air to the heated chimneys. A thorough and proper distribution of the incoming fresh air would demand such a distribution also that collecting ducts could be largely dispensed with; or if they were necessary, that they should be as short and as large in size as the conditions of least resistance might demand.

Such a system might in many cases be preferable to one involving the use of a fan even in large buildings; but unless such buildings have been designed with this plan in view, proper inlets for fresh air forming a part of the plan, it is difficult to apply it with success.

In nearly all public buildings of large size, which come under the head of old buildings in which the necessities of proper ventilation were originally neglected, the fan will probably be found to be the most efficient remedy for deficient ventilation.

DISCUSSION.

Mr. Geo. H. Babcock.—The conclusion of the author that the relative efficiency of a chimney and fan ventilation depends only on the height of the chimney and in no way on the temperature within the chimney, is at first somewhat startling, but is doubtless correct as to the expenditure of heat per unit of work done, because while this is assumed to be constant for a fan driven by a steam engine, the efficiency of a chimney as a heat engine is itself dependent only upon the height of the chimney and the density of the external air. This is reasonable when we consider that the work is done by the falling of a column of external air, the weight of which is equal to the height of the chimney multiplied by the density of the air.

By dividing the foot-pounds of work (equation 4) by the units of heat expended (equation 6), we find that the foot-pounds of work per unit of heat is equal to the height divided by the temperature and specific heat of the external air.

$$\frac{\overline{W}}{Q} = \frac{H}{c_p T_a} = \frac{C}{c_p} H D_a,$$

or, as the density is inversely as the temperature, equal to a constant multiplied by the weight of a column of external air equal in height to the chimney. Reducing heat units to foot-pounds, the efficiency is represented by

$$E = \frac{H}{772c_p T_a} = \frac{C}{772c_p} HD_a = .0001357 HD_a,$$

which at an atmospheric temperature of 60° gives an efficiency of one per cent. for a chimney 1,000 feet high and in direct proportion for other heights. Within the range of mechanical possibilities the efficiency of a chimney as a heat engine is very low. But the infinitely wise Being who engineered the building of this world made no mistake when He adopted substantially that as the means of putting in motion the great currents of air which ventilate our globe. When we consider that His chimney is the height of the atmosphere with a range of temperature equal to the average differences between the equator and the poles, it is more than probable that His ventilator is a "perfect heat engine" in the sense of utilizing the greatest possible proportion of the heat expended upon it.

But the efficiency of an ordinary chimney as a heat engine is not a measure of its value as a means of ventilation, for the reason that the object to be obtained is not foot-pounds of work done, but pounds of air removed. The conditions under which a chimney works are that the same addition of heat which increases the velocity and the work, also decreases the density of the air, and, as a consequence, the amount of air removed is not in proportion to the increase of velocity, the foot-pounds of work done, or the expenditure of heat.

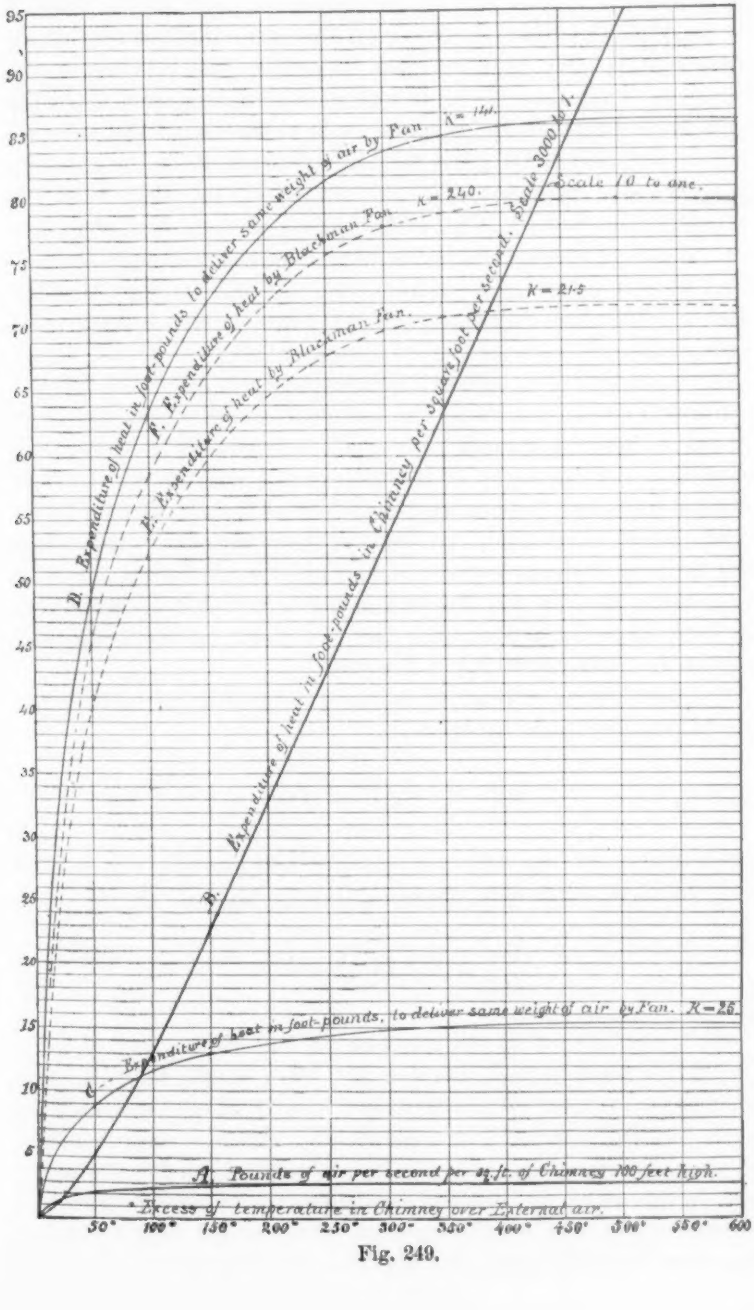
The weight of air moved in a second is represented by the density of the air in the chimney multiplied by its volume, and may be found from formula (3) remembering that the density of air is always equal to a constant (C) divided by the absolute temperature. Substituting and massing constants we have:

$$D_c V = \frac{C_1}{T_1} \sqrt{H\left(\frac{T_c - T_a}{T_a}\right)}$$

in which the constant $C_1 = C\sqrt{2g}$, in which, $C = 40$, approximately. Assuming that the temperature of the external air is 60°, T_a becomes a constant of 520, in which case

$$D_c V = \frac{14}{T_c} \sqrt{H(T_c - T_a)}$$

within less than one-half of one per cent. It will be noticed that



D. Expenditure of heat in foot-pounds to deliver same weight of air by Fan K = 141.
F. Expenditure of heat by Blackman Fan K = 240.
Scale 10 to one.
E. Expenditure of heat by Blackman Fan.
K = 21·5
B. Expenditure of heat in foot-pounds in Chimney per square foot per second. Scale 3000 to 1.
C. Expenditure of heat in foot-pounds, to deliver same weight of air by Fan. K = 25
A. Pounds of air per second per sq. ft. of Chimney 100 feet high.
Excess of temperature in Chimney over External air.

Fig. 249.

the variable T_c occurs in both the numerator and denominator of the fraction, in different ratios, and therefore the quantity of air delivered will vary in a different ratio from the velocity, or the difference of temperatures. It will become a maximum when the absolute temperature in the chimney is twice that of the external air, and will vary but slightly for large differences in the chimney temperatures. This is graphically shown by the line A in the accompanying diagram (Fig. 249), it being assumed that $H = 100$, and the area of chimney is one square foot, in which case the volume (V) and the velocity (v) are equal. It will be seen that the quantity varies less than one per cent. between 400° and 600° and is within ten per cent. of the maximum at 200°.

The horizontal scale of this diagram gives the difference in temperatures between the air in the chimney and that outside. The vertical scale is for pounds of air, and foot-pounds of work, each per second, and applies to all the lines except two, as noted. The scale for these two lines must be multiplied by the figures given (10 and 3,000 respectively) to compare them with the others. The lines showing expenditure of heat are in all cases for the quantity of air shown by line A. Per *unit of air* removed, the lines showing expenditure of heat by the fans would all be horizontal and that for the chimney would be diagonal.

The expenditure of heat per unit of air removed by the chimney may be obtained from equation (6).

$$\frac{Q}{D_c v} = c_p (T_c - T_a).$$

Multiplying this by 772 to reduce to foot-pounds, and substituting for c_p its value .238, we have, making $772 Q = Q_{f.p.}$

$$\frac{Q_{f.p.}}{D_c v} = 183.74 (T_c - T_a).$$

This is represented on the diagram by line B, the vertical scale being 3,000 times that of the line A. Thus it will be seen that in chimney ventilation the cost per pound of air delivered is directly as the difference of internal and external temperatures, and that, therefore, the best results are secured by low temperatures in the chimney, without reference to its height—entirely different controlling conditions from those which would obtain, if we were considering its efficiency as a heat engine.

It will be noticed that the difference of temperature $T_c - T_a$ bears the same ratio as the square of the velocity, hence the cost for a given weight of air removed is directly as the square of the velocity, which is the same as with a fan or other means. In order, however, that this cost may bear a constant ratio by chimney and fan, it is necessary that the densities must be the same, that the velocities may also be equal. But the density of the air moved by a fan is rarely the same as that in a heated chimney. Therefore, to ascertain the ratio of relative cost we must find the expenditure of heat per unit of air moved by a fan. The density of this air is represented by D_f. The work done in moving a given volume, as a cubic foot of air, equals the density of the air multiplied by the height h, from which a body must fall to produce the same velocity.

$$W = D_1 h.$$

But the energy of the heat expended is not all utilized as work by the fan. Let K be this ratio of efficiency, *i. e.* $K = \frac{E}{E'}$, in which E equals the energy of the heat used, and E' the energy utilized by the fan, then

$$Q'_{\text{f.p.}} = KD_1 h, \text{ and as } h = \frac{v'^2}{2g}, \quad \therefore \quad Q'_{\text{f.p.}} = KD_f \frac{v'^2}{2g}.$$

Now it is part of the problem that the weight of air moved is the same by the chimney and fan, and is equal to unity, therefore, velocity and volume being equal, $D_c v = D_f v' = 1$. Whence $v' = \frac{1}{D^f}$, substituting this value of v' we obtain

$$Q'_{\text{f.p.}} = \frac{K}{2gD_f} \text{ which is the same as } Q_{\text{f.p.}} = \frac{KT_f}{2gC}.$$

As $2g$ and C are constants, and K constant for a given apparatus, we see that the foot-pounds of energy expended per unit of air varies inversely as the density, or directly as the temperature of the air moved, which upon consideration is an evident proposition. Massing the known constants we have for fan ventilation:

$$Q'_{\text{f.p.}} = \frac{KT_f}{2576}.$$

Now we found that in the case of the chimney, the expenditure

of energy varied directly as the difference in temperature, $Q_{f.p.} = 183.74\,(T_c - T_a)$ and by dividing, we have

$$\frac{Q'}{Q} = \frac{K\,T_f}{473{,}314\,(T_c - T_a)},$$

which is quite a different relation from that of the expenditure of heat per foot-pound of work done, as deduced by Prof. Trowbridge.

Line C of the diagram shows the cost in foot-pounds of energy, of exhausting by means of a fan the amount of air shown by line A on the same scale, the efficiency of the mechanism being $\frac{1}{25}$, as assumed by Prof. Trowbridge. In practice, however, it is rare to find an engine and fan giving so great an efficiency. A small engine, such as is commonly used to drive such fans, consumes all the way from 4 to 10 lbs. of coal per hourly horse-power. Line D of the diagram shows the same fan with an engine using 10 lbs. of coal per hourly horse-power, on the same scale as A and C.

But in order that a fan blower be comparable with a chimney on these terms, it must have an area of opening equal to that of the chimney, for if the outlet be reduced, the velocity must be increased in the same ratio, with an increase of cost in the ratio of the square of the velocities. It is rarely practicable to use a fan with the same area of opening as a chimney intended for removing the same quantity of air, and therefore this is a matter of importance in the consideration of the question before us.

The table of experiments with the Blackman fan, given in the paper, is very interesting, and a desire to know what the real efficiency was, induced me to figure the efficiency of the different experiments on the basis of the ratio of the work actually done to the horse-power expended, making no allowances for friction in the pipe or fan, or for contracted vein at the entrances. The first four experiments are evidently the subject of some error, because the efficiency is such as to prove on an average that the fan was a source of power sufficient to overcome all losses and help drive the engine besides. The second series is less questionable, but still the efficiency in the first two experiments is larger than might be expected. In the third and fourth series, the resistance of the cheese-cloth in the pipe reduced the efficiency largely, as would be expected. In this case, the value has been calculated from the height equivalent to the water pressure, rather than the actual velocity of the air.

TABLE OF EXPERIMENTS WITH BLACKMAN FAN, WITH CALCULATIONS OF EFFICIENCY AND RATIO OF INCREASE OF POWER TO INCREASE OF VELOCITY.

Revolutions per minute.	Cubic feet of air per minute.	Horse-power.	Head in inches of water.	Ratio of increase of speed S.	Ratio of increase of delivery $= \frac{v}{v}$	Ratio of increase of power $H. P.$	Exponent x. $H. P. \propto v^x$	Exponent y. $h \propto v^y$	Efficiency of fan.
350	25,797	0.65							1.682
440	32,575	2.29		1.257	1.262	3.523	5.4		.9553
534	41,929	4.42		1.186	1.287	1.843	2.4		1.062
612	47,756	7.41		1.146	1.139	1.677	3.97		.9358
For	series.			1.749	1.851	11.140	4.		
340	20,372	0.76							.7110
453	26,660	1.99		1.332	1.308	2.618	3.55		.6063
536	31,649	3.86		1.183	1.187	1.940	3.86		.5205
627	36,543	6.47		1.167	1.155	1.676	3.59		.4802
For	series.			1.761	1.794	8.513	3.63		
340	9,983	1.12	0.28						.3939
430	13,017	3.17	0.47	1.265	1.304	2.837	3.93	1.95	.3046
534	17,018	6.07	0 75	1.242	1.307	1.915	2.25	1.74	.3319
570	18,649	8.46	0.87	1.068	1.096	1.394	3.63	1.60	.3027
For	series.			1.676	1.704	7.554	3.24	1.81	
330	8,399	1.31	0.26						.2631
437	10,071	3.27	0.45	1.324	1.199	3.142	6.31	3.06	.2188
516	11,157	6.00	0.75	1.181	1.108	1.457	3.66	4.96	.2202
For	series.			1.563	1.329	4.580	5.35	3.72	

This record of experiments made with the disk fan shows very conclusively that this kind of a fan is not adapted for use where there is any material resistance to the flow of the air. In the centrifugal fan the power used is nearly proportioned to the amount of air moved under a given head, while in this fan, as is shown by inspection of the figures given, the power required for the same number of revolutions of the fan increases very materially with the resistance, notwithstanding the quantity of air moved is at the same time considerably reduced. In fact, from the inspection of the 3d and 4th series of tests it would appear that the power required is very nearly the same for a given pressure, whether more or less air be put in motion. It would seem that the main advantage, if any, of the disk fan over the centrifugal fan for slight resistances consists in the fact that the delivery is the full area of the disk, while, with centrifugal fans intended to move the same quantity of air, the opening is usually much smaller.

It will be seen by columns 8 and 9 of the table that the power used increased much more rapidly than the cube of the velocity, as in centrifugal fans, and in mechanics generally. The different

experiments do not agree with each other, but a general average may be assumed as about the cube root of the eleventh power. With this value and an efficiency of .58, the average of the second series of tests—other elements being the same as for line *C*,—line *E* of the diagram has been computed. Line *F* is for the same fan, working against resistance as in series 3 and 4 of the table, and with an engine using 10 lbs. of coal per hourly horse-power. The scale of this line is ten times that of the other fan curves, and $\frac{1}{300}$ of that of the line showing cost of ventilation by chimney.

From the foregoing we may conclude that under nearly all conditions, so far as the economical expenditure of energy is concerned, chimney ventilation is much more expensive than fan ventilation. With only ten degrees difference in temperature, a chimney calls for 7 times the expenditure of energy of a Blackman fan at its worst, areas being equal, and 360 times that of a centrifugal fan at its best. But wherever the air is already heated for other purposes, of course the chimney is cheapest, and also where the cost of attendance would exceed the difference in cost of heat used.

Mr. Nagle.—I would like to ask Mr. Babcock how the efficiency of the chimneys is affected by the diameter, the height, and other conditions remaining the same? Has that no effect on the efficiency?

Mr. Babcock.—Prof. Trowbridge expressly stated in his paper that he did not consider that question in his problem, as the friction in the passage through the flue would be practically the same for the two modes. Under the conditions assumed by me, it would be less for the fans and would increase their relative efficiency.

CCXIII.

EXPERIMENTS ON THE TRANSMISSION OF POWER BY BELTING.

MADE BY MESSRS. WM. SELLERS & CO.

PRESENTED BY WILFRED LEWIS, PHILA., PA.

THESE experiments were undertaken with a view to determine, under actual working conditions, the internal resistances to be overcome, the percentage of slip, and the co-efficient of friction on belt surface. They were conducted, during the spring of 1885, under the direction of Mr. J. Sellers Bancroft, in a manner similar to the experiments on gearing, already reported to this society.*

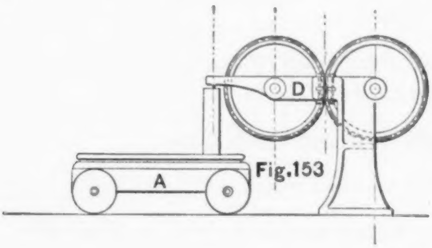
The apparatus used is represented in plan by Fig. 151, and in elevation by Figs. 152 and 153. Power is received by the pulley *P*, and transmitted through the dynamometer *D*, to the belt to be tested. This dynamometer is constructed, as already described in the previous paper, so that its own resistance will not be registered by the weighing apparatus. In these experiments, however, the error due to stiffness in the universal joints was duly measured and allowed for.

The belt to be tested is shown as driving the shaft *L M* from the shaft *H G* upon pulleys of equal size. The power transmitted to *L M* is absorbed by the brake *B*, and measured by the scales upon which it rests at a fixed distance from the center of the shaft. The diameter of the pulleys being known, it is a matter of simple proportion to determine from the reading of the scales *B*, the difference $T - t$ of the tensions upon each side of the belt.

In order to measure the total pull or sum of the tensions $T + t$, the shaft *G H* is mounted in the bell-crank frame *K*, one end of which is supported upon knife-edges bolted securely to the floor, and the other suspended from a frame-work resting upon plat-

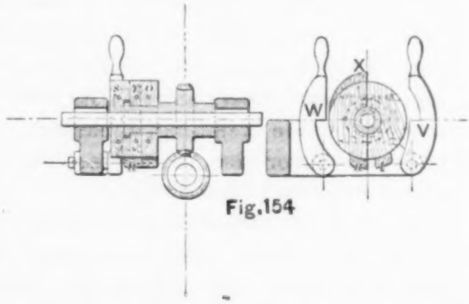
* No. CXCVIII, Vol. VII., p. 273, Transactions A. S. M. E.

form scales. From the position of the arms of the bell-crank lever, it will be seen that .4 of the tensions should be measured by the scales C. The pull of the belt naturally tended to reduce the weight resting upon these scales, and, to prevent the possibility of overturning the frame K, it was heavily loaded beyond the strength of the belts to be tested. From simultaneous observations at B and C, it became possible to know the tension upon each side of the belt while doing work. At the same time, the difference between the readings of A and B showed the loss from internal resistances.



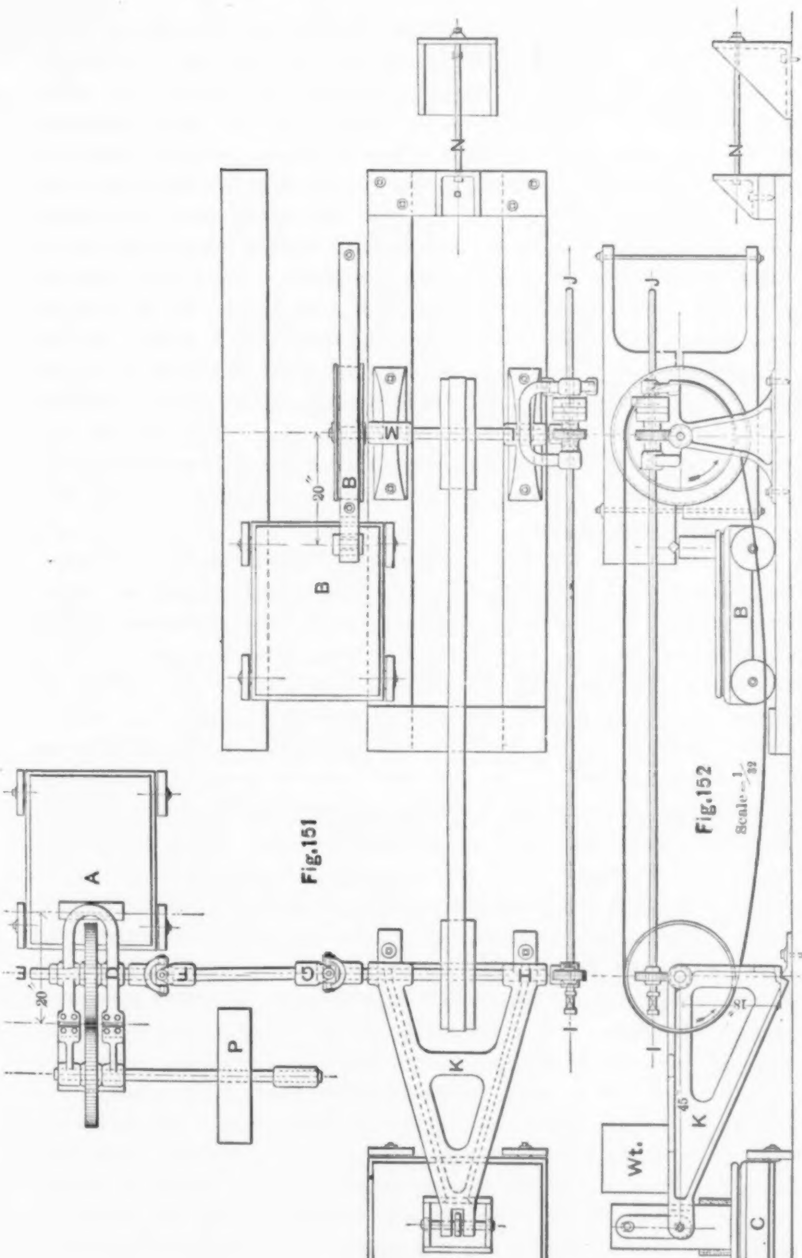
It should be observed that the shaft L M, together with the brake and scales B, are mounted upon a movable frame-work sliding between guides on the floor, so that by means of the bolt N, the initial tension upon the belt could be adjusted at pleasure.

There is still another important item to be determined upon which the efficiency of belt transmission largely depends, namely, the percentage of slip, and this was measured by an apparatus of special design attached to the shaft $I J$, the details of which are shown on a larger scale in Fig. 154. The shaft I J is driven positively by a worm on the end of the shaft G H, and the end I is supported at this point by an outside stand not shown in the drawing.



The shaft L M also carries a worm of the same pitch gearing with a wheel of the same number of teeth. This wheel, however, is loose upon the shaft $I J$, and carries upon its hub the wooden disc o, which is made in halves and clamped to it by gentle friction.

The wooden discs r and s are fastened together as shown, but the disc s is clamped tightly to the shaft $I J$, and the disc r



EXPERIMENTAL APPARATUS TO DETERMINE POWER & EFFICIENCY OF BELT TRANSMISSION

is adjusted to move with gentle friction on the hub of the worm-wheel. The disc *s* is provided with the lug *x*, which is caught by the levers *W* and *V*, checking its motion and causing the shaft *I J* to slip under the friction of the clamping pressure. The disc *r* also carries a lug *u*, which projects over the disc *o*, and forms a stop for the lug *t* carried by it. Suppose, now, that the stop *x* rests against *W*, while the apparatus is in motion, then the wheel *o* will be carried forward by the friction on hub of worm-wheel until its stop *t* comes against *u*, and this position gives the zero from which all readings are taken. If the stop *W* be removed, the discs will revolve together until *x* strikes against *V*, and then, if the friction of the disc *s* on the shaft be great enough, as it should be, the whole system will be lifted until the shaft *I J* strikes the top of its slotted bearing. This throws the worm wheel out of gear and stops the motion of everything except the shaft *I J*, which, by its friction, continues to hold the worm wheel out of gear.

The friction of the disc *r* is abundantly sufficient to overcome the friction of the shaft *I J* in the worm-wheel, and at the same time prevent any change in the relative positions of *r* and *o* after striking the stop *V*. It will thus be seen that the discs *r* and *s* are driven by the shaft *I J* through the friction of *s*, which is powerful enough to lift one end of the shaft when the stop *x* strikes against *V*, and that the disc *o* is driven by its friction on hub of worm-wheel, while the disc *r* serves only to retain the worm-wheel in the position which it held when lifted out of gear. The disc *s* grips the shaft *I J* very tightly, and the friction of *r* on hub of worm-wheel is made comparatively light, so that *s* never slips upon the shaft except when resting against one of the stops. It then follows from this construction that when the stop *x* strikes *W*, the disc *o* will be carried forward until *t* strikes *u*, and that all the discs will slide then under the clamping pressures applied to them. When the stops *W* and *V* are both withdrawn, the disc *r* rotates at a speed proportional to that of the shaft *H G* and the disc *o* at a speed proportional to that of the shaft *L M*. The pulleys upon these shafts were selected in all cases to be as near as possible the same size. There was always a slight difference, however, not often more than $\frac{1}{32}''$, and in order to measure the effect of this upon the slip counter, it was necessary to place the larger pulley on the shaft *L M*. With a tight belt and no load upon the brake *B*, the relative diameters of the two

pulleys could be measured in this way within one-tenth of one per cent., and duly allowed for in estimating slip in other experiments. The discs r and o were graduated so that their relative positions could be read by a vernier.

There were 40 teeth in the worm-wheels, and the shaft IJ would necessarily make $2\frac{1}{2}$ revolutions for 100 revolutions of the shaft HG, and to measure the percentage of slip, it was simply necessary to pull back the stop W and allow the disc s to make two revolutions before throwing in the stop V. After striking the stop V, the percentage of slip could be read off by the operator at his leisure. Then, by removing the stop V and presenting the stop W, the discs became automatically adjusted to zero for another experiment. One revolution of the graduated disc represented 40 per cent., and on a circumference of 20 in. the graduations were naturally $\frac{1}{2}$ in. apart. It was easy to read one-tenth of this by a vernier, and it would have been quite possible, had it been thought necessary, to have extended the refinement to one-hundredth of one per cent. It might have been better, as a matter of practical convenience, to have had the disc r divided into 200 instead of 40 parts, so that but one-half of a revolution would have been needed to measure the percentage of slip. The number of teeth in the worm-wheels was, of course, unimportant, but in this case it happened to suggest an easy method of graduating the disc r, and at the same time of bringing the stops W and V on opposite sides, as desired.

To obtain zeros for the readings of A and C, the apparatus was simply run at the proper speed, with the test belt thrown off and the scales adjusted by small weights to balance at zero while the shaft HG was in motion. A very small amount of power was required to operate the slip-counter when the stops W or V were thrown in to engage with the stop x, but not sufficient, however, to deserve notice as a correction. It was only enough to deflect the scale beam of the dynamometer without requiring a change of reading, and when the stops were thrown out the deflection was imperceptible for the power required to run the shaft IJ. Whenever convenient, the readings of A and C were taken, with the stops removed to insure accuracy, although the precaution was not necessary or important to the general results.

Having adjusted the scales A and C for any desired speed, the test-belt was put on and the brake B removed. The apparatus was again put in motion, and, by means of the bolt N, the tension

upon the belt was gradually increased until its working limit was supposed to be reached. Readings were taken by the dynamometer *A* as the tension was increased, and thus the internal resistances due to journal friction, resistance of the air and stiffness of the belt, could be accurately measured for the different tensions and speeds. As usual, there was considerable variation in the results, arising from variations in the lubrication, for which no standard condition could be fixed.

These experiments seemed to show that the principal resistance to straight belts was journal friction, except at very high speeds, when the resistance of the air began to be felt. The resistance from stiffness of belt was not apparent, and no marked difference could be detected in the power required to run a wide double belt or a narrow light one for the same tension at moderate speeds. With crossed and quarter-twist belts the friction of the belt upon itself or upon the pulley in leaving it was frequently an item of more importance, as was shown by special experiments for that purpose.

In connection with the experiments upon internal resistances, some interesting points were noted. Changes in tension were made while the belt was running, commencing with a very slack belt and increasing by definite amounts to the working strength. As this point was approached, it was found necessary, to maintain a constant tension, that the bolt *N* should be constantly tightened on account of stretch in the belt. Then, again, as the tension was reduced from this limit, it was found that at lower tensions the belt would begin to shrink and tighten for a fixed position of the sliding frame. This stretching and tightening would continue for a long time, the tightening being, of course, limited, but the stretching indefinite and unlimited.

Experiments were made to determine the ratio of stress and strain for different tensions, but these were so seriously affected by changes from stretching and shrinking that it was found impossible to give definite values without the introduction of a time limit, and this was rather beyond the scope of our work. The method of procedure consisted in taking the tensions after a given number of turns of the adjusting nut in ascending and descending progression, and although the results could not be formulated, enough was learned to show, that, in all probability, leather is more elastic under light tensions than it is under high ones.

The first series of experiments was made upon paper coated pulleys 20″ diameter, which carried an old $5\frac{1}{2}''$ open belt $\frac{3}{16}''$ to $\frac{1}{4}''$ thick and 34 ft. long weighing 16 lbs. The arc of contact on the pulleys has been calculated approximately from the tension on slack side, and for this purpose the width and length of the belt were taken. The percentage of slip must be considered as equally divided between the two pulleys, and from observations made it is easy to calculate the velocity of sliding when the speed is given.

Some of the most important results obtained with this belt are given in Table I. in which the experiments have been selected to avoid unnecessary repetition. In all cases, the co-efficient of friction is shown to increase with the percentage of slip. The adhesion on the paper-covered pulleys appears to be greater than on the cast-iron surfaces, but this difference may possibly have been due to some change in the condition of the belt surfaces.

After a fresh application of the belt dressing known as "Beltilene," the results obtained are even higher on cast-iron than on paper surfaces, but after a time it was found that the adhesive property of this substance became sensibly less and less. Flakes of a tarry nature rolled up from the belt surface and deposited themselves on the pulleys, or scaled off.

So much was found to depend upon the condition of the belt surface and the nature of the dressing used, that the necessity was felt for experiments upon some standard condition which could be easily realized and maintained. For this purpose a belt was taken from a planing machine when it had become perfectly dried by friction. The results of experiments upon this belt are given in Table II. When dry, as used on the planer, the co-efficients for any given percentage of slip were much smaller than those given in Table I. This was naturally to be expected, and the experiments were continued to note the effect of a belt dressing in common use, known as "Sankey's Life of Leather," which was applied to the belt while running. At first, the adhesion was very much diminished, but it gradually increased as the lubricant became absorbed by the leather, and in a short time the coefficient of friction had reached the unprecedented figures of 1.44 and 1.37.

An interesting feature of these and subsequent experiments is the progressive increase in the sum of the belt tensions during an increase in load. This is contrary to the generally accepted theory

that the sum of the tensions is constant, but it may be accounted for to a large extent by the horizontal position of the belt, which permitted the tension on the slack side to be kept up by the sag. That this is only a partial explanation of the phenomenon, and that the sum of the tensions actually increases as their difference increases for even a vertical position of the belt, will be shown by a special set of experiments. If a belt be suspended vertically, and stretched by uniformly increasing weights, it will also be found that the extension is not uniform but diminishes as the load is increased, or, as already stated, the stress increases faster than the extension. A little reflection will show that when this is the case the tensions must necessarily increase with the load transmitted.

A piece of belting 1 sq. in. in section and 92 ins. long was found by experiment to elongate $\frac{1}{4}$ in. when the load was increased from 100 to 150 lbs., and only $\frac{1}{8}$ in. when the load was increased from 450 to 500 lbs. The total elongation from 50 to 500 lbs. was $1\frac{11}{16}''$, but this would vary with the time of suspension, and the measurements here given were taken as soon as possible after applying the loads. In a running belt the load is applied and removed alternately for short intervals of time, depending upon the length and speed of the belt, and the time for stretching would seldom be as great as that consumed in making the experiments just mentioned.

The differences between the initial and final tensions unloaded, as given in the tables, show the effect of extension or contraction during the course of the experiments made at a fixed position of the pulleys. The percentage of elongation which a belt undergoes in passing from its loose to its tight side, is the measure of the slip which must necessarily take place in the transmission of power. This is a direct loss and within the assumed working strength of 500 lbs. per sq. in. for cemented belts without lacings, experiment indicates that it should not exceed $1\frac{1}{2}$ or 2 per cent. When, therefore, an experiment shows less than 2 per cent. of slip, the amount may be considered as allowable and proper, and the belt may be relied upon to work continuously at the figures given.

Table III. gives the results of experiments upon a soft and pliable rawhide belt made by the Springfield Glue and Emery Co. This belt had been used by the Midvale Steel Co. for a period of seven months at its full capacity, and was sent in its usual working condition to be tested. It had been cleaned and dressed with

castor oil at intervals of three months and was received three weeks after the last dressing. Commencing with the light initial tension of 50 lbs. on a side, it was found impossible with the power at command to reach a limit to the pulling power of the belt, and in order to do so the experiment was made of supporting the slack side of the belt upon a board to prevent sagging.

These experiments, however, are subject to an error arising from the friction of the belt upon the board, the amount of which was not determined. All of the experiments, in fact, are subject to slight errors which were extremely difficult to eliminate or properly allow for, but an effort has been made throughout to obtain results which should approximate as closely as possible to the truth. The sum of the tensions, as measured by the scales C, was subject only to errors in observation. This part of the apparatus was carefully tested by a horizontal pull of known amount and made to register correctly.

The difference of the tensions $T-t$, as computed from the reading of the scale B, was measured by the force of an equivalent moment at 20″ radius. This moment, divided by the radius of the pulley, was taken to be the difference $T-t$.

In this calculation, it will be noticed that two slight corrections have been omitted which are opposite in effect and about equal in degree. One is the friction of the brake shaft in its bearings, which of course was not recorded on the scales, and the other is the thickness of the belt which naturally increases the effective radius of the pulley. Both of these errors are somewhat indefinite, but the correctness of the results obtained was tested in a number of cases by the sag of the belt, and the tension t, as calculated from the sag, was found to agree closely with the tension calculated by the adopted method.

As the limiting capacity of the belt was reached, the difficulty of obtaining simultaneous and accurate observations was increased by the vibrations of the scale beams. This was apparently due to irregularity in the slip, and it was only by the use of heavily loaded beams and a dash-pot that readings could then be taken at all. The dash-pot consisted of a large flat plate suspended freely in a bucket of water by a fine wire from the scale beam. This provision, however, was applied only to the scale C, on which the vibrations were more pronounced.

A peculiar and important feature of Tables III. and IV. is the effect of time upon the percentage of slip. In previous experi-

ments the percentage of slip was measured at once after the load was applied, but it was accidentally discovered that repeated measurements seldom agreed, and investigation showed that these discrepancies were principally due to the duration of the experiment. The continual slipping of the belt was found to cause a deposit of a thick black substance upon the surface of the pulley, which acting as a lubricant, continued to increase the slip still further.

Upon removing the load on brake-wheel, this deposit would be again absorbed by the belt, and the original adhesion would be restored. The temperature was also found to affect the slipping, and, in general, the colder the weather the slower would this deposit take place.

Experiments 353 to 360 inclusive were made to determine the limit at which the belt would run continuously without increasing its percentage of slip. After the pulleys had become well coated and the slip had reached a high per cent. the load on the brake-wheel was gradually removed until a marked improvement was reached, as shown by experiments 359 and 360. The highest allowable co-efficient of friction for this belt is therefore estimated to be somewhere between 1.13 and .995, or we may safely say 1. The highest co-efficient obtained was 1.67, but, of course, this was temporary. The diameter of the pulley also appears to affect the co-efficient of friction to some extent. This is especially to be noticed at the very slow speed of 18 revolutions per minute on 10 in. and 20 in. pulleys, where the adhesion on the 20 in. pulleys is decidedly greater; but, on the other hand, at 160 revolutions per minute the adhesion on the 10 in. pulleys is often as good, and sometimes better, than appears for the 20 in. at the same velocity of sliding.

It might be possible to determine the effect of pulley diameter upon adhesion for a perfectly dry belt, where the condition of its surface remains uniform, but for belts as ordinarly used it would be very difficult, on account of the ever-changing condition of surface produced by slip and temperature. It is generally admitted that the larger the diameter the greater the adhesion for any given tension, but no definite relation has ever been established, nor, indeed, does it seem possible to do so except by the most elaborate and extensive experiments.

It should be observed, however, that such a variation, if true, implies a corresponding variation in the co-efficients of friction for

different intensities of pressure upon the same pulleys, and that, consequently, our experiments should show higher co-efficients under the lighter loads for the same velocity of sliding. Referring to Table II., where the condition of the belt is dry and uniform for a large range of tensions, we find that this inference is generally sustained, although there are some few exceptions.

Experiment 106 may be compared with 116 and 112 with 133, also 108, 113, and 135, all showing great reductions in the co-efficients of friction for increments in tension. The exceptions are all to be found under the smallest velocities of sliding, and appear only in the third decimal place, so that the weight of their record against the probability of such a law is light. By a similar inference it should also follow that a wide belt would drive a little more at a given tension than a narrow one, on account of the reduction in pressure per square inch against the pulley. The mean intensity of pressure of a belt against its pulley may be considered as proportional to the sum of the tensions divided by the product of pulley diameter and width of belt, and an analysis of the experiments referred to will show the relation there existing between intensity of pressure and co-efficient of friction.

If we let I = intensity of pressure, and φ = co-efficient of friction, we shall find that φ is approximately proportional to $I^{-.15}$, or, in other words, that doubling the width of belt or diameter of pulley would apparently increase the co-efficient of friction about 10 per cent. of its original value. This relation is not proved, of course, and it is given only as a suggestion toward the solution of the question. If the co-efficient of friction does vary with the intensity of pressure, the problem of determining the driving power of a belt on strictly mathematical principles will indeed be complicated.

The co-efficient of friction in the tables has been calculated by a well-known formula, developed upon the assumption of a uniform co-efficient around the arc of contact, but this could no longer be considered as correct if the co-efficient is known to vary with the pressure. Referring from Table II. to Table III., we shall find at once the proof and contradiction of the inferences drawn from Table II., and we are left as much in the dark as ever respecting the value of pressure intensity.

Practical millwrights all know, or think they know, that an increase of pulley diameter increases the drive, and it is a matter of common observation that when large and small pulleys are

connected by a crossed belt, the smaller pulley will invariably slip first.

On one side a great deal of testimony can be adduced to show that pressure intensity should be an important factor in the theory of belt transmission, and, on the other hand, we have strong evidence to the contrary. I may refer, in this connection, to the experiments of Mr. Holman in *Journal of Franklin Institute* for September, 1885, in which there is no indication that the co-efficient of friction varies at all with the pressure. The co-efficients obtained by Mr. Holman follow the variations in slip like our own, and it gives us pleasure to observe that our general results and conclusions are so strongly corroborative of each other. There is at the same time a great difference in the methods pursued in arriving at the same results. In his experiments, the velocity of sliding was the fixed condition upon which the co-efficient of friction was determined, while, in ours, the conditions were those of actual practice in which the percentage of slip was measured. Our least amount of slip, with a dry belt running at the extremely slow speed of 90 feet per minute, was 1.08 inches, and ten times this would be perfectly proper and allowable. A great many of Mr. Holman's experiments are taken at rates below 1″ per minute, and the co-efficients obtained are very much below the average practice, as he himself seems to believe.

The velocity of sliding, which may be assumed in selecting a proper co-efficient is directly proportional to the belt speed, and may safely be estimated at .01 of that speed. For a pair of pulleys we should have .01 on each pulley and therefore .02 for slip. Few belts run slower than 200 or 300 ft. per minute, and consequently a slip of less than 2 or 3 ft. per minute need seldom be considered. Another point of difference which may possibly affect the co-efficients obtained, is that, in Mr. Holman's case, the same portion of belt surface was subject to continuous friction, while in ours, the friction was spread over the belt at successive portions as in actual work. This we consider a new and important feature of our experiments. As a matter of practical importance, care was taken to observe, as nearly as possible, the maximum slip which might safely take place before a belt would be thrown from its pulley. A number of observations taken throughout the experiments led to the final conclusion that 20 per cent. of slip was as much as could safely be admitted. This information has been found of value in cases where work is done

intermittently by a fly-wheel and the belt has to restore the speed of the wheel. It cannot be said in regard to a maximum value of φ that any was determined or even indicated, although it is certain that the increase at high rates of slip becomes less rapid.

We have now seen that the driving power of a leather belt depends upon such a variety of conditions, that it would be manifestly impracticable if not impossible to correlate them all, and it is thought better to admit the difficulties at once than to involve the subject in a labyrinth of formulæ which life is too short to solve.

The relative value of pulley diameters may vary with different belts, and all that can be expected or desired is some general expression covering roughly the greatest number of cases. Our apparatus did not admit of extensive variations in this respect, and our attention was given principally to the question of slip.

The co-efficients given in Table III. are remarkably high, and show a great superiority for the rawhide over tanned leather in point of adhesion. The belt in question was very soft and pliable, but a little twisted from use on a cone pulley where it had rubbed against one side. It is not desirable, on account of its soft and adhesive nature, to use this kind of belt where frequent shifting is required, and when used on cone pulleys it is liable to climb and stretch against the side of the cone; but for a plain straight connection, there seems to be little room for improvement. Table IV. contains the results of similar experiments upon an oak-tanned leather belt made by Chas. A. Shieren & Co. Here the co-efficients are much smaller than those given in Table III., and there is quite a marked difference between the co-efficients for 10 in. and 20 in. pulleys.

As before noticed, the outside temperature has its effect, and it is probable that much lower results would have been obtained had the experiments been made in the heat of midsummer. The high co-efficients obtained, together with the rapid increase of tension, show that the pulling power of a long horizontal belt must, in many cases, be limited by its strength rather than by its adhesion.

Table V. gives the results of experiments upon a light planer belt at very slow and very high speeds. As would naturally be expected, much higher co-efficients were found at the high speed on account of the greater velocity of sliding.

It may here be mentioned that the sum of the tensions was the horizontal pressure of the belt against the pulleys, and that no allowance was necessary for the effect of the centrifugal force. At the speed here used, the tension indicated in the belt at rest was about 50 lbs. greater than when in motion.

The conclusion to be drawn from this series of experiments is the great importance of high speed in the economy of belt transmission. The friction of belts on pulleys is evidently dependent on the velocity of sliding, and, as a general rule, the greater the velocity the greater the friction. There are but few apparent exceptions to this rule, and investigation of them has led to the inference that in all such cases, the condition of the belt or pulley surface had undergone a change either by heating or by deposit from the belt on the pulley. The percentage of slip is the measure of the power lost in transmission by the belt itself, and the higher the speed the less this becomes. There is a limit, however, to the power which may be transmitted as the speed is increased, and this limit is caused by the reduction in pressure against the pulley arising from the action of centrifugal force.

This point has been clearly demonstrated in a paper read before this Society by Mr. A. F. Nagle on the "Horse-Power of Leather Belts," * and the formula there developed is written thus:

$$HP = CVtw\,(S - .012\,V^2) \div 550 \quad . \quad . \quad . \quad . \quad (1.)$$

in which C is a constant to be determined from the arc of contact and co-efficient of friction as expressed in the equation:

$$C = 1 - 10^{-.00758 f \alpha} \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

V = velocity of belt in feet per second.
t = thickness of the belt in inches.
w = width " "
S = working strength of leather in lbs. per square inch.
f = co-efficient of friction.
α = arc of contact in degrees.

The velocity at which the maximum amount of power can be transmitted by any given belt is independent of its arc of contact

* Transactions A. S. M. E., Vol. II., page 91. See also Mr. Nagle's Tables I., II., and III., in Appendix VI. to this paper, for values of C. and $H. P.$

and co-efficient of friction, and depends only upon the working strength of the material and its specific gravity.

From equation (1.) we obtain for the maximum power of leather belts the condition:

$$V = \sqrt{28S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

and for any other material whose specific gravity is y, we find

$$V = 5\sqrt{\frac{S}{y}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

The co-efficient of friction .40, adopted by Mr. Nagle, appears from these experiments to be on the safe side for all working requirements, except in cases where dry belts are run at slow speeds.

If we assume 2 per cent. as the greatest allowable slip, and select within this limit the co-efficient corresponding to the nearest approximations to it, we can form some idea of the co-efficients which can be relied upon at different speeds.

Table VI. gives the average results obtained for this maximum allowance of slip, and shows an extreme variation in the co-efficient of friction from .251 for a dry oak-tanned belt at the slow speed of 90 feet per minute to 1.38 for a rawhide belt at the moderate speed of 800 feet per minute.

For continuous working, it is probable that the co-efficient 1.38 is too high, but still it is certain that a co-efficient of 1.00 can be steadily maintained for an indefinite length of time, and we may say that in actual practice the co-efficient of friction may vary from .25 to 1.00 under good working conditions. This extreme variation in the co-efficient of friction does not give rise, as might at first be supposed, to such a great difference in the transmission of power. It will be seen by reference to formula (1.) that the power transmitted for any given working strength and speed is limited only by the value of C, which depends upon the arc of contact and the co-efficient of friction.

For the usual arc of contact 180°, the power transmitted when $f = .25$ is about 24 per cent. less than when $f = .40$, and when $f = 1.00$, the power transmitted is about 33 per cent. more, from which it appears that in extreme cases the power transmitted may be $\frac{1}{4}$ less or $\frac{1}{3}$ more than will be found from the use of Mr. Nagle's co-efficient of .40.

The percentage of slip is the most important factor affecting the

efficiency of belt transmission, but in addition to this we have journal friction, the resistance of the air, and with crossed belts the friction of the belt upon itself. These have been termed internal resistances, and their values for some of the most common arrangements of pulleys are given in Table VII. From this table it appears that the moment required to run a straight belt varies from 15 to 25 inch lbs. at 100 lbs. tension for all speeds. At 160 revolutions per minute and 1,000 lbs. tension, the required moment varied from 45 to 90 inch lbs., and at 18 revolutions per minute and the same tension it varied from 80 to 160 inch lbs.

From the average of these quantities we find the moment of resistance to be expressed by the following formulæ for straight open belts between 2″ journals:

At 160 r. p. m.:

$$M = .053\,S + 14.7 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

At 18 r. p. m.:

$$M = 11.\,S + 9 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

in which

M = moment of resistance in inch lbs.
S = sum of tensions.

When a crossed belt does not rub upon itself, the resistance is the same as for an open belt.

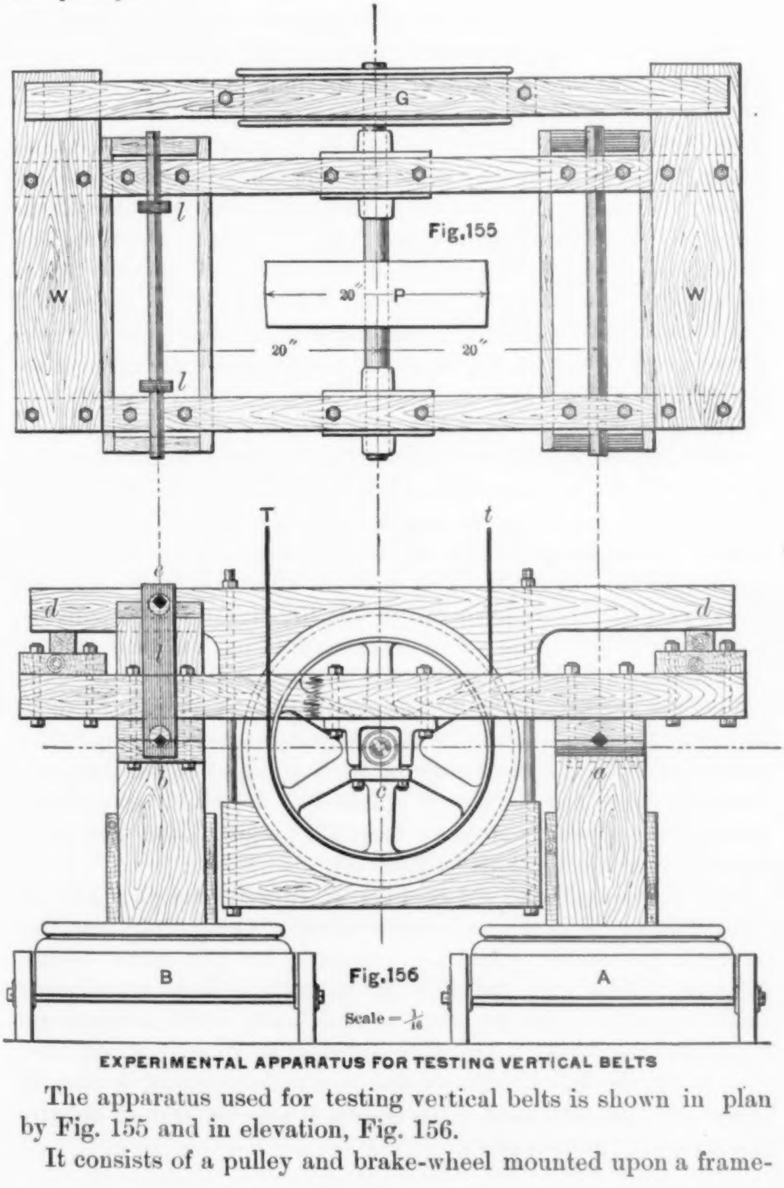
The resistance offered by the introduction of carrying pulleys and tighteners is appreciable, and depends upon the pressure brought to bear against their journals. If the belt rubs against the flanges of the carrying pulleys, the resistance is very much increased, and this is often liable to occur in horizontal belts from a change of load. The friction on journals of carrying pulleys may be estimated by the formulæ already given if we substitute for S the pressure against their journals. In the experiments which were made upon internal resistances, the greatest resistance was offered by a quarter-twist belt 6 feet between journals on 20-inch pulleys.

The equation for this belt may be written:

$$M = .35\,S + 58 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

but the introduction of a carrying pulley reduced the resistance to no more than what might be expected from the same number of journals with a straight belt.

With quarter-twist belts the resistance lies chiefly in slip, which occurs as the belt leaves the pulleys, and this naturally depends upon the distance between journals in terms of the diameters of the pulleys.



EXPERIMENTAL APPARATUS FOR TESTING VERTICAL BELTS

The apparatus used for testing vertical belts is shown in plan by Fig. 155 and in elevation, Fig. 156.

It consists of a pulley and brake-wheel mounted upon a frame-

work resting upon knife-edges which are supported by the scales, *A* and *B*. The knife-edge at *a* is supported directly by suitable blocking, while that at *b* is suspended by the links *l*, in order that the scales may act freely without any disturbing side strains. The frame-work is intended to be heavily loaded by weights applied at *W*, and the scales balanced before the belt is put on.

It is evident that the sum of the tensions $T+t$ is always measured by the reduction in weight upon the scales *A* and *B*, and from the dimensions given, it is also evident the difference in tensions $T-t$ is measured by twice the difference between the readings of these scales. The brake can be supported by the blocks *d* entirely free of the brake-wheel, or, by means of the clamping bolts, it can be made to produce any desired load upon the belt to be tested. In this way the sum and difference of the tensions *T* and *t* were measured for the greatest and least amount of power which could be transmitted, and the results of the experiments are recorded in Table VIII.

The theory before alluded to, that the sum of the tensions remains constant, is directly controverted by these experiments, and it can now be positively asserted that in all cases an increase in load is accompanied by an increase in the sum of the tensions. Apparent exceptions to this rule may take place from a change in the tensions unloaded, but this does not affect the truth of the assertion. The percentage of increase in the sum of the tensions has been calculated in each set of experiments from the final sum of the tensions unloaded, and, for the maximum power of the belt, the results are remarkably uniform and show an increase of about 33 per cent. The amount of this increase in tension depends a great deal upon the co-efficient of friction, which in this case was evidently high, and for a dry planer belt it would certainly be much less, probably not more than .15, judging from the maximum ratio of the tensions in Table II.

If it were possible to obtain a set of experiments showing the ratio of stress and extension under actual working conditions, this increase in tension could be calculated for any given co-efficient of friction, but the difficulty in making such experiments would be to eliminate the effect of time, upon which so much appears to depend. It is probably sufficient to know that, with vertical belts, the increase in tension can seldom exceed $\frac{1}{3}$, while in horizontal belts of considerable length, it may be limited only by the strength of the leather.

The effect of time upon the tension of the belt used in Table VIII. is plainly shown by experiments 588 to 613 inclusive, between which the pulleys remained at a fixed distance apart, and the belt slowly stretched from a tension of 380 to 280 lbs.

To estimate the efficiency of belt transmission for an average case, we may assume 40 in. lbs. as the moment of internal resistance for a belt whose tension is 500 lbs. and 40 in. lbs. statical moment = about 20 ft. lbs. per revolution. If the belt is transmitting 400 lbs. with two per cent. of slip on 20 in. pulleys, then $.02 \times 400 \times 5 = 40$ ft. lbs. are lost per revolution in slip, making a total loss of 60 ft. lbs. per revolution.

The total power expended per revolution is about 2,000 ft. lbs., therefore .03 is lost.

Under light loads, the internal resistance, which is nearly constant in amount, may be a large percentage of the power transmitted, while under heavy loads the percentage of slip may become the principal loss.

It would be difficult to work out, or even to use, a general expression for the efficiency of belt transmission, but, from the foregoing, it would seem safe to assume that 97 per cent. can be obtained under good working conditions.

When a belt is too tight, there is a constant waste in journal friction, and when too loose, there may be a much greater loss in efficiency from slip. The allowance recommended of 2 per cent. for slip is rather more than experiment would indicate for any possible crawl or creep due to the elasticity of the belt, but in connection with this, there is probably always more or less actual slip, and we are inclined to think that in most cases this allowance may be divided into equal parts representing creep and slip proper. Under good working conditions, a belt is probably stretched about 1 per cent. on the tight side, which naturally gives 1 per cent. of creep, and to this we have added another per cent. for actual slip in fixing the limit proposed.

The indications and conclusions to be drawn from these experiments are:

1. That the co-efficient of friction may vary under practical working conditions from 25 per cent. to 100 per cent.

2. That its value depends upon the nature and condition of the leather, the velocity of sliding, temperature, and pressure.

3. That an excessive amount of slip has a tendency to become greater and greater, until the belt finally leaves the pulley.

4. That a belt will seldom remain upon a pulley when the slip exceeds 20 per cent.

5. That excessive slipping dries out the leather and leads toward the condition of minimum adhesion.

6. That rawhide has much greater adhesion than tanned leather, giving a co-efficient of 100 per cent. at the moderate slip of 5 ft. per minute.

7. That a velocity of sliding equal to .01 of the belt speed is not excessive.

8. That the co-efficients in general use are rather below the average results obtained.

9. That when suddenly forced to slip, the co-efficient of friction becomes momentarily very high, but that it gradually decreases as the slip continues.

10. That the sum of the tensions is not constant, but increases with the load to the maximum extent of about 33 per cent. with vertical belts.

11. That, with horizontal belts, the sum of the tensions may increase indefinitely as far as the breaking strength of the belt.

12. That the economy of belt transmission depends principally upon journal friction and slip.

13. That it is important on this account to make the belt speed as high as possible within the limits of 5,000 or 6,000 ft. per minute.

14 That quarter-twist belts should be avoided.

15. That it is preferable in all cases, from considerations of economy in wear on belt and power consumed, to use an intermediate guide pulley, so placed that the belt may be run in either direction.

16. That the introduction of guide and carrying pulleys adds to the internal resistances an amount proportional to the friction of their journals.

17. That there is still need of more light on the subject.

TABLE I.

STRAIGHT OPEN BELT $5\frac{1}{2}''$ WIDE BY $\frac{7}{32}''$ THICK AND 34 FT. LONG, WEIGHING 16 LBS. IN GOOD PLIABLE CONDITION, WITH HAIR SIDE ON PULLEYS 20 IN. DIAM. RUNNING AT 160 R. P. M. OR ABOUT 800 FT. PER MINUTE.

No. of Experiment.	Sum of Tensions. $T+t$ Initial.	Working.	Final.	$T-t$ Working.	T	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Remarks.
17	200	210		100	155	55	2.82	.4	1.6	177°	.336	Paper-covered
19		220		140	180	40	4.50	.6	2.4	176	.490	pulleys.
21		246		180	213	33	6.45	1.2	4.8	175	.610	
22		260		200	230	30	7.67	2.6	10.4	174	.671	
23		270	180	220	245	25	9.80	7.9	31.6	173	.756	
24	300	316		200	258	58	4.45	.7	2.8	177	.483	
27		344		260	302	42	7.20	1.0	4.	176	.643	
28		350		280	315	35	9.00	1.8	7.2	175	.719	
29		364		300	332	32	10.4	2.8	11.2	175	.784	
30		380	260	320	350	30	11.7	5.5	22.	175	.805	
31	400	422		200	211	111	1.90	.5	2.	179	.205	
33		440		280	360	80	4.50	.8	3.2	178	.484	
35		470		360	415	55	7.54	1.1	4.4	177	.654	
36		506		400	453	53	8.54	2.1	8.4	177	.694	
37		520	380	420	470	50	9.40	5.	20.	177	.725	
60	200	205		80	147.5	67.5	2.18	.5	2.	178	.251	Cast-iron sur-
61		210		100	155.	55	2.82	.9	3.6	177	.336	faces.
62		215		120	167.5	47.5	3.52	1.7	6.8	177	.407	
63		220		140	180.	40	4.50	3.	12.	176	.490	
65		246	180	180	213.	33	6.45	12.	48.	175	.610	
66	300	300		120	210	90	2.33	.5	2.	179	.270	
68		310		160	235	75	3.13	.8	3.2	179	.365	
69		315		180	247.5	67.5	3.67	1.	4.	178	.418	
70		320		200	260.	60	4.33	1.7	6.8	178	.472	
71		325		220	272.5	52.5	5.19	2.6	10.4	177	.545	
72		340		240	290	50	5.80	3.8	15.2	177	.569	
73		350		260	305	45.	6.77	5.5	22.	176	.623	
74		360		280	320	40	8.00	8.6	34.4	176	.677	
75		375		300	337.5	37.5	9.00	15.2	60.8	175	.719	
76	400	420		200	310.	110	2.82	.6	2.4	179	.336	Belt dressed
78		460		280	370.	90	4.11	1.	4.	179	.452	with "Beltilene."
81		480		340	410	70	5.86	1.5	6.	178	.569	
84		510		400	455	55	8.27	2.2	8.8	177	.684	
86		535		440	487.5	47.5	10.2	4.5	18.	177	.760	
88		560	385	480	520	40	13.0	8.4	33.6	176	.834	
89	300	320		120	220	100	2.20	.4	1.6	179	.252	
93		350		200	275	75	3.67	.8	3.2	178	.418	
97		390		280	335	55	6.60	1.6	6.4	177	.580	
101		440		360	400	40	10.00	3.1	12.4	176	.750	
104		470	310	420	445	25	17.8	8.6	34.4	173	.953	

TABLE II.

DOUBLE BELT $2\frac{1}{4}''$ WIDE BY $\frac{5}{16}''$ THICK, AND 32 FT. LONG, WEIGHING $9\frac{1}{2}$ LBS. ON 20″ CAST-IRON PULLEYS. THIS BELT HAD BEEN USED ON A PLANING MACHINE, WAS QUITE PLIABLE, DRY, AND CLEAN. 160 R. P. M.

No. of Experiment.	Sum of Tensions. $T+t$ Initial.	Sum of Tensions. $T+t$ Working.	Sum of Tensions. $T+t$ Final.	$T-t$ Working.	T	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Remarks.
105	100	104		40	72	32	2.25	.3	1.2	177	.263	
106		110		60	85	25	3.40	.8	3.2	177	.395	
107		122		80	101	21	4.81	1.7	6.8	176	.511	
108		138		100	119	19	6.26	4.3	17.2	175	.600	
109	200	208		80	144	64	2.25	.4	1.6	179	.260	
110		212		100	156	56	2.81	.7	2.8	179	.331	
111		216		120	168	48	3.50	1.	4.	179	.401	
112		220		140	180	40	4.50	1.8	7.2	178	.484	
113		230		160	195	35	5.57	4.4	17.6	178	.553	
114	300	308		120	214	94	2.28	.4	1.6	180	.262	
116		316		160	238	78	3.05	.8	3.2	180	.355	
118		322		200	261	61	4.28	1.6	6.4	179	.465	
119		330	285	220	275	55	5.	2.6	10.4	179	.516	
121	400	404		160	282	122	2.31	.7	2.8	180	.267	
124		410		220	315	95	3.37	1.5	6	180	.387	
125		412		240	326	86	3.79	2.3	9.2	180	.424	
126		414		260	338	78	4.33	3.7	14.8	179	.469	
127		416	370	280	348	68	5.12	10.1	40.4	179	.523	Belt almost slipped off.
128	500	516		200	358	158	2.27	.5	2.	180	.261	
131		520		260	390	130	3.	1.1	4.4	180	.350	
133		525		300	412.5	112.5	3.67	1.8	7.2	180	.414	
134		525		320	422.5	102.5	4.11	2.7	10.8	180	.450	
135		525	460	340	432 5	92.5	4.67	5.1	20.4	180	.490	
136	100	105		40	72.5	32.5	2.02	.2	.8	177	.228	Here the belt was coated with "Sankey's Life of Leather," and run until in good working condition before noting experiments.
137		110		60	85	25	3.40	.4	1.6	177	.396	
138		125		80	102.5	22.5	4.56	.6	2.4	176	.494	
140		150		120	135.	15.	9 00	1.8	7.2	174	.723	
141		164		140	152	12.	12.7	2.8	10.8	172	.779	
142		180		160	170	10	17.	5.	20.	170	.954	
144		215		200	207.5	7.5	27.7	7.3	29.2	166	1.15	
146		250		240	245	5	49.0	10.6	42.4	158	1.41	
147		270	90	260	265	5	53.	17.7	70.8	158	1.44	
149	100	105		40	72.5	32.5	2.02	.2	.8	177	.228	Three days later without any additional dressing.
150		110		60	85	25	3.40	.3	1.2	177	.396	
151		120		80	100	20	5.00	.4	1.6	176	.524	
153		150		120	135	15	9.00	.7	2.8	174	.723	
155		182		160	171	11	15.5	1.2	4.8	172	.913	
156		202		180	191	11	17.3	3.	12.	172	.950	
157		216		200	208	8	26.0	5.8	23.2	167	1.12	
158		232		220	226	6	37.3	7.	28.	161	1.29	
159		252		240	246	6	41.	9.8	39.2	161	1.32	
161		292		280	286	6	47.7	13.7	54.8	161	1.37	

TABLE III.

RAWHIDE BELT 4″ WIDE BY $\frac{3}{32}$″ THICK AND 31 FT. LONG, WEIGHING 15 LBS. 160 R. P. M. ON 20″ CAST-IRON PULLEYS.

No. of Experiment.	Sum of Ten's. $T+t$ Initial.	Working.	Final.	$T-t$ Working.	T.	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Duration of run at time of experiment.	Remarks.
171	100	118		40	79	39	2.03	.2	.8	177°	.229		
173		149		80	110	30	3.67	.4	1.6	176	.423		
175		168		120	144	24	6.	.6	2.4	174	.590		
177		202		160	181	21	8.62	.8	3.2	172	.661		
179		232		200	216	16	13.5	1.0	4.	170	.897		
181		268		240	254	14	18.1	1.2	4.8	167	.963		
183		302		280	291	11	26.5	1.4	5.6	163	1.15		
184		318	110	300	309	9	34.3	1.6	6.4	160	1.27		
185	100	150	115	140	145	5	29.	1.6	6.4	180°	1.02		Slack side of belt running on a board to prevent sagging.
186	200	258		240	249	9	27.4	1.2	4.8	180	1.05		
188		290		280	285	5	57.	2.2	8.8	180	1.29		
189	300	412		400	406	6	67.7	1.7	6.8	180	1.34		
190		428		420	424	4	106.	1.8	7.2	180	1.48		
191		446	275	440	443	3	148	3.3	13.2	180	1.59		
192	400	570	360	560	565	5	113.	2.	8.	180	1.47		
329	100	110		40	75	35	2.14	.3	.6	177°	.246		10″ cast-iron pulleys.
330		135		80	107.5	27.5	3.90	.6	1.2	175	.446		
331		198		160	179	19	9.42	1.	2.	171	751		
332		275		240	257.5	17.5	14.7	1.5	3	169	.911		
334		345		320	[illegible]	12.5	18.6	2	4.	165	1.01		
336		420	110	400	410	10	41.0	3.2	6.4	162	1.31		
339	200	230		160	195	35	5.86	.8	1.6	176	.576		
340		360		320	340	20	17	1.6	3.2	171	.949		
341		435		400	417.5	17.5	23.8	2.	4.	169	1.07		
342		505		4-0	492.5	12.5	39.4	2.7	5.4	165	1 28		
343		590	200	560	575.	15	38.3	5.	10.	168	1.24		
344	300	400		320	360	40	9	1.4	2.8	175	.719		
345		450		400	425	25	17	1.7	3.4	173	938		
346		520		480	500	20	25	2.1	4.2	171	1.08		
347		600		560	570	10	57	3.	6.	162	1.43	1 min.	
348		600	280	560	570	10	57	3.4	6.8	162	1.43	5 min.	
350	400	500		400	450	50	9	1.6	3.2	176	.715		
351		550		480	515	35	14.7	1.8	3.6	175	.880		
352		605		560	577.5	17.5	21.3	2.3	4.6	169	1.04		
353		680		640	660	20	33	3.2	6.4	171	1.17	1 min.	
354		680		640	660	20	33	3.7	7.4	171	1.17	5 min.	
355		680		640	660	20	33	4.1	8.2	171	1.17	10 min.	
356		680		640	660	20	33	6 1	12.2	171	1.17	15 min.	Belt slipped off 4 m. later.
357		600		560	580	20	29	10.	20.	171	1.13	20 min.	Continuing.
358		600		560	580	20	29	17.2	34.4	171	1.13	25 min.	
359		530		480	505	25	20.2	5.2	10.4	173	.955	30 min.	
360		530	350	480	505	25	20.2	2.8	5.6	173	.955	35 min.	

TABLE III.—(*Continued*).

No of Experiment.	Sum of Ten's. $T + t$			$T - t$ Working.	T.	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Duration of run at time of experiment.	Remarks.
	Initial.	Working.	Final.										
361	500	570		400	485	85	5.71	1.3	2.6	178°	.561		
364		700		640	670	30	22.3	2.3	4.6	174	1.02		
365		755		720	637.5	17.5	36.4	3.2	6.4	169	1.22		
366		820		800	810	10	81.	6.6	13.2	162	1.55		Belt slipped off 2 m. later.
367		750		720	735	15	49.	5.1	10.2	168	1.32	1 min.	
368		750		720	735	15	49.	11.	22.	168	1.32		
369		690		640	665	25	26.6	12.	24.	173	1.09	5 min.	Belt slipped off 3 m. later.
370		610		560	585	25	23.4	14.4	28.8	173	1.05	1 min.	
371		610		560	585	25	23.4	20.	40.	173	1.05	4 min.	
372		550		480	515	35	14.7	7.4	14.8	175	.880	1 min.	
373		550	410	480	515	25	14.7	2.3	4.6	175	.880	5 min.	
374	600	680		480	580	100	5.8	1.5	3.	178	.566		
376		755		640	697.5	57.5	12.1	2.1	4.2	177	.807		
378		850		800	825	25	33.	2.8	5.6	173	1.16	1 min.	
379		850		800	825	25	33.	3.5	7.	173	1.16	5 min.	Belt slipped off 5 m. later.
380		780		720	750	30	25.	8.8	17.6	174	1.06	1 min.	
381		680		560	620	60	10.3	11.2	22.4	177	.755	5 min.	
382		680		560	620	60	10.3	2.	4.	177	.755		After running 5 minutes at $T - t = 560$.
383		730		640	685	45	15.2	2.5	5.	176	.886	1 min.	
384		730		640	685	45	15.2	2.4	4.8	176	.886	5 min.	
385		780		720	750	30	25.	4.6	9.2	174	1.06	1 min.	
388		780	550	720	750	30	25.	8.8	17.6	174	1.06	5 min.	
389		780		720	750	30	25.	4.	8.	174	1.06	1 min.	Belt scraped.
390		780		720	750	30	25.	6.4	12.8	174	1.06	5 min.	Belt slipped off 2 m. later.
391		730		640	685	45	15.2	3.7	7.4	176	.886	1 min.	
392		730	550	640	685	45	15.2	3.9	7.8	176	.886	5 min.	
396	600	680		400	540	140	3.86	2.	.45	179	.432		18 r. p. m. 10″ cast-iron pulleys.
397		820		720	770	50	15.4	17.2	3.87	176	.890		
398		750		640	695	55	12.6	15.	3.37	177	.874		
399		700		560	630	70	9.	9.4	2.17	177	.711		
400		670		480	575	95	6.05	4.5	1.12	178	.579		
401		630	550	400	515	115	4.48	8.5	.75	178	.483		
402		830		720	775	55	14.1	26.	5.85	177	.856		
403		630		320	475	155	3.06	1.5	.30	179	.358		
404		610		60	335	275	1.22	.7	.16	180	.063		
408	600	610		120	365	245	1.49	.2	.09	180	.127		20″ cast-iron pulleys. 18 r. p. m.
413		660		400	530	130	4.08	1.	.45	179	.450		
415		710		560	635	75	8.46	1.9	.86	177	.691		
416		750		640	695	55	12.6	3.2	1.44	177	.820		
417		800		720	760	40	19.	3.8	1.71	175	.964		
418		340		200	274	70	3.91	.6	.27	177	.441		
419	300	380		280	330	50	6.6	1.2	.54	176	.614		
421		450		400	425	25	17.	3.2	1.44	173	.938		
423		515		480	497.5	17.5	28.4	4.	1.8	169	1.13		
425		580		560	570.	10.	57.	5.	2.25	162	1.43		
427		695		680	687.5	7.5	91.7	7.	3.15	155	1.67		

TABLE IV.

DOUBLE OAK-TANNED LEATHER BELT 4'' WIDE BY $\frac{5}{16}$'' THICK AND 30 FT. LONG, WEIGHING 17 LBS. ON 10'' CAST-IRON PULLEYS. 160 R. P. M.

No. of Experiment.	Sum of Ten's. $T + t$ Initial.	Sum of Ten's. $T + t$ Working.	Sum of Ten's. $T + t$ Final.	$T - t$ Working.	T	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Co-effici't of Friction.	Duration of run at time of exp'ment.	Remarks.
209	120	120		48	84	36	2.33	.4	.8	176°	.275		
210		140		80	110	30	3.67	.6	1.2	175	.426		
211		168		120	144	24	6.	.9	1.8	174	.590		
212		198		160	179	19	9.42	1.6	3.2	170	.756		
213		235		200	217.5	17.5	12.4	2.3	4.6	174	.829		
214		270		240	255.	15	17.	3.2	6.4	168	.966		
215		310		280	295	15	19.7	5.1	10.2	168	1.02		Sag 10'' at middle of belt.
216		345	122	320	332.5	12.5	25.8	9.4	18.8	164	1.13		Finally slipped off.
217	200	200		48	124	76	1.63	.4	.8	179	.156		
219		240		160	200	40	5.	1.	2.	176	.524		
220		360		320	340	20	17.	2.7	5.4	170	.954		
221		430		400	415	15	27.7	15.	30.	167	1.13		
222	300	318		160	239	79	3.03	.8	1.6	179	.354		
223		350		240	295	55	5.36	1.2	2.4	177	.543		
224		400		320	360	40	9.	2.	4.	175	.719		
225		470		440	455	15	30.3	8.	1.6	167	1.17		Belt finally slipped off.
226		450		400	425	25	17.	4.	8.	172	.943	1 min.	
227		450		400	425	25	17.	8.	16.	172	.943	5 min.	
228		450		400	425	25	17.	17.3	34.6	172	.943	10 min.	Belt slipped off.
229		418		360	389	29	13.4	3.	6.	173	.859	15 min.	Continuing.
230	400	405		160	282.5	122.5	2.30	.8	1.6	179	.267		
232		455		320	387.5	67.5	5.74	1.4	2.8	177	.566		
233		495		400	447.5	47.5	9.42	1.9	3.8	176	.730	1 min.	
234		495	370	400	447.5	47.5	9.42	2.1	4.2	176	.730	5 min.	
235		560		480	520	40	13.	2.7	5.4	175	.859	Starting.	
236		560		480	520	40	13.	4.5	9.	175	.859	5 min.	
237		560		480	520	40	13.	7.5	15.	175	.859	10 min.	
238		550	380	480	465	85	5.47	20.	40.	178	.547	15 min.	
239	400	560		480	520	40	13.	3.4	6.8	175	.859	1 min.	After running 5 m. without load.
240	500	610		480	545	65	8.38	2.1	4.2	177	.688	1 min.	
241		610		480	545	65	8.38	2.5	5.	177	.688	5 min.	
242		660		560	610	50	12.2	3.2	6.4	176	.814	1 min.	
243		655		560	607.5	47.5	12.8	8.4	16.8	176	.830	5 min.	Belt slipped off 2 m. later.
244	600	700		560	630	70	9.	1.9	3.8	177	.711	1 min.	
245		700		560	630	70	9.	2.1	4.2	177	.711	5 min.	
246		690	556	560	625	65	9.69	2.3	4.6	177	.735	10 min.	
247	600	750		600	675	75	9.	2.2	4.4	177	.711	1 min.	
248		740	585	600	670	70	9.57	2.4	4.8	177	.731	5 min.	
249	600	770		640	705	65	10.8	2.5	5.	177	.770	1 min.	
250		765		640	702.5	62.5	11.2	3.5	7.	177	.782	5 min.	
251		770	600	640	685	85	8.06	4.2	8.4	178	.672	10 min.	
252	600	790		680	735	55	13.4	4.3	8.6	176	.845	1 min.	
253		790		680	735	55	13.4	6.3	12.6	176	.845	5 min.	Belt slipped off 2 m. later. Pulley warm.
254	100	100		44	72	28	2.57	.6	1.2	176	.307		Belt scraped.
256		160		120	140	20	7.	2.1	4.2	172	.648		
257		200		160	180	20	9.	4.	8.	171	.736		
258		230		200	215	15	14.3	6.6	13.2	168	.907	1 min.	
259		230	100	200	215	15	14.3	7.2	14.4	168	.907	5 min.	
261	100	100		44	72	28	2.57	.6	1.2	176	.307		Belt dressed with preparation recommended by maker.
263		160		120	140	20	7.	2.8	5.6	172	.648		
264		200		160	180	20	9.	5.1	10.2	171	.736		
265		230		200	215	15	14.3	7.3	14.6	168	.907	1 min.	
266		230		200	215	15	14.3	7.9	15.8	168	.907	5 min.	
267		270		240	255	15	17.	10.7	21.4	168	.966	1 min.	Belt slipped off 3 m. later.

TABLE IV.—(*Continued*).

No. of Experiment.	Sum of Ten's. $T + t$ Initial.	Working.	Final.	$T - t$ Working.	T	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Duration of run at time of exp'ment.	Remarks.
268	300	350		240	295	55	5.36	1.4	2.8	177	.544		
269		400		320	360	40	9.	3.	6.	175	.719		
270		450		400	425	25	17.	6.8	13.6	172	.943	1 min.	Belt slipped off 3 m. later.
271		418		360	389	29	13.4	8.8	17.6	173	.859	1 min.	
272		418		360	389	29	13.4	15.6	31.2	173	.859	5 min.	Belt slipped off 2 m. later.
273	600	700		560	630	70	9.	6.3	12.6	177	.711		
274		650		480	565	85	6.65	3.1	6.2	178	.610	1 min.	
275		650		480	565	85	6.65	3.9	7.8	178	.610	5 min.	
276		650		480	565	85	6.65	4.4	8.8	178	.610	10 min.	
277	600	652		400	526	126	4.17	1.4	2.8	178	.460		One day later.
279		715		560	637.5	77.5	8.23	2.4	4.8	177	.682		
280		705		560	632.5	72.5	8.72	2.8	5.6	177	.701		
281		700	560	560	630.	70.	9.	3.	6.	177	.711		
282	560	750		640	695	55	12.6	4.1	8.2	176	.824	1 min.	
283		735	535	640	682.5	47.5	14.3	22.	44.	176	.866	5 min.	Belt slipped off.
284		770		640	705	65	10.7	5.4	10.8	177	.767	1 min.	After 3 min. intermission.
285	300	350		240	295	55	5.36	1.2	2.4	177	.543		Temperature 52°.
286		400		320	360	40	9.	1.8	3.6	175	.719		
287		430		360	395	35	11.3	2.7	5.4	174	.798		
289		465		400	432.5	32.5	13.3	5.3	10.6	174	.852		
290		455		400	427.5	27.5	15.5	7.3	14.6	173	.907		
291		460		400	430.	30.	14.3	11.6	23.2	173	.881		
292	100	100		44	72.	28.	2.57	.5	1.	176	.307		
293		125		80	102.5	22.5	4.55	.8	1.6	173	.502		
294		165		120	142.5	22.5	6.33	1.2	2.4	173	.611		
295		200		160	180.	20.	9.	2.1	4.2	171	.736		
296		230		200	215	15	14.3	3.4	6.8	168	.907		
297		230		200	215	15	14.3	3.9	7.8	168	.907		
298	100	270		240	225	15	17.	5.7	11.4	168	.966	1 min.	
299		270		240	255	15	17.	7.6	15.2	168	.966	5 min.	
300		270		240	255	15	17.	9.3	18.6	168	.966	10 min.	Belt slipped off 4 m. later.
303	100	110		40	75	35	2.14	.1	.4	177	.246		20 in. pulleys.
304		132		80	106	26	4.08	.4	1.6	174	.463		
305		160		120	140	20	7.	1.	4.	172	.648		
306		195		160	177.5	17.5	10.1	1.9	7.6	169	.814		
307		230		200	215.	15.	14.3	3.	12.	168	.907	1 min.	
308		230	90	200	215.	15	14.3	3.5	14.	168	.907	5 min.	
309		270		240	255	15	17.	4.5	18.	168	.966	1 min.	
310		270		240	255	15	17.	5.8	23.2	168	.966	5 min.	
311		270		240	255	15	17.	6.2	24.8	168	.966	10 min.	
312		270		240	255	15	17.	6.2	24.8	168	.966	15 min.	Temperature 56°.
313		270		240	255	15	17.	2.	8.	168	.966	1 min.	
314		270		240	255	15	17.	2.1	8.4	168	.966	5 min.	Temperature 42°.
315		305		280	292.5	12.5	23.4	3.4	13.6	165	1.09	1 min.	
316		305	100.	280	292.5	12.5	23.4	3.5	14.	165	1.09	5 min.	
317	100	335		320	327.5	7.5	43.7	5.2	20.8	152	1.42	1 min.	
318		335		320	327.5	7.5	43.7	6.5	26.	152	1.42	5 min.	
319	300	380		320	350.	30.	11.7	1.3	5.2	173	.814	1 min.	
320		380		320	350.	30.	11.7	1.4	5.6	173	.814	5 min.	
321		440		400	420.	20.	21.	2.1	8.4	170	1.03	1 min.	
322		440	260	400	420.	20.	21.	2.4	9.6	170	1.03	5 min.	
323	300	480		440	460.	20.	23.	2.8	11.2	170	1.06	1 min.	Temperature 46°.
324		480	285	440	460.	20.	23.	3.	12.	170	1.06	5 min.	
325		510		480	495.	15.	33.	3.2	12.8	167	1.20	1 min.	[ley warm.
326		510		480	495,	15.	33.	5.	20.	167	1.20	5 min.	Belt slip. off 5 m. lat. Pul-

TABLE V.

OAK-TANNED LEATHER BELT 2″ WIDE BY $\frac{3}{16}$″ THICK AND 30′ 4″ LONG, WEIGHING 4 LBS. ON 20″ CAST-IRON PULLEYS. DRY AND SMOOTH, TAKEN FROM SERVICE ON PLANER.

No. of Experiment.	Sum of Ten's. $T+t$ Initial.	Sum of Ten's. $T+t$ Working.	Sum of Ten's. $T+t$ Final.	$T-t$ Working.	T	t	$\frac{T}{t}$	Percentage of Slip.	Velocity of Slip in ft. per minute.	Arc of Contact.	Coefficient of Friction.	Duration of run at time of exp'ment.	Remarks.
429	100	110		40	75	35	2.14	1.2	.54	179°	.243		18 r. p. m.
430		115		60	87.5	27.5	3.18	6.1	2.75	178	.372		
431		118		70	94	24	3.92	16.5	7.42	178	.440		
432		105		20	62.5	42.5	1.47	.3	.14	179	.123		
433		112		50	81	31	2.61	3.5	1.57	178	.309		
435	200	204		40	132	82	1.61	.2	.09	180	.152		
436		206		60	133	73	1.82	.7	.32	180	.191		
437		208		80	144	64	2.25	1.8	.81	179	.260		
438		210		100	155	55	2.82	3.7	1.66	179	.332		
439		212		120	166	46	3.61	7.7	3.47	179	.411		
440		215		140	177.5	37.5	4.73	18.4	8.28	179	.497		
442	100	110		60	85	25	3.40	.3	7.12	178	.394		950 r. p. m.
443		120		80	100	20	5.	.7	16.62	178	.518		
445		125		90	107.5	17.5	6.14	3.	71.25	177	.587	Starting.	
446		125		90	107.5	17.5	6.14	25.	593.7	177	.587	5 min.	
448	200	200		80	140	60	2.33	.4	9.5	179	.271		
449		200		100	150	50	3.	.5	11.87	179	.352		
450		195	175	120	157.5	37.5	4.29	.8	19.	179	.459		
451	150	175		120	147.5	27.5	5.36	.9	21.38	178	.540		
452	135	160		120	140	20	7.	20.	475.	178	.626		

TABLE VI.

SHOWING THE AVERAGE CO-EFFICIENT OF FRICTION AND VELOCITY OF SLIP FOR A NUMBER OF EXPERIMENTS IN WHICH THE SLIP APPROXIMATED 2 PER CENT.

No. of experime'ts in av'ge.	Perce'age of Slip.	Veloc'y of Slip in ft. per min.	Coeffic'nt of Friction.	Belt.	Pulleys.	Remarks.
3	1.4	5.6	.661	5¼″ old belt. See Tab. I.	20″ diam. pap. covered.	Belt in nor. w'king con.
2	1.7	6.8	.44	5¼″ old belt. See Tab. I.	20″ diam. cast-iron surf.	" " "
2	1.55	6.2	.575	5¼″ old belt. See Tab. I.	20″ diam. cast-iron surf.	Belt dressed with "Beltilene."
5	1.7	6.8	.452	2¼″ dbl. belt. See Ta. II.	20″ diam. cast-iron surf.	B't dry as used on plan'r
2	1.5	6.	.818	2¼″ dbl. belt. See Ta. II.	20″ diam. cast-iron surf.	Belt dressed with "Sankey's Life of Leather."
2	1.7	6.8	1.38	4″ r'hide b't. See Ta. III.	20″ diam. cast-iron surf.	Belt in nor. w'king con.
11	1.8	3.6	.861	4″ r'hide b't. See Ta. III.	10″ diameter...........	" " "
1	2	.45	.432	4″ r'hide b't. See Ta. III.	10″ diameter...........	" " "
1	1.9	.86	.691	4″ r'hide b't. See Ta. III.	20″ diameter...........	" " "
7	1.94	3.88	.617	4″ o.tan'd b. See Ta. IV.	10″ diameter...........	" " "
4	1.85	7.40	.906	4″ o.tan'd b. See Ta. IV.	20″ diameter...........	" " "
2	1.5	.67	.251	2″ o.tan'd b. See Tab. V.	20″ diameter...........	B't dry as used on plan'r
2	.8	38.	.529	2″ o.tan'd b. See Tab. V.	20″ diameter...........	" " "

TABLE VII.

SHOWING THE TORSIONAL MOMENT IN LBS. REQUIRED TO OVERCOME JOURNAL FRICTION AND OTHER INTERNAL RESISTANCES, FOR BELTS AT VARIOUS SPEEDS AND TENSIONS ON DIFFERENT ARRANGEMENTS OF PULLEYS.

No. of Experiment.	Tension. $T + t$	Moment in in. lbs.	Diameter of pulleys.	Revolutions per min.	Width of Belt.	Thickness of Belt.	Manner of Driving.	Remarks.
1	100	20	20″	160	6″	$\frac{7}{32}$″	Straight open belt.	
3	300	25						
5	500	30						
7	700	35						
10	1000	45						
45	100	15						
47	300	22.5						
49	500	27.5						
51	700	35						
54	1000	50						
163	100	17.5	20″	160	4″	$\frac{9}{32}$″	Straight open belt.	
165	300	25						
167	500	30						
169	700	35						
194	100	17.5	10″	160	4″	$\frac{5}{16}$″	Straight open belt.	
196	300	27.5						
198	500	40.						
200	700	55.						
202	900	70.						
203	1000	80.						
327	100	20	10″	18	4″	″	Straight open belt.	
328	1000	80						
393	100	20						
394	1000	100						
395	600	60						
405	100	20	20″	18	4″	$\frac{9}{32}$″	Straight open belt.	
406	1000	160						
407	600	100						
428	100	20	20″	18	2″	″	Straight open belt.	
434	200	25						
441	100	25	20″	950	2″	$\frac{3}{16}$″	Straight open belt.	
447	200	30						
452	100	25	20″	160	6″	$\frac{7}{32}$″	Crossed belt.	14′ 6″ between pulleys.
454	500	60						
455	1000	110						14′ 6″ between pulleys.
459	100	15	20″	160	6″	$\frac{7}{32}$″	Straight open belt.	14′ 6″ between pulleys.
460	500	25						
461	1000	65						
462	100	25	20″	160	6″	$\frac{7}{32}$″	Straight open belt.	With 8″ tightener.
463	500	60						
464	1000	110						
465	100	45	20″	160	6″	$\frac{7}{32}$″	Crossed belt.	8 feet between pulleys.
466	500	105						
467	1000	180						
470	100	25	20″	160	6″	$\frac{7}{32}$″	Quarter turn belt on 16″ diameter mule pulleys.	
471	500	80						
472	750	145						
473	1000	250						
474	750	170						
475	500	110						
476	1000	220						

TABLE VII.—(*Continued*).

No. of Experiment.	Tension. $T+t$	Moment in in. lbs.	Diameter of pulleys.	Revolutions per min.	Width of belt.	Thickness of Belt.	Manner of Driving.	Remarks.
477	1000	140	20″	160	6″	$\frac{7}{32}$″	Quarter turn belt on 16″ diameter mule pulleys.	Freshly oiled.
478	750	100						
479	500	70						
480	100	20						
481	50	60	20″	160	6″	$\frac{7}{32}$″	Quarter turn on 16′ mule pulleys.	Belt rubbing against lower guide of mule pulley.
482	25	120						
483	100	20	20″	160	6″	$\frac{7}{32}$″	Quarter turn on 16″ mule pulleys.	Well oiled, after a run of 2 hrs. at $T+t=100$.
484	500	50						
485	750	70						
486	1000	105						
495	250	30	20″	160	6″	$\frac{7}{32}$″	Half turn belt on 16″ mule pulleys.	
496	500	50						
497	750	90						
498	1000	170						
503	1000	260	20″	160	6″	$\frac{7}{32}$″	Quarter twist.	10 feet between pulleys.
504	750	190						
505	500	180						
506	250	80						
507	100	30						
513	100	50	20″	160	6″	$\frac{7}{32}$″	Quarter twist.	7′ 6″ between pulleys.
514	250	105						
515	500	200						
516	750	290						
517	1000	380						
523	100	25	20″	160	4″	$\frac{1}{4}$″	Quarter twist.	10 feet between pulleys.
524	250	50						
525	500	95						
526	750	145						
527	1000	210						
528	100	65	20″	160	4″	$\frac{1}{4}$″	Quarter twist.	6 feet between pulleys.
529	250	135						
530	500	245						
531	750	380						
533	100	25	20″	160	6″	$\frac{7}{32}$″	Quarter twist.	16′ 6″ between pulleys.
534	250	40						
535	500	75						
536	750	105						
537	1000	165						
539	1000	130	20″	160	6″	$\frac{7}{32}$″	Quarter twist with 16″ diameter carrying pulley.	7′ 6″ between pulleys.
540	750	110						
541	500	90						
542	250	60						
543	100	40						
544	100	30						
545	250	55						
546	500	90						
547	750	120						
548	1000	170						
569	100	25	20″	160	6″	$\frac{7}{32}$″	Straight open belt.	
571	500	55						
572	750	70						
573	1000	90						

TABLE VIII.

SHOWING THE INCREASE IN THE SUM OF THE TENSIONS ON A VERTICAL BELT 4″ WIDE BY $\frac{1}{4}$″ THICK, AND 24 FT. LONG, ON 20″ CAST-IRON PULLEYS AT 120 R. P. M.

No. of Experiment.	Scales A.	Scales B.	Tension $T+t$	Tension $T-t$	T	t	Increment of $T+t$	P'rcentage of Increment.	Date.
578	93	101	194	16	105	89	0		5–15–1885.
579	70	142	212	144	178	34	18		
580	67	170	237	206	221.5	15.5	43		
581	66	180	246	228	237	9	52		
582	66	188	254	244	249	5	60	.323	
583	91	101	192	20	106	86	— 2		
584	202	210	412	16	214	198	0		5–15–1885.
585	167	250	417	166	292.5	126.5	5		
586	145	300	445	310	376.5	66.5	33	.171	
587	185	195	380	20	200	180	—32		
588	190	199	380	0	190	190	0		5–18–1885.
589	143	250	393	214	303.5	89.5	13	.033	
590	177	177	354	0	177	177	0		5–19–1885.
591	156	203	359	94	226.5	132.5	5		
592	138	235	373	194	283.5	89.5	19		
593	135	250	385	230	307.5	77.5	31		
594	128	275	403	294	348.5	54.5	49		
595	125	300	425	350	387.5	37.5	71		
596	123	325	448	404	426	22	94	.333	
597	168	168	336	0	168	168	—18		
598	143	143	286	0	143	143	0		5–25–1885.
599	140	148	288	16	152	136	2		
600	130	160	290	60	175	115	4		
601	122	170	292	96	194	98	6		
602	116	180	296	128	212	84	10		
603	112	190	302	156	229	73	16		
604	108	200	308	184	246	62	22		
605	105	210	315	210	262.5	52.5	29		
606	102	220	322	236	279	43	36		
607	100	230	330	260	295	35	44		
608	99	240	339	282	310.5	28.5	53		
609	98	250	348	304	326	22	62		
610	98	260	358	316	337	21	72		
611	99	270	369	342	355.5	13.5	83		
612	100	280	380	360	370	10	94	.357	
613	140	140	280	0	140	140	— 6		

APPENDIX VI.

The following tables give values for *C.* and *H. P.* calculated from the formulæ of Mr. A. F. Nagle, given in Vol. II., p. 91, Trans. A. S. M. E.

TABLE I.

VALUE OF $C = 1 - 10^{-.00758 f @}$.

f = Co-efficient of friction, @ = degrees of contact.

VALUE OF f	ARC OF SUBTENSION = @.										
	90° .250	100° .277	110° .306	120° .333	130° .361	140° .388	150° .417	160° .444	170° .472	180° .500	200° .555
.15	.210	.230	.250	.270	.288	.307	.325	.342	.359	.376	.408
.18	.246	.270	.292	.314	.335	.356	.376	.395	.414	.432	.466
.20	.270	.295	.319	.342	.364	.386	.408	.428	.448	.467	.503
.23	.303	.331	.357	.382	.406	.430	.452	.474	.495	.514	.552
.25	.325	.354	.381	.407	.432	.457	.480	.503	.524	.544	.582
.28	.356	.387	.416	.444	.470	.496	.520	.542	.564	.585	.624
.30	.376	.408	.438	.467	.494	.520	.544	.567	.590	.610	.649
.33	.404	.438	.469	.499	.527	.554	.579	.602	.624	.645	.684
.35	.423	.457	.489	.520	.548	.575	.600	.624	.646	.667	.705
.38	.449	.485	.518	.549	.578	.605	.630	.654	.676	.697	.735
.40	467	.502	.536	.567	.597	.624	.649	.673	.695	.715	.753
.43	.491	.528	.562	.593	.623	.650	.676	.699	.721	.741	.777
.45	.507	.544	.579	.610	.640	.667	.692	.715	.737	.757	.792
.48	.529	.567	602	.634	.663	.690	.715	.738	.759	.779	.813
.50	.544	.582	.617	.649	.678	.705	.730	.752	.773	.792	.825
.53	.565	.603	.638	.670	.700	.726	.750	.772	.793	.811	.843
.55	.578	.617	.652	.684	.713	.739	.763	.785	.805	.822	.853
.58	.598	.637	.672	.703	.732	.758	.781	.802	.821	.838	.868
.60	.610	.649	.684	.715	.744	.769	.792	.813	.832	.848	.877
1.00	.792	.825	.853	.877	.897	.913	.927	.937	.947	.956	.969

TABLE II.

HORSE POWER OF A LACED LEATHER BELT ONE INCH WIDE.

Formula $HP = C\,V\,t\,w\,(T - .012\ V^2) \div 550$.
For $f = .40$ and $@ = 180^\circ$, $C = .715$; $T = 275$; $w = 1.''$

VELOCITY IN FEET PER SECOND.	THICKNESS IN INCHES = t.						
	$\frac{1}{7}''$ .143	$\frac{1}{6}''$ .167	$\frac{3}{16}''$ .187	$\frac{7}{32}''$ .219	$\frac{1}{4}''$ .250	$\frac{5}{16}''$ .312	$\frac{1}{3}''$ .333
10	.51	.59	.63	.73	.84	1.05	1.18
15	.75	.88	1.00	1.16	1.32	1.66	1.77
20	1.00	1.17	1.32	1.54	1.75	2.19	2.34
25	1.23	1.43	1.61	1.88	2.16	2.69	2.86
30	1.47	1.72	1.93	2.25	2.58	3.22	3.44
35	1.69	1.97	2.22	2.59	2.96	3.70	3.94

TABLE II.—(*Continued*).

VELOCITY IN FEET PER SECOND.	THICKNESS IN INCHES = t.						
	$\frac{1}{7}''$ .143	$\frac{1}{6}''$ .167	$\frac{3}{16}''$ .187	$\frac{7}{32}''$ .219	$\frac{1}{4}''$ .250	$\frac{5}{16}''$ .312	$\frac{1}{3}''$ .333
40	1.90	2.22	2.49	2.90	3.32	4.15	4.44
45	2.09	2.45	2.75	3.21	3.67	4.58	4.89
50	2.27	2 65	2.98	3.48	3.98	4.97	5.30
55	2.44	2.84	3.19	3.72	4.26	5.32	5.69
60	2.58	3.01	3.38	3.95	4.51	5.64	6.02
65	2.71	3.16	3.55	4.14	4.74	5 92	6.32
70	2.81	3.27	3.68	4.29	4.91	6.14	6.54
75	2.89	3.37	3.79	4.42	5.05	6 31	6.73
80	2.94	3.43	3.86	4.50	5.15	6.44	6.86
85	2.97	3.47	3.90	4.55	5.20	6.50	6.93
90	2.97	3.47	3.90	4.55	5.20	6.50	6.93

V becomes a maximum at 87.41 feet per second, = 5245 feet per minute.

TABLE III.

HORSE POWER OF A RIVETED LEATHER BELT ONE INCH WIDE.

Formula as in Table II., except that $T = 400$ instead of 275.

VELOCITY IN FEET PER SECOND.	THICKNESS IN INCHES = t.						
	$\frac{7}{32}''$ .219	$\frac{1}{4}''$ .250	$\frac{5}{16}''$ .312	$\frac{1}{3}''$ .333	$\frac{3}{8}''$ .375	$\frac{7}{16}''$ .437	$\frac{1}{2}''$ .500
15	1.69	1.94	2.42	2.58	2.91	3.39	3.87
20	2.24	2.57	3.21	3.42	3.85	4.49	5.13
25	2.79	3.19	3.98	4.25	4.78	5.57	6.37
30	3.31	3.79	4.74	5.05	5.67	6.62	7.58
35	3.82	4.37	5.46	5.83	6.56	7.65	8.75
40	4.33	4.95	6.19	6.60	7.42	8.66	9 90
45	4.85	5.49	6.86	7.32	8.43	9.70	10.98
50	5.26	6.01	7.51	8.02	9.02	10.52	12.03
55	5.68	6.50	8.12	8.66	9.74	11.36	13.00
60	6.09	6.96	8.70	9.28	10.43	12.17	13.91
65	6.45	7.37	9.22	9.83	11.06	12.90	14.75
70	6.78	7.75	9.69	10.33	11.62	13.56	15.50
75	7.09	8.11	10.13	10.84	12.16	14.18	16.21
80	7.36	8.41	10.51	11.21	12.61	14.71	16.81
85	7.58	8.66	10.82	11.55	13.00	15.16	17.32
90	7.74	8.85	11.06	11.80	13.27	15.48	17.69
95	7.90	9.03	11.28	12.04	13.54	15.80	18.06
100	7.96	9.10	11.37	12.13	13.65	15.92	18.20
105	8.00	9.13	11.41	12.17	13.69	15.97	18.26

V becomes a maximum at 105.4 ft. per second = 6324 ft. per minute.

In Tables II. and III., the angle of Subtension, @, is taken at 180°.

Should it be...............	90°	100°	110°	120°	130°	140°	150°	160°	170°	180°	200°
Multiply above values by..	.65	.70	.75	.79	.83	.87	.91	.94	.97	1.	1.05

DISCUSSION.

Mr. John T. Hawkins.—In the very valuable and interesting experiments described in the above paper there seems to have been no importance attached to the question of centrifugal action of high-speed belts, while it is very certain that, in the operation of all such belts as necessarily possess considerable weight and which run at high velocities over pulleys of comparatively small diameter, this becomes a very important factor in their efficiency; quite sufficiently so to nullify the value as generalizations of "Indications and Conclusions," Nos. 12 and 13 of the paper in question.

In many varieties of machinery, wood-working machinery particularly, a large amount of power is required to be transmitted through belts which, in order to possess the requisite tensile strength, must be of sufficient weight to make centrifugal action in passing around the smaller pulley very much greater, I think, than is generally recognized; as, for instance, wood planers and surfacers, special turning lathes, etc.; and it is to be regretted, I think, that the experiments were not extended to conditions covering the action of belting of this kind. From page 554 of Mr. Lewis' paper I quote: "These experiments seem to show that the principal resistance to straight belts was journal friction, except at very high speeds, when the resistance of the air began to be felt," from which it would appear to be rather curious that so small a factor as the air resistance should be recognized, while the inevitable release of belt pressure upon the pulley and diminution of arc of contact through centrifugal action should have been lost sight of; the latter, even on a 20-inch pulley, which seems to have been the smallest pulley used at high speed, being of a certainty a much more serious factor than the air resistance could possibly be; and this leads me to think that perhaps the "peculiar and important feature" of Tables III. and IV., as quoted from p. 558, may not be as supposed, due entirely to "the effect of time upon the percentage of slip," but was more likely due to centrifugal action not having had time to assert itself when "the percentage of slip was measured at once after the load was applied," but did so after a sufficiently longer period had elapsed to permit of the belt obtaining its full speed and normal running conditions.

From the tables it appears that the highest belt-speed used was with 20-inch pulleys making 950 revolutions per minute, equal to

nearly 5,000 feet per minute of belt, without slip, as given in experiment 441, Table VII. In this case, if the driven pulley had been, say, one-quarter this diameter, the effect of centrifugal action in reducing the efficiency would have been so apparent, I think, as to show the necessity of taking this factor into account in any such general conclusions as Nos. 12 and 13, page 568.

I may also say that conclusion No. 4, same page, will not hold good for a vertical high-speed belt running over a lower driven pulley of small diameter, when the shafts are properly parallel. I have repeatedly seen such a belt run toward the center of the pulley when purposely led to one side or the other, while the driven pulley was forcibly held almost at a stand-still, the slip being fully 90 per cent. on this pulley; it appearing that the belt, reasonably enough, persisted in moving in the direction imparted to it upon leaving the overhead driver. I might say here that in these crude experiments it was found perfectly feasible, after allowing such a pulley and belt to get up to its speed, to put a brake upon the fly-wheel attached to this pulley and gradually retard the revolutions of the spindle, while the belt itself kept up to its maximum speed, until the spindle was brought entirely to rest, the belt continuing to pass around the pulley, making the slip 100 per cent. This I have repeatedly done in making these experiments, and with a belt whose tension when at rest would be considered, for any moderate speeds, as very great.

I regret to say that I have never instituted any quantitative experiments in connection with such high-speed belting, or, in fact, any but of the crudest description. Even these, however, I think establish beyond peradventure that the operation of centrifugal force in modifying the efficiency of high-speed belting is a factor which is in all cases considerable, and in extreme cases enormous; in fact, it can be easily shown, I think, that it may, in a very extreme case, deprive a belt almost entirely of its power to move the driven pulley, while its tension was such that at moderate speeds it would transmit a fair amount of power.

Five thousand feet per minute is a quite ordinary belt speed in wood-working machinery where the belt is anywhere from 2 to 5 inches wide, and running sometimes over driven pulleys as small as 3 inches in diameter; and in other classes of machinery, notably a stereotyper's routing machine, we have spindles running as high as 20,000 turns per minute with belt over a pulley one inch in diameter, corresponding to a belt speed without slip of over 5,000 feet

per minute. In this latter case it is found that leather belts of sufficient strength cannot be used, for the reason that their weight, to possess sufficient strength to resist the tension which must be imparted to them while at rest, is such that the centrifugal action throws them so nearly completely from contact with the small driven pulley as to deprive them almost entirely of their driving power. In all such cases a very strong but light, flat linen web, or tape, is found to be the best practicable belt.

This centrifugal action is, however, best illustrated, so far as done in any experiments I have made, in a wood-turning lathe, of which I had some ten or twelve running for several years, giving good opportunity for observation and such crude experiment as is described herein.

These lathes were used in turning thread spools. The resistance offered to the rotation of the spindle was rapidly intermittent but very great while the knives were cutting. In the production of these spools, the ends were both squared up by flat knives, or cutters, equal in width to a radius of the spool-head less one-half the diameter of the hole in the spool, on the end of the grain, at one operation; and immediately following, the depression was cut out of the cylindrical blank, at one operation, by three flat knives set to produce the required shaped depression; this whole operation being done so quickly that one lathe performed it upon as many as 120 gross in 10 hours, equal to an average for the whole day of 24 per minute; and, as the time required to sharpen and set the cutters and do other work in connection was generally more than one-half, this operation was actually performed as rapidly as 50 to 60 per minute in regular practice; and I have seen as many as 100 spools and over turned out per minute for a short time. Probably no belt could be devised which would continuously drive one of these spindles under the resistance offered in squaring the ends, or even in cutting out the depression; and the intermittent character of the resistance alone rendered it practicable to perform the work at the rate mentioned, the spindle being provided with a fly-wheel in which the power became stored during the time elapsing between cuts. These machines had steel spindles with journals one inch in diameter running in bronze boxes. The spindle carried a pulley 4 inches in diameter, and 3½ inch face, on one side of which the fly-wheel was formed, being a circular ring of about 1¼ inch section, and about 10 inches diameter. The driving pulley overhead was 30 inches diameter. The revolutions of spindle

were calculated for 5,000 turns per minute, corresponding to over 5,000 feet per minute belt-speed, without slip. The belt used was 3 inches wide, and generally the lightest leather procurable for that width was found to give the best results. The standing tension of these belts required to be such as would be generally considered excessive for low speeds, in order that the belt should not be thrown entirely away from the pulley surface or so nearly so as to practically deprive it of its power while running; and the inevitable fact followed that in this operation, with a tight and loose pulley belt from the line to the counter-shaft sufficiently slack to permit of considerable slip, and thus result in a diminution of speed in the vertical belt when the resistance of the cutting was offered to the spindle, the vertical belt would continue to perform the work of cutting at much reduced speed of spindle and overcome a much greater resistance of the knives per revolution, from the evident fact, as easily observed, that at high speed the belt would touch at but a very small part of the circumference of the pulley, while as soon as materially reduced by the slipping of the horizontal belt overhead it could be plainly seen to increase the arc of contact. Nothing could be more palpably apparent than that, with an exceedingly tight belt enwrapping under great pressure while at rest all that arc of the small pulley due to the belt running over a 30-inch pulley overhead, while running up to speed the belt would not touch more than one-third that amount; and this was invariably the case under the best working conditions.

Probably the most curious feature in corroboration of this centrifugal action is that, under the standing tension indispensable to these belts, when run at about half-speed the spindle journals and boxes would have great tendency to heat, while at the maximum speed they would run cool, showing clearly enough that the centrifugal action of the belt greatly reduced the pressure upon the journals as well as reduced the arc of contact.

For a considerable time I was troubled in getting new spindles or boxes to run cool, running them slowly, as would naturally suggest itself, in order to cool them or keep them from heating until worn down to a perfect bearing. It was finally discovered, however, that the best way to insure their running cool was to put them up to, and keep them at, top-speed; and any new spindle which would not run cool at top-speed would invariably heat up more when slowed down.

It would appear probable from the above that, first, so far as high-speed belts are concerned, the economy of belt transmission does not always "depend principally upon journal friction and slip;" but, in some cases at least, more largely upon this centrifugal action than on either; and, second, that in many cases it is not "important to make the belt-speed as high as possible within the limits of 5,000 or 6,000 feet per minute," and that the two generalizations 12 and 13 of the paper should be modified to embrace this disturbing element of centrifugal action as governed by diameter of pulley and weight of belt.

I am of the opinion that further experiments, embracing the features above described, are desirable, and that such would be very valuable in many branches of the arts where difficulty is now encountered in obtaining a desirable efficiency in such belts.

Mr. Oberlin Smith.—The point that struck me as particularly pertinent in the paper was the seventeenth remark, "that there is still need of more light on the subject." It seems to me that we are still a long way in the dark, and a great many more experiments are necessary. But the same feature which Mr. Hawkins has pointed out struck me forcibly in regard to the experiments here mentioned, that no attention, or scarcely any, was paid to the centrifugal force, which must be very great at high speeds. I have not, however, made any experiments myself in this particular line. The comparatively low speeds here given do not tend to develop this point.

Mr. H. R. Towne.—The Society is indebted to Mr. Lewis and to the firm of William Sellers & Co. for giving us the results of experiments which have cost so much time and money as these represent, and which add so much to the value of the Society's transactions. I think that all of us will agree that firms and individuals who are willing to contribute results of this kind are entitled to the warmest thanks of the Society.

There is one small factor which it seems to me may perhaps account for some of the losses in transmission by belting which has not been touched upon, and which I can conceive, under some conditions, may be a greater one than that of centrifugal force. It is the non-elastic quality of the material of the belting. If our belting was composed of a thin strip having perfect elasticity in bending over the pulley, while absorbing power on the entering side, it would give back that power on the other side, and so no power would be lost. But leather belting is not perfectly elastic, and

when new is very stiff and requires considerable power to bend it. Undoubtedly some power is thus absorbed, and this loss increases a good deal when the belts are new. When the belts are old it is probably an unimportant factor. I think any equation which undertook accurately to embrace all the conditions of the case would have to include this as one of the factors. The most interesting point which the experiments bring out, it seems to me, is that the determining element in the transmitting power of the belt is its *percentage of slip*. That is a point which has to a great extent escaped the observation of those who have treated this subject heretofore, most of whom have written about it more on the basis of theory than of actual observed experiments. And again: one of the other deductions, namely, that the sum of the tensions on the two sides of the belt, T_1 plus T_2, is not the same under all conditions. I think that in all theoretical discussions of this subject heretofore, it has been assumed that the sum of the tensions remains constant, whether the belt is running or standing still, or doing much work or little. Apparently the experiments presented by Mr. Lewis show conclusively that this is not true. The element of slip, which has been overlooked, interferes to prevent the constancy of that rule.

Mr. F. H. Underwood.—I would ask Mr. Lewis if he made his experiments with the grain side or the flesh side of the leather next the pulley?

Mr. John. Walker.—I would like to ask Mr. Lewis about this rawhide belt referred to in the paper. Was it the common untanned rawhide, or was it semi-tanned?

Mr. F. W. Taylor.—This rawhide belt was used at the Midvale Steel Works, with which I am connected, but I am unable to say exactly what process was used in preparing the leather for the belt. The leather used in the body of the belt was not fulled, being of about the thickness of rawhide, and very dense. I understood from the maker that in preparing the leather, it was bent backward and forward a great number of times, by being passed around a series of drums, which accounts for its being exceedingly soft and pliable. I further found, after having the belts in use for about two years, a fact which they did not call my attention to at first, which was, that there was a very thin layer of soft leather glued to the outside of the belt, which gave it a remarkably tenacious surface, a very much more sticky and adhesive surface than I have ever seen in any other belt. The surface felt to the touch almost like that of kid.

I think it would scarcely be a fair sample of rawhide belting, although it more nearly resembles rawhide than oak-tanned belting. It is but fair to state that in some cases, after two years of use, this thin facing of leather came off from the body of the belt in flakes.

Mr. W. H. Doane.—This is a question of deep interest to every member of this Association. I am very much pleased with the report of Mr. Lewis. It seems to me to enter fully into this matter, and yet it falls short in some respects. Now the branch of industry in which I am in:erested uses a high speed and quick transmission of power for the performance of the work of wood reduction by its wood-cutting tools. Those familiar with this branch of manufactures are also aware of the importance of obtaining and maintaining high velocity. It is impossible without this to do the work which machines are designed to accomplish, and the conditions of belts under extremely high velocity are quite different from what they are under slow velocity. For instance, the slippage of a belt in running iron-working machinery at a slow velocity, is very much less in my judgment than it would be where it is necessary to maintain high velocity like wood-working machinery. In addition to the reasons which have been named, there are several others, and I have found in practical experience the estimate of the amount of power required to be transmitted through a belt, to perform a certain amount of work depends largely upon its slippage, or rather the amount of the belt slippage depends very much upon the amount of power being transmitted. Take, for example, rotary cutters for working wood. Some of these run at great speed, often as high as 6,0.0, 8,000, and even 10,000 revolutions per minute, necessitating the belt that drives them to travel at a speed of nearly two miles a minute, and the conditions will be found to vary continually. Now if we consider the two miles of travel per minute of the belt, the condition of the cutters for displacing the wood, the disproportion of the size of the driver, and the driven pulley, the amount of fiber to be reduced from the wood itself, it will be apparent that this will create a resistance which the power of the belt must overcome; and if there is not power enough in the belt and behind the belt, and it is not just of the right kind, both in size and thickness, and its pliability is not such as will adjust itself to the contour of the pulleys, slipping must ensue and continue. Therefore, this is why I would like to see these experiments carried further. I believe there is no question likely to come before this body which has a greater interest to the

manufacturers of the country at large, than the question of transmission of power, and slipping of belts. There is no standard of belt transmission of power to-day, that I am aware of. Ask any practical operator or engineer the question, how much horse power a belt of a given size will transmit, and he will give you only an approximation, the conditions, as I have said, not being alike in any two cases. Now if we could reach hold of this matter, and by a series of tests establish some formula which was accurate and reliable, applicable to the general condition of shops, I would be very glad. Then would follow a more perfect transmission of power and higher quality and standard of belting, and a blessing and benefit would be conferred on the manufacturing interests at large.

Mr. H. R. Towne.—As Mr. Doane has very well said, this subject is of importance to every one who uses power, and it is one of the most encouraging signs of the good work that the Society is doing that two such representative institutions as the firm of William Sellers & Co. and the Massachusetts Institute of Technology, under Professor Lanza's direction, are both at work in this field. At our last meeting in Boston, you will remember that Professor Lanza gave us a very valuable paper upon this subject,* reporting the results of experiments which he was conducting in the Institute on this very question—the transmission of power by belting. Those experiments he is still continuing, and he has promised us a further report, which I hope we may have at our next meeting.

Just twenty years ago I became interested in this subject and made a series of tests and experiments on the transmission of power by belting—very crude as they seem now in the light of what is being done to-day, but still of some use; and one of the factors which came in very clearly at that time, and of course does always, where belting is used, was the hygrometric condition of the atmosphere, and I think that in any accurate tests of belting this must be taken into account. I found that the belt which I was testing to day, and which gave certain results, and pretty uniform results, would to-morrow give very different results indeed; and noting the condition of the atmosphere, I found that those changes were coincident with changes in the amount of moisture in the air. Undoubtedly belting is very much influenced by this, so that the perfect equation must bring in the hygrometric condition of the atmosphere, although, of course, this point is rather theoretical than practical.

* No. CCII. Transactions A. S. M. E., p. 347, Vol. VII.

Mr. Thos. R. Morgan.—It seems to me that another factor which must be considered in our discussions on belting is the length of the belt. This is very often governed by the conditions of the workshop and the positions of the shafting. As a rule, belting will transmit more power when it is long, but I do not know that we have any tables or formulæ to tell us how much. I think an important point would be gained if experiments on this subject could be made by somebody who might have the time and opportunity. Where there are variations from tabulated results, I have often found them to follow variations in the lengths of the belt.

Mr. Wm. Kent.—It seems to me that the discussion has led more to the subject of what we don't know about belting, than what we do know about belting, and it shows that the field for experiment on this subject is an enormous one. It is far larger than the firm of William Sellers & Co., or any Institute of Technology can expect to cover in the next ten or twenty years. I think the firm of William Sellers & Co. are entitled to much credit for all that they have done, and I hope they will go on in the same line. The same difficulty stares us in the face in every problem of mechanical engineering that is presented to us—What size of shafting shall we use? What size of pipe shall we use? and so on. One branch of the subject was taken up by a United States Government test commission about ten years ago, and its work was left sadly incomplete on account of the refusal in Congress to continue the appropriation. I merely make these remarks to show the necessity of a national or private endowment of a fund of a few hundred thousand dollars to carrying on such experiments as they should be conducted. Professor Lanza is doing all he can with his limited facilities at the Massachusetts Institute of Technology. His facilities ought to be increased tenfold. To-day, when we talk about steam, we have to go back to the experiments of Regnault, made in France at the expense of the French Government. If we consider the strength of materials, we have to go to the experiments made at the expense of the German Government. But there is no one in the country to-day prepared to do such work unless they have a national endowment or a very large private endowment.

Mr. Lewis.—I may say in regard to the stiffness of the belt and the power lost in bending and unbending, that we were unable to detect any such loss. We ran very thin belts and very thick belts under the same conditions of speed and tension, and found no difference in the amount of power consumed. In quarter-twist belts,

where the bending is edgewise and there is more slip in leaving the pulley, there was considerable loss from that cause, but in ordinary straight belts, I think the loss is practically inappreciable.

In reply to Mr. Underwood's question as to which side of the belt was next the pulley in the experiments, I would say that it was generally the grain side. In some of the old belts it was difficult to say whether it was the grain side or not.

In closing the debate,* I would say that the point raised by Mr. Hawkins in regard to conclusion No. 4, "That a belt will seldom remain upon a pulley when the slip exceeds 20 per cent.," is, I think, well taken.

This conclusion applies more particularly to horizontal belts on crowned pulleys, and undoubtedly there are cases, like those cited, in which the slip may be as great as 100 per cent. without causing the belt to leave the pulley.

It is the crowning of a pulley which causes the belt to remain upon it in a central position, when there is not much slip, and it is also the crowning which causes the belt to leave the pulley when, as we determined, the slip exceeds 20 per cent.

On flat-faced pulleys, properly set, there is no tendency for the belt to stay on or run off whether it slips or not, and very slight influences are sufficient to work it laterally.

When a small pulley is driven by a large one, so that the slip is confined principally to the former, I think it might be advisable to make the face of the small pulley straight and have crowning only on the large pulley which does not slip.

In regard to the effect of centrifugal action, I do not see how there can be any question about it, and I think a more careful study of my paper will show that so far from its having been neglected as a matter of no importance, it was really the principal subject of consideration in arriving at conclusion No. 13, which Mr. Hawkins refers to as one of those whose value as a generalization is nullified by neglect of this important factor.

The conclusion states that it is important on account of journal friction and slip to make the belt-speed as high as possible within the limits of 5,000 or 6,000 feet per minute, and by reference to formula (3), page 563, it will be found that these limitations were derived directly from the consideration of centrifugal force alone.

I did not deem it necessary to dwell at length upon this part of the subject, because it had been so thoroughly treated in Mr.

* Contributed after adjournment under the rules.

Nagle's paper on the "Horse Power of Leather Belts," that a brief statement of his results seemed to be sufficient. I hope, however, to be able to correct the impression that generalizations 12 and 13 require to be modified on account of centrifugal action, except in so far as it may affect the arc of contact, and with this provision that the arc of contact remains constant or nearly so, I adhere to the original text and repeat the importance of making "the belt-speed as high as possible within the limits of 5,000 and 6,000 feet per minute," as the only part affected by that consideration. The statement following the formulæ on page 562, that "the velocity at which the maximum amount of power can be transmitted by any given belt is independent of the arc of contact and co-efficient of friction, and depends only upon the working strength of the material and its specific gravity," applies, as intended, to any constant values that may be assumed for arc of contact, co-efficient of friction, strength, and specific gravity, and if any of these values were supposed to vary with the speed, it would, of course, be necessary to establish the law of variation in the general formula for horse power in order to determine the velocity of maximum effect.

To do this would be extremely difficult, if not impossible, for even a special case, while for general practice it is entirely out of the question, and it is still thought that the velocities stated can be shown to give the best results for the largest range of practical conditions.

Mr. Hawkins quotes from page 554: "These experiments seem to show that the principal resistance to straight belts was journal friction, except at very high speeds, when the resistance of the air began to be felt," and observes it "to be rather curious that so small a factor as air resistance should be recognized, while the inevitable release of belt-pressure upon the pulley and diminution of arc of contact through centrifugal action should have been lost sight of." It would be curious enough if it were a fact, and granting it to be true, it would be still more interesting to know how "centrifugal action" and "arc of contact" can be considered at all in the light of resistances opposing the travel of a belt.

It is distinctly stated on page 562 that "the sum of the tensions" was the horizontal pressure of the belt against the pulleys, and that no allowance was necessary for centrifugal force.

The results obtained by our apparatus required no correction, and the loss in pressure against the pulleys as the speed increased took care of itself.

For the highest speed used, about 4,800 feet per minute, the same paragraph relates that "the tension indicated in the belt at rest was about 50 pounds greater than when in motion," and it would have been quite possible to have kept a record throughout our experiments of the loss in tension from centrifugal action, had there been any apparent need of it.

When the speed and weight per cubic inch or specific gravity of a belt are known, its centrifugal tension per square inch becomes a matter of calculation, and while it might have been interesting to have compared this with observations upon the release of pressure against the pulleys, it does not appear that any important results could have been gained thereby. Still it may be regretted, for the sake of argument and illustration, that our experiments were not more complete in this respect, for although "centrifugal tension" and "release of pressure" are generally considered as synonymous terms, it could then be shown that they are not necessarily one and the same thing, and that while the former may be calculated with precision, the latter is variable and depends largely upon such circumstances as the relative position of the pulleys and the tension of the belt.

When two fixed pulleys of equal size are connected by a vertical belt, *centrifugal tension* must correspond with *release of pressure*, but in other cases the release of pressure is never perfect on account of the sagging of the belt. We are enabled by means of our experiments to determine in every case the effective tensions upon opposite sides of a belt while doing work, and, in order to know the actual stress upon the belting at the given speeds, we should have to add the calculated centrifugal tension and not the release of pressure which might have been observed. For example, in experiments 448 to 450 inclusive, where the release of pressure when running without slip was observed to be 50 lbs. total or 25 lbs. on each side, we should have to add to T and t about 30 lbs. for centrifugal tension, instead of 25 lbs., the release of pressure, and it may be observed as a general principle that the greater the horizontal distance between pulleys and the smaller the initial tension, the greater will be the difference between centrifugal tension and release of pressure against the pulleys.

If the driven pulley in experiment 441 had been one-quarter of the actual diameter used, as Mr. Hawkins suggests, I can see no reason why the effect of centrifugal action would have become more apparent, and I doubt very much whether the diminution in the

arc of contact would have been any greater than should have resulted from the release of pressure. In fact, it is conceivable, that with horizontal belts driving underneath, "the inevitable diminution in the arc of contact through centrifugal action" might disappear altogether and become a negative quantity to the still greater advantage of high speeds.

In the case described by Mr. Hawkins, the arc of contact was undoubtedly a very important variable, much more so than in any of our experiments, and I think the difference is entirely due to the circumstance of position which was not contemplated in our generalizations.

It is not difficult to understand how, with a vertical belt running from a 30″ pulley overhead to a 4″ pulley underneath, the arc of contact might be diminished with the speed to one-third of its original amount, or that there might even be no contact at all, for the release of pressure would be almost equal to the centrifugal tension, and the curve of the running belt would naturally approach the arc of a catenary suspended from the circumference of the overhead pulley.

With horizontal belts, from which our conclusions were chiefly derived, I cannot imagine centrifugal action to have any greater effect upon the arc of contact than that which usually results from changes in load at any given speed, and as this is seldom in excess of a few degrees, more or less, according to the position of the driving side, it does not seem to be a vital consideration, and in forming our conclusions we were certainly nearer the truth for all such cases in taking it as constant than we would have been by supposing it either greater or less.

If we let

V = velocity of belt in feet per second,

y = its specific gravity,

T = centrifugal tension in lbs. per sq. in., it can readily be shown that

$$T = .0135\ V^2 y \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and, substituting the values of y, we shall have for new or dry belting,

$$T = .012\ V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

for old or saturated belting,

$$T = .014\ V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or for general practice, say,

$$T = .013\ V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

And it should be observed that all of these expressions are entirely independent of pulley diameter or size of belt.

A heavy belt will, of course, exert more centrifugal force at a given speed than a light one, but the centrifugal tension per square inch in the two belts will be the same, and it is evidently the tension per square inch which we have to consider in determining the maximum power of belting whether it be heavy or light.

The working tension per square inch which may be allowed for leather belts is variously estimated, but we may assume, on Mr. Nagle's authority, 275 lbs. for laced belts and 400 lbs. for cemented belts without lacings.

Through the action of centrifugal force, this working tension becomes divided into two parts, one of which may be called the centrifugal tension and the remainder the driving or effective tension.

For any given arc of contact and co-efficient of friction, the effective tension times the speed is proportional to the power transmitted.

As the speed increases this tension must diminish, and so it happens that a limiting speed is reached on either side of which the power transmitted becomes less.

What this limiting speed must be can be found from the general equation, page 562,

$$HP = C\,V t w\,(S - .012\ V^2) \div 550 \quad . \quad . \quad . \quad . \quad (5)$$

by differentiating with reference to V and equating to zero, whence we obtain,

$$V^2 = \frac{S}{.036} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or by substituting .013 for the co-efficient of V^2 in equation (5) we have

$$V^2 = \frac{S}{.039} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When $S = 275$, $V = 84$, and when $S = 400$, $V = 101$ and multiplying by 60 for the speeds per minute, we have 5040 and 6060 in support of the limitations expressed in conclusion 13.

Having demonstrated the fact that centrifugal action *was* considered in arriving at this conclusion, I wish to show that the economy of belt transmission depends principally upon journal friction and slip. In the first place, it should be understood that "economy of transmission" has reference only to the energy received and delivered by the transmitting medium.

It is not an absolute quantity, but the relation between two quantities, that we have to consider. A belt receives energy from one pulley and imparts a portion of it to another, which in turn transmits a portion of what it receives to a shaft.

Energy is consumed in friction by the journals of the driving and driven pulleys, by the slip of the belt on the pulleys, by the air, and by the bending of the belt, and the remainder, if any, is to be considered as useful effect.

The useful effect divided by the total energy consumed in the transfer is the efficiency of the medium.

We conclude from our observations that the bending of the belt and the resistance of the air consumed so little energy under ordinary conditions that they might be neglected, but we always found that journal friction and slip were prominent sources of loss, and we simply made a statement to that effect in our 12th conclusion.

With crossed belts there might be another serious loss from friction at the point of crossing, but it is not an unavoidable one like the others, and apart from this I can imagine no other channels for the escape of energy under proper conditions than those just enumerated.

Having reached this conclusion, we proceeded to observe the importance of making the belt-speed as high as possible within the limits of 5,000 or 6,000 ft. per minute.

It was thought to be important because experiments upon journal friction show very generally that an increase of speed reduces the resistance, and also because the necessary slip is rendered thereby a smaller percentage of the belt travel.

The limitations of 5,000 or 6,000 ft. per minute were derived, as already shown, from the consideration of centrifugal force, and the possibility of any misconception on this point did not occur until brought out by the discussion in question.

There seems, therefore, to be an unfortunate omission in the text, which can easily be supplied without affecting the meaning intended, and although I cannot agree entirely with the views and opinions expressed by Mr. Hawkins, I am nevertheless indebted to

his thoughtful discussion for the corrections and amendments which it seems necessary to make in our "indications and conclusions." One other point I desire to notice, and, from what has already been said, it may readily be inferred that "the peculiar and important feature" of Tables III. and IV. was not due in any way to "centrifugal action," and the opinion expressed in regard to it arises, I think, from some misapprehension of the experimental conditions.

The centrifugal tension at a speed of 800 ft. per minute would amount to very little, and at any speed, however great, it would necessarily be co-existent with the speed, and should therefore require no "time to assert itself."

From the foregoing considerations, and after a careful review of the criticisms expressed, I conclude:

4. That a *horizontal or inclined* belt will seldom remain upon a *crowned* pulley when the slip exceeds 20 per cent.

12. "That the economy of belt transmission depends principally upon journal friction and slip."

13. That it is important on this account to make the belt-speed as high as possible within the limits of *economy in belting due to centrifugal action, which may be put at* 5,000 or 6,000 ft. per minute *for belts whose direction is chiefly horizontal.*

I think it must be admitted that centrifugal action properly expressed is not governed at all by pulley diameter, and that whenever the arc of contact can be shown to vary from the natural result of the effective tensions T and t, the variation must be ascribed to some other cause.

It may be due to irregularities or stiffness in the belt, the resistance of the air, or something else, but it certainly cannot, on mathematical principles, be ascribed to the effect of centrifugal force.

I should like to see the question of the arc of contact settled, if need be, by experiment, and I regret very much that I have nothing to offer on this point but theoretical deductions which I have ventured to submit for what they may prove to be worth.

In regard to the effect of hygrometric conditions, suggested by Mr. Towne, I think everything is important that may act to affect the condition of the frictional surfaces, and that temperature is also another consideration which seems especially to affect the dressings worked into the leather.

There is always a large margin for variation in the adhesion of

belts under the same hygrometric conditions, and the same may be said of temperature.

The great difficulty is to determine with certainty the causes of the effects observed. There are so many variables, slip, temperature, pressure, surface, arc of contact, pulley diameter, belt dressing, pliability, etc., besides hygrometric conditions, which operate together in producing a given result, that it seems to be practically impossible to study the effect of each independently.

CCXIV.

THE RELATIVE EFFICIENCY OF CENTRIFUGAL AND RECIPROCATING PUMPS.

BY WILLIAM OLIVER WEBBER, LAWRENCE, MASS.

UNTIL quite recently very little has been done to perfect centrifugal pumping machinery in the United States. There are no well-authenticated experiments recorded by American engineers containing all the necessary data for comparison with other types. The situation in Europe is better; in 1883, pumps constructed on the Gwynne system gave under a lift of 14.7 feet an efficiency of 61.3 per cent. of useful effect. The term efficiency here used indicates the value of

$$\frac{\text{W. H. P.}}{\text{I. H. P.}},$$

and does not therefore show the full efficiency of the pump, but that of the combined pump and engine. It is, however, a very simple way of showing the relative values of different kinds of pumping engines having their motive power forming a part of the plant.

Several diagrams have been prepared illustrating this subject, which are presented as representing the results of experiments made with the two types of engines which are being considered. It may be of interest to insert here the following description of the testing apparatus used by the writer in determining the efficiency of the belted centrifugal pumps which are illustrated in Figs. 159 and 160. Referring to cuts 157 and 158, *a* is a pump,* in position to be tested, and is bolted down to the floor over the tank *g*; *o* is the strainer on lower end of suction-pipe; *n* is the gate valve in the discharge-pipe, to be closed when using the injector *h* in exhausting the air from the pump so as to prime it; a taper expander, *i*, is used on the discharge-pipe tapering from 5″ to 6″,

* Known on the makers' lists as a 5-inch Class B pump.

this being found to act something like a taper draft tube on a water-wheel, increasing the efficiency of pumps so connected from 2 to 5 per cent.; the pipes *j* and *j'* are wrought-iron tubing, such as ordinarily used in piping, and the elbows *k* are the long turn

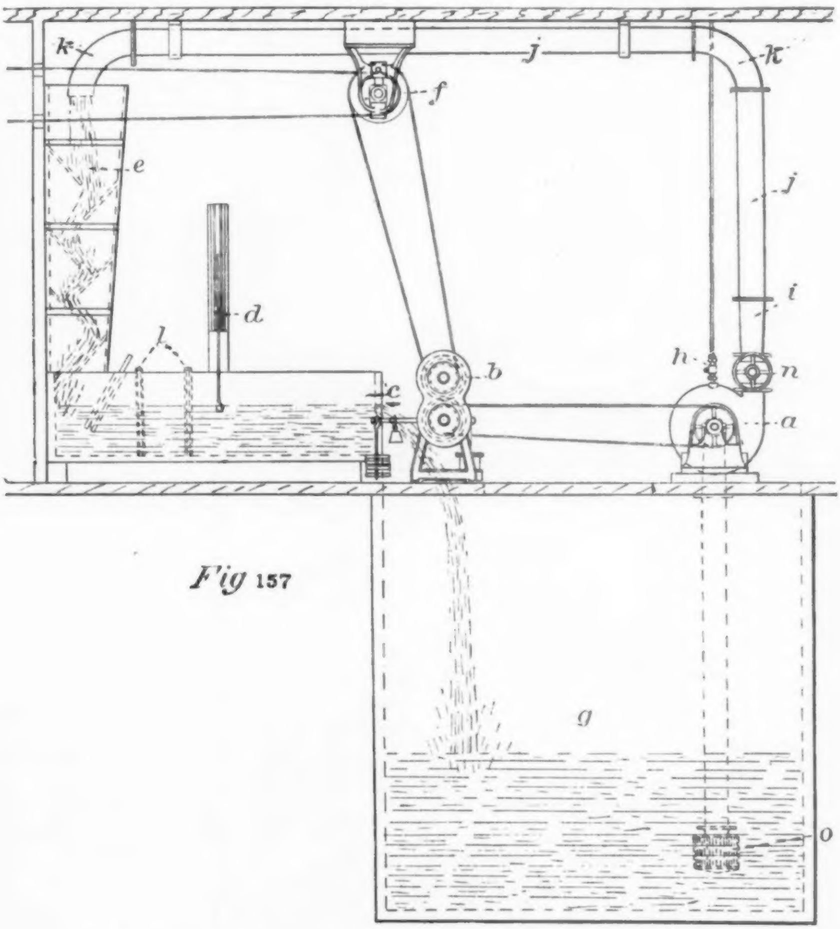


Fig 157

elbows especially designed for automatic sprinkler purposes, and are used here in preference to ordinary elbows, owing to the great loss of power in driving water around such short corners.

The power being used is transmitted by the counter-shaft *f* from the main line, to the balance transmitting dynamometer *b*, which carefully weighs the power being used by the pump *a*.

The water being pumped from the tank g passes up through the piping j and j', and is delivered into the diffusing-box c, where its

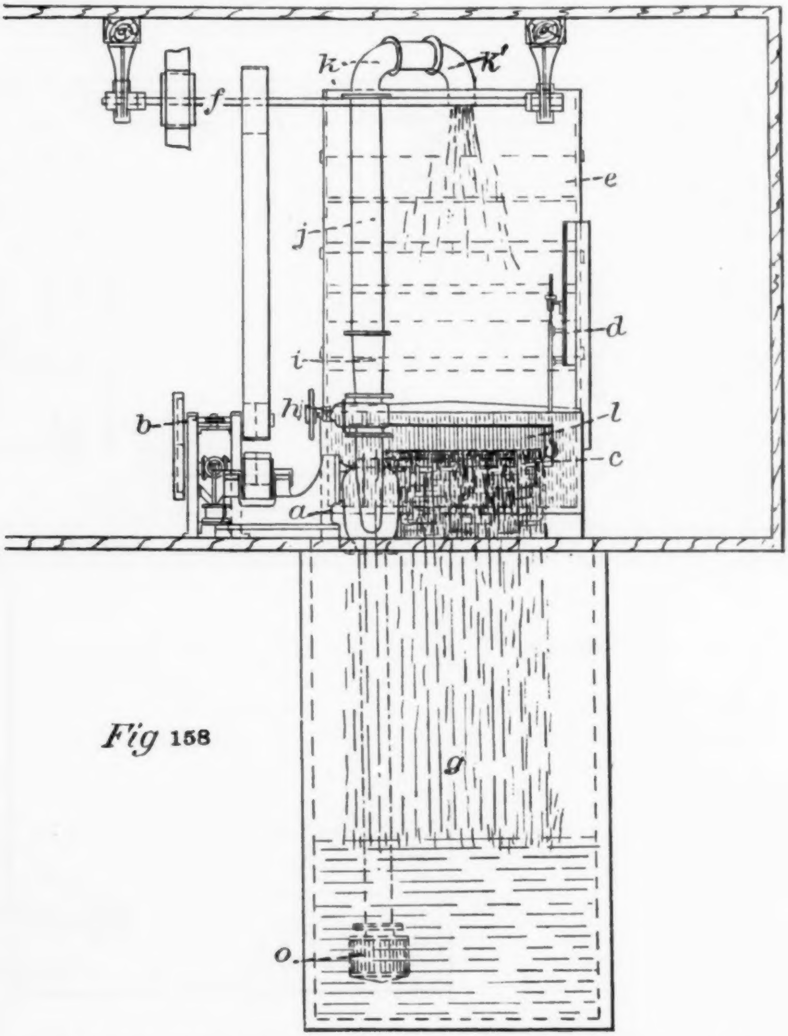


Fig 158

velocity is retarded, and the solid round stream is broken up and spread out into a thin sheet by the inclined shelves shown in dotted lines, and the water then delivered in a broad sheet, but with the velocity due only to its own weight in falling, into the weir-box c, at the front end of which is the weir proper, e, having an opening

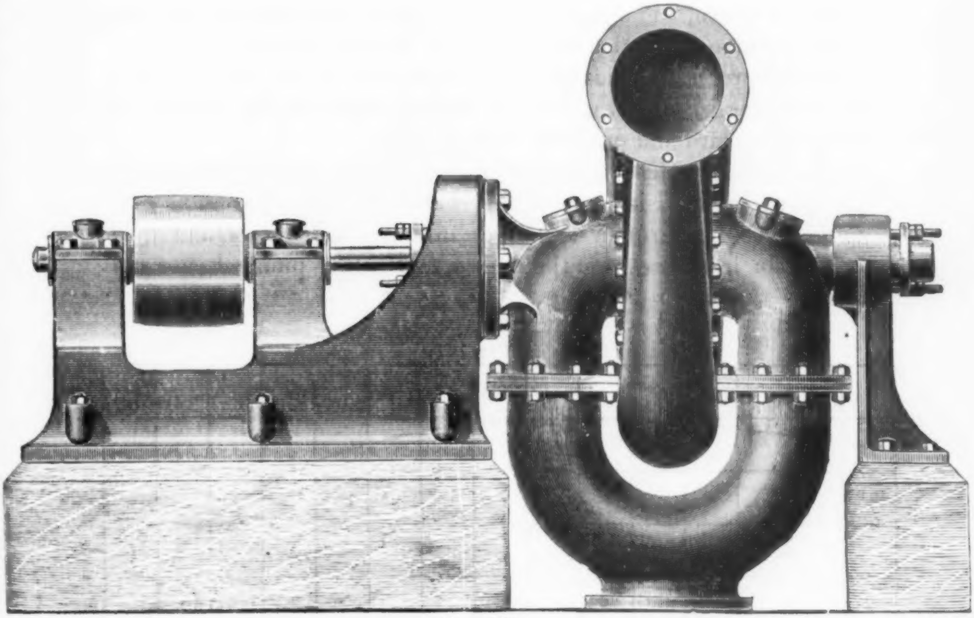


FIG. 159.

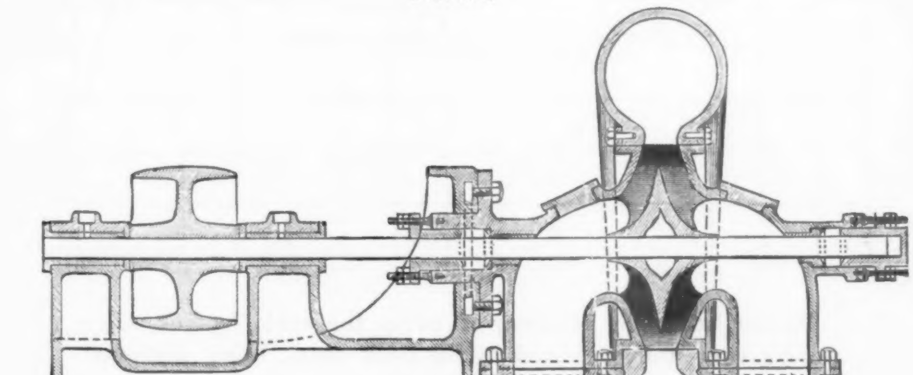


FIG. 160.

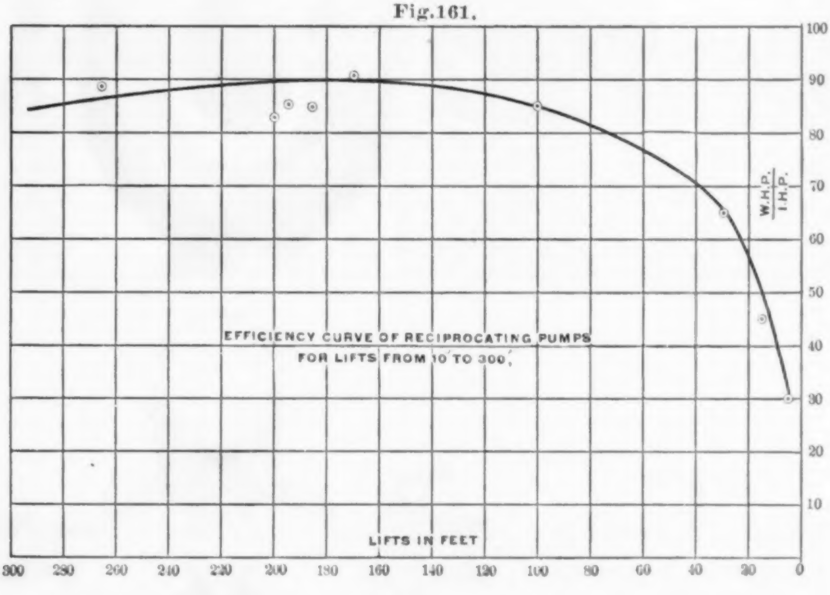
VERTICAL SECTION THROUGH BELTED PUMP.

of 48″ in width, and a depth of water below the crest of the weir equal to three times that of the stream flowing over it.

At the proper distance back from the crest is the hook gauge *d* for measuring the depth, and still farther back are the smoothing racks *l* to retard and quiet the flow of water.

In making a test, after exhausting the air and starting the pump, the valve *n* is opened and water is discharged in a continuous stream through the pipes into the weir-box *c*, and then falls again in a sheet into the tank *g*, and is thus pumped over and over.

Fig. 161.



In calculating the efficiency of pump, the cubic feet of water passing over weir, measured by the hook gauge, being converted into pounds by multiplying by 62.5, is again multiplied by the height from level of water in tank *g*, when the pump is running, to the center of the horizontal discharge-pipe *j* at top of test-room, and the foot pounds so obtained, divided by 33,000, equals the water horse-power being developed.

The power used to do this work is measured by the dynamometer, and (minus the friction of the dynamometer itself, which is in every test weighed and deducted) equals the dynamometer horse-power; the water horse-power being divided by the dyna-

mometer horse-power equals the efficiency of the pump being tested; or to formulate

$$\frac{\text{W. H. P.}}{\text{D. H. P.}} = E^1.*$$

Figure 161 is prepared by plotting the values of $\frac{\text{W. H. P.}}{\text{I. H. P.}}$ as found in the various tests made to determine the duty of some of the best designed reciprocating pumping engines, of bucket and plunger, piston type, etc.

The highest value of this term with which the writer is familiar is .9164 for a lift of 170 feet, and 3,615 gallons per minute. This was obtained in a test of the Leavitt Pumping Engine at Lawrence, Mass., July 24, 1879, made by Richard H. Buel, C. E., the following being the results obtained during the duty trial:

Duration of trial	15.1 hours.
Pounds of wood used to start fires	400.
Of coal put into furnaces	3,500.
Of coal withdrawn from furnaces at end of trial	27.
Of coal consumed .... $(400 \times .4) + (3{,}500 - 27) =$	3,633.
Pressure in main, by gauge	64 lbs. per sq. in.
Water level in well below gauge	29.05 feet.
Water pressure $29.05 \times 0.433) + 64 =$	76.6 lbs. per sq. in
Area of pump bucket	536.0465 sq. in.
Revolutions of engine	12,337.
Duty of engine $\left(\frac{536.0465 \times 8 \times 12{,}337 \times 76.6 \times 100}{3{,}633}\right) =$	111,548,925 ft.-lbs.
Average revolutions per minute	13.62.
Steam pressure by gauge	89.5 lbs. per sq. in.
Vacuum	27.4 inches.
Barometer, inches	29.81.
Temperature of engine-room	79°.
" of feed water	119°.
" of flue	358°.
Total quantities:—Pounds of coal	3,633.
" " " of ashes	223.
" " " of combustible	3,410.
" " " of feed water	36,800.
U. S. gallons of water pumped per 24 hours, calculated from pump capacity	4,401,272.
Per cent. of ashes	6.14.

* In order to avoid confounding the tests of two types of pumps—*i. e.*, those coupled direct to engine and those driven by belt—the expressions $\frac{\text{W. H. P.}}{\text{I. H. P.}} = E$ will be used for the former and $\frac{\text{W. H. P.}}{\text{D. H. P.}} = E^1$ for the latter.

Hourly quantities:—Pounds of coal				241.
"	"	"	of combustible	26.
"	"	"	of coal per sq. ft. of grate	8.38.
"	"	"	of combustible, per ditto	7.86.
"	"	"	of heating surface, "	0.236.
"	"	"	of combustible, per ditto	0.222.
"	"	"	of feed water	2.437.

Evaporation:—Pounds of water per pound of coal, at observed temperature and pressure ... 10.13.

Per pound of combustible, ditto ... 10.79.

Per sq. ft. of heating surface per hour, ditto ... 2.39.

Per pound of coal from and at 212° ... 11.49.

Per pound of combustible, per ditto ... 12.24.

Per sq. ft. of heating surface per hour, ditto ... 2.71.

Horse-power:—Net (calculated from water pressure) ... 125.55.

" Indicated $\left(\frac{135.55}{0.91643}\right)$ = ... 147.91.

Pounds of coal and water per hour, per horse-power:—

Coal, per net horse-power ... 1.78.

Per indicated horse-power ... 1.63.

Feed water, per net horse-power ... 17.98.

Per indicated horse-power ... 16.48.

For higher lifts than 170 feet the curve of efficiencies falls, and from 200 to 300 feet lift the average value seems to be about .84 per cent.

Below 170 feet the curve also falls reversely and slowly until at about 90 feet its descent becomes more rapid, and at 35 feet .727 appears the best recorded performance.

There are not any very satisfactory records below this lift, but some figures are given for the yearly coal consumption and total number of gallons pumped by engines in Holland under a 16-foot lift from which an efficiency of .44 has been deduced.

An interesting collection of information was published in 1883 by an Italian engineer, Signor Cuppari, concerning pumping engines in Holland, which will be found useful in this branch of hydraulics.

Fig. 162 contains two curves, one transferred from Fig. 161 on a different scale, the other the result of tests made with centrifugal pumps.

As in the first type considered, there is a lift at which we find centrifugal pumps giving a maximum efficiency, while for lifts above or below this, the efficiency decreases.

The lift at which the maximum efficiency is obtained in the latter case is approximately 17 feet.

At lifts from 12 to 18 feet some makers of large experience claim now to obtain from 65 to 70 per cent. of useful effect.

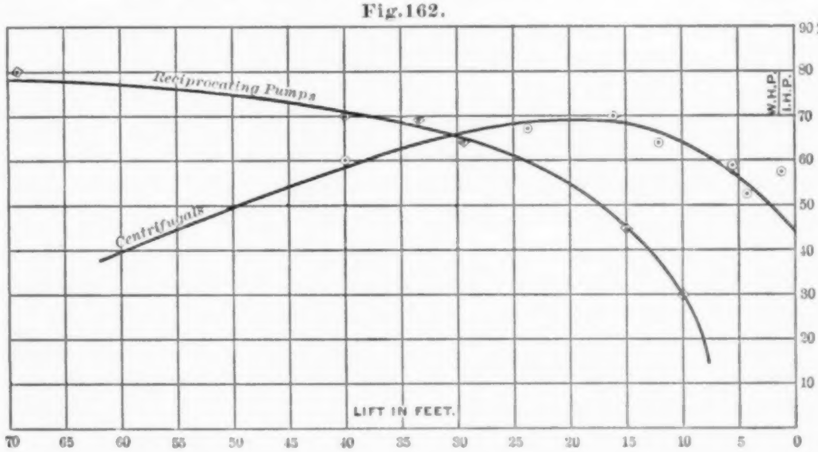
As already stated, .613 is at present the best done at a public test under 14.7 feet head.

Some very carefully conducted trials of 31″ centrifugals recently made at Fas Bouches du Rhone gave the following results.*

Lifts:	4.13 feet	4.51 feet	5.26 feet	5.46 feet
$\frac{\text{W. H. P.}}{\text{I. H. P.}}$	.535	.555	.567	.579

From the above we see that the efficiency rises with the head in

Fig. 162.



feet, as far as the experiments go. In all the curves thus far considered the amount of water pumped has been large, 18,000,000 to 100,000,000 gallons of 231 cu. in. in ten hours. Above a 20-foot lift for centrifugals, and below 35 feet for reciprocating pumps, experiments of value seem to be sadly deficient. It is however probable that the curves shown on Fig. 162 approximate the relation between the two types under consideration.

Let us see what these two lines say. The work done, in taking water from one level and placing it at a higher level, is divided into three important parts: (*a*) lifting a certain number of gallons or pounds a given number of feet high; (*b*) overcoming the friction of the water through the pipes and pump; (*c*) overcoming the friction of the moving parts of the pump and engine due to the load imposed.

The reason why we are able to obtain such high efficiencies with reciprocating pumps at high lifts, is because the first factor is so

* See *London Engineering*, Aug., 1885.

large a proportion of the work done, for if a certain number of gallons is to be raised, in a given time, to a given height, our only hope for high efficiencies lies in making the friction of the water through pipes and pump, as well as the friction of the pump itself, a minimum. Here is where a change in style of pump on low lifts is taking place. The water in passing through or passing over the disc of a centrifugal pump meets with almost no resistance in the shape of valves to be opened, corners to be turned sharply, and contracted passages, so often found in the common piston pump.

Again, the friction of the pump itself is confined entirely to what is generated by the revolving shaft in two or sometimes three bearings, and it is well known that a revolving shaft is the most easily lubricated of any form of bearings. Again, the friction of motion being less than that of rest a slight advantage seems to be gained in the continually revolving shafts over the stops and starts of a reciprocating pump. Besides this, there is also to be considered the subject of shocks and jars of large masses of water moving at variable velocities as well as the freedom from an air-cushion on the piston in a centrifugal pump. These are some of the more important reasons to account for what we find to exist, namely, that for lifts up to 34 feet, water can be handled more efficiently by centrifugal than by piston pumps.

There is this point which may be worth considering, in connection with the point at which the curves of efficiencies cross. It appears to correspond with the head of water which is supported by the atmospheric pressure, and it has been suggested that centrifugal pumps will give higher efficiencies than piston pumps just as long as they can depend upon the help of the atmosphere, but when this help is not available a direct push seems the proper thing.

Having considered thus far the comparative value of the two types from the efficiencies alone, we might now look at some other points of importance to the user of any apparatus for raising fluids; under this head we shall consider the following subjects: (1) comparative weights; (2) first cost; (3) annual costs; (4) ease in handling.

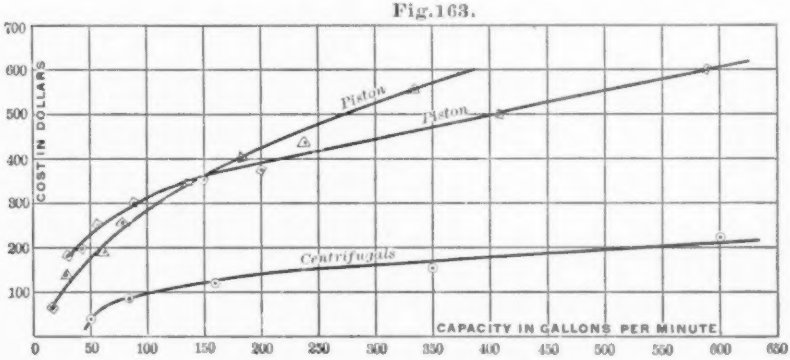
The drainage pumps constructed some years ago for the Haarlem Lake were designed to lift 70 tons per minute 15 feet, and they weighed about 150 tons. Centrifugal pumps for the same work weigh only 5 tons. The weight of a centrifugal pump and engine to lift 10,000 gallons per minute 35 feet high is 6 tons. This lift is the same that the Boston Sewage System station pump is at work

against; but the latter capacity is double the 10,000 gallons per minute, and the proportionate weight for a centrifugal pump would be 12 tons. We have not at hand the actual weight of the Boston pump, but it must be many times 12 tons.

Fig. 163 shows the relation between the two types as regards the prices, the upper two curves showing the prices of piston pumps to raise different amounts of water. The lower line gives the same for centrifugal pumps from which we deduce this approximation; first cost of centrifugal pump = .4 first cost of piston pumps for the same duty.

The annual costs include several items, the more important of which are: (1) interest on investment; (2) depreciation in value; (3) heat units used up in running pump per hour; (4) oil, care,

Fig. 163.



repairs, etc. For low lifts up to 40 feet, each above item is lower for the centrifugal pump.

The pumps placed by Gwynne at the Ferrara Marshes, Northern Italy, in 1865, are still in working order, and seem good for a long time to come. These pumps are, as far as the writer knows, capable of handling more water than any other set of pumping engines in existence. The work performed by these pumps is the lifting of 2,000 tons per minute, over 600,000,000 gallons per 24 hours, on a mean lift of about 10 feet (maximum of 12.5 feet). (See *Engineering*, 1876.)

The diagrams of Fig. 162 dispose of the third item, while the last items are surely not greater than for piston pumps.

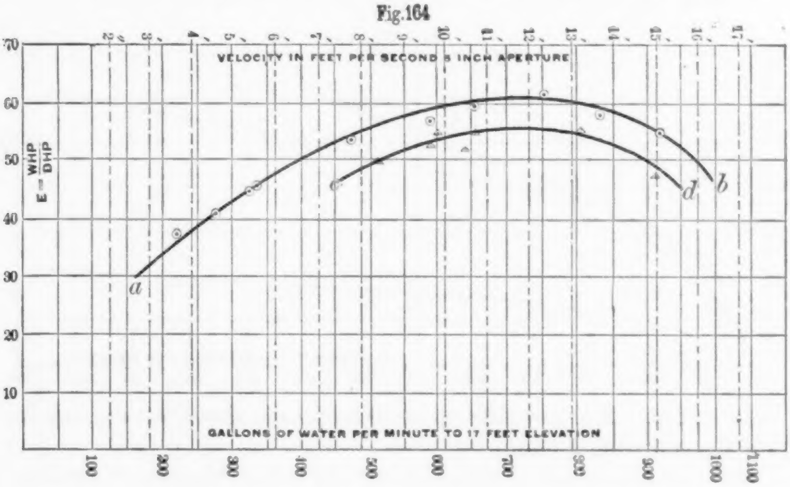
It is fair to say that Fig. 163, showing relative prices of the two types of pumps, includes only pumps of each type up to a capacity of 600 gallons per minute, and were plotted out direct from catalogues of well-known makers of each style.

The writer not having access to any reliable data of time performances of direct-action steam pumps generally in use for tank and similar work, can make no graphical comparison between these and centrifugal pumps.

Having made some 70 or 80 experiments on efficiencies of centrifugals, lifts from 5 to 50 feet, and capacities from 50 to 1,500 gallons per minute, measured by a dynamometer, it would be very useful to have some information concerning the former class for comparison in the smaller sizes, and we would be glad to see some published reliable results under this head.

Fig. 164 shows two efficiency curves for different velocities, plotted from tests made of two pumps with 5″ discharge apertures.*

Fig.164



These tests were made under an average elevation of 17 feet, the pumps in both cases draughting about 9 feet and discharging 8 feet higher. The upper curve *a b* was the result of tests made by a pump that was very clean and smooth inside. The lower curve *c d* was made by a pump in which, through carelessness in the foundry, the core-sand had been allowed to burn into the inside face of volute or casing, and water passages. The difference between these two curves (which, by the way, are remarkably uniform), shows the absolute necessity of having the inside of all such pump castings very smooth and free from the slightest roughness. Both of these pumps were taken at random from stock, and were in

* Known on makers' lists as a No. 5 Class B pump.

nowise especially prepared for these tests. These tests seem to show that the efficiency rises very gradually and uniformly until the water reaches a velocity equal to 11½ feet per second. The highest efficiency with this size pump seeming to equal a velocity of 12 feet per second, after passing which point the efficiency falls very rapidly.

The complete log of the tests is given in the accompanying table, and Fig. 165 repeats Fig. 164 with the test numbers filled in for comparison and reference.

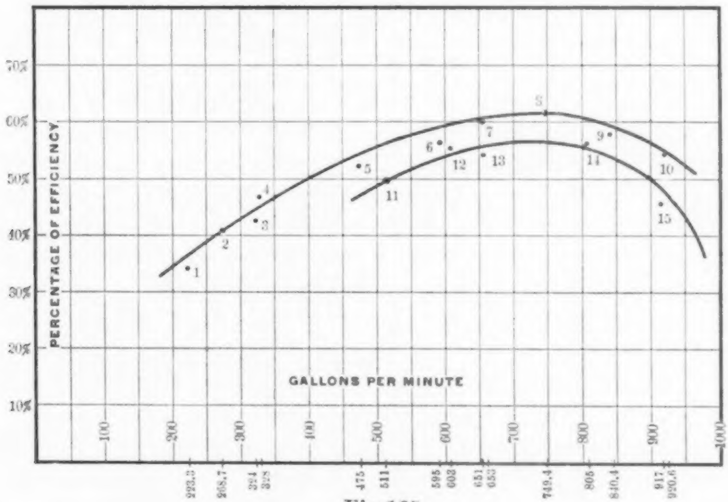


Fig.165

The writer has other series of tests in progress, not quite completed, which he proposes to embody in another paper on "Centrifugal Pump Efficiencies," which seem to show that the efficiency of centrifugal pumps increases as the size of pump increases, and which might be approximately stated as follows: that a 2″ pump (this designation meaning always the size of discharge outlet in inches of diameter) giving an efficiency of 38 per cent., a 3″ pump giving 45 per cent., and a 4″ pump giving 52 per cent., were giving as good results, proportionally, as a 5″ pump at 60 per cent., and a 6″ pump at 64 per cent. of efficiency.

TABLE OF TESTS OF CENTRIFUGAL PUMPS.

Number of test	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Revolutions of Pump	390	400	410	420	445	470	460	470	585	510	450	460	470	472	590
Height of Lift	16′ 8″	14′ 5″	14′ 5″	16′ 8″	17′ 10″	18′ 5″	17′ 2″	17′ 2″	17′ 10″	17′ 2″	16′ 5″	17′ 2″	17′ 2″	17′ 2″	17′ 2″
Time of 100 rev. of Dynamo.	10″	12″	12″	10.4″	10.5″	9.6″	10″	9.6″	9.2″	8.4″	9.7″	10″	9.7″	10″	8.5″
Net Load on Dynamo	14,850	15,900	18,200	17,850	23,200	27,700	25,905	28,036	32,790	34,188	23,500	25,200	27,500	34,815	40,000
Depth of Water on Weir	.112	.127	.144	.145	.186	.216	.230	.253	.272	.29	.198	.218	.23	.264	.289
Water in cu. ft., per min	29.78	35.93	43.85	43.80	63.51	79.36	87.14	100.42	111.83	123	68.14	80.46	87.14	107.18	122.37
Water in Galls., per min	223.35	268.75	324.26	328.5	475.05	595.2	651.8	749.4	840.4	920.6	511	603.75	653.5	805	917.77
Water in foot lbs.	31,249.4	32,474.8	39,181	45,990	70,585	91,348.3	93,457.6	107,250	124,819.5	136,730.9	73,109.9	82,974.3	93,495.7	115,068.9	131,295.3
Water Horse Power	.948	.984	1.187	1.39	2.14	2.76	2.83	3.25	3.78	4.08	2.215	2.51	2.833	3.48	3.978
H. P. for Dynamo	2.7	2.409	2.757	2.98	4.007	4.882	4.71	5.31	6.5	7.4	4.404	4.58	5.154	6.33	8.55
Efficiency = $\frac{W. H. P.}{D. H. P.}$	.35	.408	.43	.466	.532	.5668	.60	.6125	.585	.552	.50	.557	.55	.559	.465

DISCUSSION.

Mr. Thos. J. Borden.—My experience with various sizes of centrifugal pumps has shown that the velocity usually prescribed by the makers through the pipes connected to such pumps, is too high for good results, especially if the point of delivery is at any considerable distance from the pumps.

I had a case where two pumps were located ten or twelve feet from each other, with a direct lift of about fifteen feet, and then flowing through five hundred feet of pipe on a descending grade, with a total fall from the highest point of ten feet. After using them for awhile, taking liquor from two separate machines, I had occasion to combine the operations of the two in one machine, and the pipes leading from the two, and running side by side within six inches of each other, were disconnected from the respective pumps and joined by long bend L's and a long turn T to one of the pumps. The two pumps were of the same size. After resuming work, with one of the pumps doing the work of the two, with no change of belt and apparently no increase in consumption of power, the one proved amply sufficient to do the work of the two, although the pipes as originally connected to each were larger than prescribed by the makers of the pumps.

The temptation is to connect pipes of the sizes of the inlet and outlet of the pumps. Unless the pipes are very short, much better results can be obtained by using piping materially larger than the inlet and outlet of the pump, and the closer to the pump these enlargements are made, the better are the results obtained.

CCXV.

A NOVEL CHIMNEY STAGING.

BY FREDERICK G. COGGIN, LAKE LINDEN, MICH.

In the Fall of 1885, the Calumet and Hecla Mining Company completed a new brick boiler-house for its stamping and concentrating works at Lake Linden, Mich. It was 206 feet long and 70 feet wide, giving room for fourteen fire-box boilers, whose shells are 90 inches in diameter, with a total length of 34 feet.

The chimney designed for this boiler-house was to be of wrought iron, 13 feet 7 inches in diameter and 165 feet high, above the brick base upon which it stood, and the top of the latter was 20 feet above the ground, making a total height of 185 feet above the surface.

The courses were five feet high with four sheets in each course, the ends and edges butted together, the joints being covered with straps riveted to the sheets on the outside.

The first ten courses were $\frac{3}{8}''$ thick, the second $\frac{5}{16}''$, the third $\frac{1}{4}''$, the top three courses $\frac{3}{16}''$ thick. The late arrival of the material for the chimney, with other circumstances, brought the commencement of its erection rather late in the season, so that it became a serious question as to whether it could be completed in time to allow the brick lining to be put in before the freezing weather set in. In fact, it became evident that with the ordinary method of staging it could not be done. Such a staging would have required ten uprights of $8'' \times 8''$ timber, with the bracing necessary to hold them in position, and girting, and provision for a platform every five feet—*i. e.* for every course—sufficiently strong and wide to allow the workmen to stand outside for holding rivets and bolting together, all requiring not less than 26,000 feet of lumber.

Such a staging would have to be put up in sections, during the operations for which the iron work would have to be suspended, and the time put upon the staging and platforms would be nearly as much as that for putting the plates in position and riveting,

and the expense full as much. But, regardless of the question of extra cost, the delay which such a staging would occasion made it imperative to devise some more rapid method for raising the chimney, and the result was the plan illustrated in Fig. 190.

This consisted of a frame about nine feet square, with four 8″ × 8″ uprights 16 feet long, suitably braced and bolted together, with a platform at the bottom, one about four feet from the bottom,

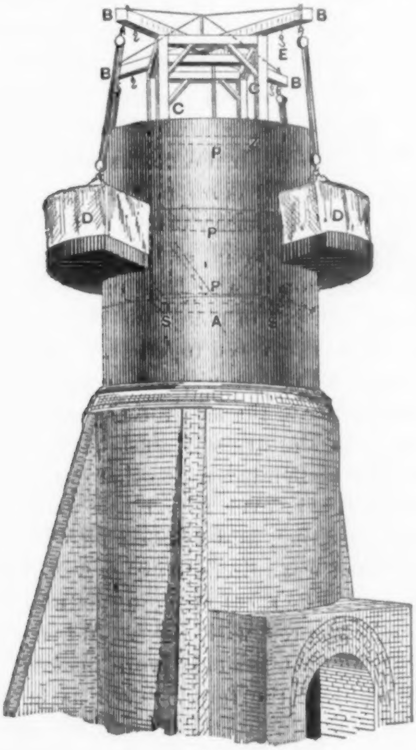


Fig. 190

which carried the workmen while riveting, and one still higher for carrying the forge, etc., the platforms being indicated by the letter P.

Upon the top of this frame were four arms, B, jointed at the center, through which it was bolted to a cross-girt, but so as to allow it to swing freely. To the ends of these arms were suspended the cages, D, by blocks and falls as shown. These cages extended a little more than one-quarter round the chimney, and

consisted of a segmental platform about three feet wide, with a railing of gas-pipe, and covered with canvas, to protect the workmen from the wind and prevent the possibility of their falling The whole thing required less than 1,000 feet of lumber. The frame having been bolted together within the chimney base, the process of erection was begun. The cast-iron ring, upon which the chimney was to rest, having been put in place upon the top of the base, a loose platform was laid over the opening, and the first two courses were raised into place with a "gin-pole" and bolted together. Two snatch-blocks were then hooked on to the upper sheet near the two opposite corner posts of the frame, at the bottom end of which were eye-bolts, into which were hooked the hoisting ropes which passed up through the blocks and down to the bottom through another pair of blocks on to the drum of a small steam hoisting machine. The temporary platform was then removed, and the frame was raised high enough so that the two sticks of timber, A, could be placed on top of the base under the uprights. The cross-bars, B, were then put in place, and the cages, D, suspended, and the two courses were riveted together. The gin-pole was now laid aside, and the third course was put in place by the method to be used from that point to the top, the ease and facility of which are worth noting. In the arms B, just back of the eye-bolts to which the cages were suspended, were other eye-bolts, E, into which was hung a snatch-block, over which was passed a rope leading from the hoisting machine, and hooked into the sheet upon the ground. As the sheet was raised, the cage was swung out to allow it to pass up behind it, the sheet swinging naturally and easily into place, where it was secured with bolts. When the whole course was thus secured, the snatch-blocks were hooked on to the top of the sheet as before, and the frame raised as before, so that the loose cross-beams, A, could be laid into the stirrups, S, which had previously been bolted in place at the horizontal seam, and from this point up, the frame—except when it was being raised—was resting upon the two cross-beams, A, hanging in the four stirrups, of which there were two sets, so that while the frame was hanging in one, the other could be transferred to the seam above. There was, therefore, no delay, for as each course was riveted up and another bolted in place ready for riveting, but a few moments were required to hook on the snatch-blocks, raise the frame, transfer the cross-beams, A, to the next set of stirrups, and drop the frame on to them. The sheet being riveted one-quarter round on

the opposite sides, the cross-bars, B, were swung so that the cages covered the other two quarters, and the riveting was completed.

In this way, this traveling staging, carrying eleven men, went to the top with no trouble whatever, the operations following each other in rapid succession, and within 27 working days from the driving of the first rivet at the bottom, the last rivet was driven at the top, including the hanging of three sets of guys, and painting the chimney inside and out. A cast-iron capping having been put in place, a permanent iron ladder was hung from top to bottom.

The cages were then lowered to the ground and the frame taken apart and dropped, two pieces of timber being laid across the top, from an eye-bolt in which were hung blocks and falls for the purpose of raising a platform which carried the masons and material for putting in an eight-inch lining, which was done in about 20 days. The blocks were then lowered and the cross-timbers dropped, and a completed chimney stood as a testimonial of the quickest time on record for such a job. The total weight of the chimney, including the base, ring, and cap, is 100,105 lbs. The cost for the labor, including punching and rolling the sheets and straps, and all labor incidental to the erection, did not exceed $2\frac{1}{16}$ cents per lb.

DISCUSSION.

Mr. Borden.—There is a chimney at Chester, Pa., similar to the one just described which was built without any staging. The upper course was put together first, then raised sufficiently to put another under it, and so on until the whole was completed. It was lined with a single course of fire-brick laid spirally.

The brick were $12'' \times 6'' \times 4\frac{1}{2}''$, and made to fit the circle of the shaft.

Mr. Kent.—I think the method of building described by Mr. Coggin is practised in other places by the builders of fire-brick stoves for blast furnaces. They require chimneys from 150 to 200 feet high. I have heard of one of that kind being built in Harrisburg.

Mr. Nagle.—The stand-pipe for the Providence Water Works was erected in the same manner. It is 108 feet high. I don't know the cost of it. It was done under contract work.

The Chairman.—The statement made by Mr. Borden is interesting. The courses are laid on a spiral, and one obvious advantage in doing it was, I presume, that the pitch of the screw was the

thickness of one brick, and thereby the cutting of bricks was avoided. The question whether the brick was a true multiple of the circumference of the circle was eliminated.

Mr. Borden.—The first course was laid rising just enough to gain the height of a brick in one course around.

Mr. Durfee.—In this connection I will say that I built one chimney carrying out the idea named, on which I laid the brick in the form of a double-threaded screw. The flue was six feet in diameter and 100 feet high. The bricks were four and a half inches wide and two and a half inches thick.

Mr. Borden.—That chimney had a stone base about fifteen feet high, and the iron tube was one hundred and eighty feet high, the iron in a few of the lower courses being $\frac{3}{8}$ inch thick, then about ten courses each of $\frac{5}{16}$ inch, $\frac{1}{4}$ inch, and $\frac{3}{16}$ inch thick, and a few of the upper courses were $\frac{5}{32}$ inch thick.

The diameter of the flue was nine feet four inches.

CCXVI.

THE PURIFICATION OF WATER FOR DOMESTIC AND MANUFACTURING PURPOSES.

BY THOMAS S. CRANE, NEWARK, N. J.

MUCH attention has been attracted of late years to the question of water purification, by reason of the constantly increasing pollution of the sources of water supply; but, so far as known to the writer, it is considered simply impracticable to purify the immense volume of water now used in our large cities. Knowing this to be the state of public opinion on this subject, it has seemed that a purifying apparatus of such effective character and such unlimited capabilities as is hereafter described would deserve notice on account of its special mechanical features, and be of interest. It not only accomplishes the removal of the grosser particles, but it is adapted, by a careful imitation of nature's mechanism, to effect the precipitation of the more subtle impurities, and to secure their removal, and the impregnation of the fluid with the needful gaseous elements.

In the course of nature, pure rain-water is contaminated by every gaseous and mineral element which it meets, on its course from the clouds to the sparkling spring where it is often found in perfect purity. Such a transformation from what we would expect to what we actually find, suggests two questions: How is such purification effected; and can the process be cheaply and successfully imitated?

It would seem that the presence of lime, iron, and alum, whose salts are so common in the earth, operate efficiently by combining with the impurities which are in solution or very finely diffused in the water, to change or collect them into masses which may be arrested by the soil. The water is then filtered by the porous rocks and strata of the earth, and is undoubtedly mingled with air in the various steps of the process, before it can present the sparkling appearance which it so often exhibits as it emerges from the earth. Such aeration may be effected in part by the currents

of brooks and the dashing of water-falls, before it is absorbed by the soil; but, however effected, it is a most efficient means of purification by the oxidation of organic matters.

The apparatus illustrated is known as the Hyatt Apparatus, and is in use upon a large scale, in many locations, effecting all that can be desired in the purification of water. Plants aggregating 3,000,000 gallons per day are in use at Cohoes, N. Y.; and plants, varying in their capacity from 40,000 to 500,000 gallons daily, are in use in the water-works of Raritan and Somerville, N. J.; in the Arlington Hotel, in Washington, D. C.; and in other hotels in Cincinnati, New Orleans, Atlanta, St. Louis, Pittsburgh, etc. One of 36,000 gallons is in operation at the Phœnix Distilling Co. in Chicago. As the water supply of cities costs about $125 per million gallons, and as the cost of operating such plants as referred to above is about $3 per million gallons, it will be seen that the increased cost for purifying water by this apparatus is but little over two per cent.

The distinctive principle which makes the filter itself effective and of permanent efficiency, is the tumbling of the filter-bed inside out and upside down when it becomes foul; and the use of the water-current to effect this, in a nearly automatic manner; so that no expenditure of force and but little skill is required to restore the filter-bed to its normal condition. This cleansing of the bed is effected by closing the outlet from the filtering chamber, and permitting the fluid pressure within to discharge the sand of the filter-bed through a comparatively small pipe into a tank full of water. The passage of the sand through such pipe scours the particles of the bed against each other and the inside of the pipe, and entirely rubs off the collected impurities, so that the water in the receiving-tank readily washes them off and removes them by an overflow pipe. The sand, when purified by this method, is left as good as new after each cleansing, and may be returned to the filter and used as at first. The operation takes from 15 to 40 minutes, according to the size of the filter, and is performed once in twelve hours or oftener, if needful.

A large area is always supplied for the passage of the water, so that the surface of the bed performs its functions effectively for a long time and the internal resistance involves but little loss of head or pressure; and the plant is provided with two or more filters where the current cannot be interrupted. The washing-tank may be above the filter, so that the bed may be returned by gravity; or

the tank and filter may be so connected with the water-pipes, that the bed may be used alternately in each, and the movement after washing be thus avoided. In the illustrations, the filters are shown with superposed tanks, and an automatic device for feeding coagulant is shown applied to the filter, as an attachment. The aerating device is also a separate attachment, required chiefly in purifying water for drinking. Filters of this construction were used at the World's Exposition at New Orleans, and purified 1,500,000 gallons of the foul Mississippi water daily, during the continuation of the Exposition, removing 10 tons of solid sediment therefrom, and delivering the fluid clear and sparkling.

How this mass of sediment could be removed from the filter-bed and the latter restored to a pure and efficient state, will be understood by reference to Fig. 192, which is a mere diagram illustrating the operation of the filter.

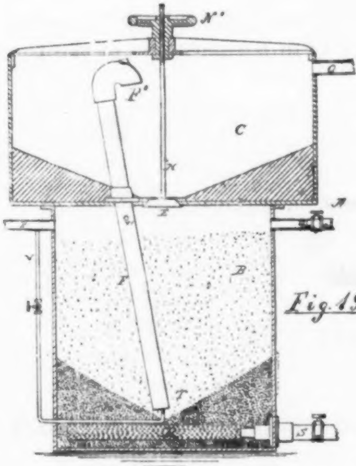


Fig. 192.

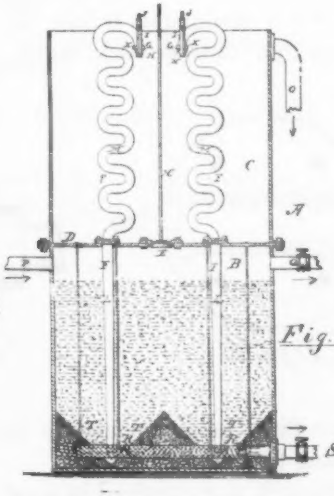


Fig. 193.

In this figure, B is the filter, T the filter-bed, P its inlet supplied with water under a suitable pressure, R a perforated strainer pipe placed beneath the bed to deliver the filtered water to the outlet S, C is a tank above the filter, and F a transfer-pipe extending from the bottom of the filter-bed to the upper part of the tank.

It is plain that if the outlet S be closed and the inlet P remain open, the water pressure within the filter will force the substance of the filter-bed up through the transfer-pipe F, and deposit it in

the tank. The tank is filled with water before transferring the bed thereto, and is provided with a waste-pipe *O* to carry off the current of foul water discharged from the bed.

The transfer of the bed is facilitated by the flow of the water through the pipe with the sand or other materials; and the whole bed being thoroughly mingled together in the process, and its particles mixed by contact with the return bend shown at the top of the pipe *F*, it is then ready for renewed use, and is restored to its place within the filter by opening a valve *E*, seated in the tank bottom and actuated by a screw stem *N* and hand-wheel *N'* above the tank.

The metal bottom of the tank may be made conical to assist in discharging the sand therefrom, or may be lined with brick-work and hydraulic cement in conical shape, as shown in the drawing.

As the filter proper remains filled with water when the bed has been transferred to the tank, the filtering material receives an additional washing as it falls into the same; the inlet-pipe being closed by a suitable cock, and a cock *Q* in a waste-pipe at the top

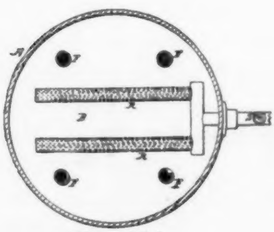


Fig. 194.

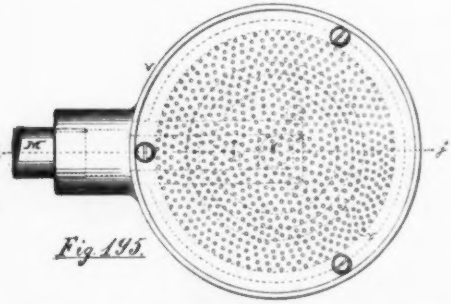


Fig. 195.

of the filter being opened to permit the water engaged in such washing to flow off.

Although not required in most cases, a small pipe as at *V* may be led from the inlet *P* to the lower end of the transfer-pipe, for introducing a jet of water directly at the bottom of the transfer pipe to facilitate the movement of the sand upward, and a hole *W* may also be formed in the transfer-pipe within the filter to admit an additional proportion of water during the transfer.

Figs. 193 and 194 show a section and plan of a filter provided with four transfer-pipes, having a serpentine form to secure the most complete agitation and abrasion of the particles of the filter-bed to detach all foreign matter. A special grade of sand,

mixed with "breeze coke," is found to be the best material for general filtering purposes. Cone-shaped formations of cobblestone and gravel are shown in the bottom of these filters to direct the elements of the bed toward the lower ends of the transfer-pipes, but are not required when the water pressure is applied at the bottom of the bed. By operating the filter under pressure up to 40 lbs. on the square inch, the volume of water passed through the filter may be very greatly increased; the tops of the transfer-pipes being then closed, to prevent escape of water, by a valve, as at *H* in Fig. 193.

Outlet strainers adapted for such a use are shown in Figs. 195

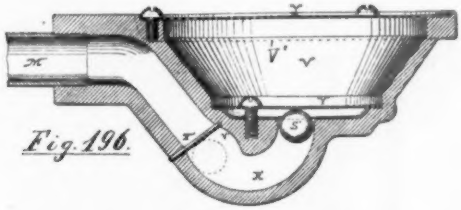


Fig. 196.

and 196, and are supplied with the water during the transfer by a connection with the inlet-pipe.

The strainers consist in funnels having their upper and lower ends covered by screens of perforated copper; and the space between these two screens is, in practice, filled with copper shot or flakes prepared for the purpose; the object of which is to permit the use of a coarse strainer, while preventing the escape of fine filtering material. The arrangement and operation of these conical strainers are shown in Figs. 197 and 198, where a filter 10 feet diameter and 13 feet high is shown, provided with all the necessary attachments for purifying water at the rate of 250 gallons per minute, more or less, according to the foulness of the water. In these views, the filter is shown provided with four transfer pipes *P*, having stationary pistons attached, by brackets, to their upper ends; and hydraulic cylinders Q^2, fitted to such pistons for opening automatically the transfer-pipes and the discharge-valves in the bottom of the tank. A plan of such cylinders and discharge-valves and their water connections is shown in Fig. 199. In Figs. 197, 198, and 199, *A* is the filter, *B* the tank, *C* the bottom of the tank braced by stay rods *W*, to support the flat ends of the filter against internal pressure; and *F* are the strainers, shown in Fig. 198, connected, in three separates series, by pipes

H, *I*, *J*, to a main discharge-pipe *G*, provided with cock *G'*. As the water first filtered after the cleansing of the bed is not clear, it is discharged into the tank for the subsequent washing. G^3 is a pipe provided with a cock G^2 to discharge the water first filtered into the tank. *K* is the inlet-pipe connected by cock K^2 to the top of the filter, and by a cock and pipe *N* to the pipe *G*, by which means the inlet current can be diverted into any of the pipes *H*, *I*, or *J*, to wash the filter-bed in sections by a current from beneath. *S* are conical rubber valves to discharge the bed from the tank after the washing operation, and *O* is a hose supplied

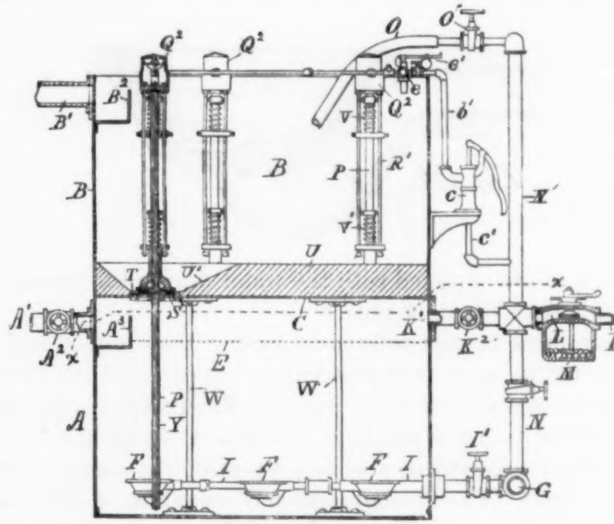


Fig. 197.

with water by a cock *O'*, for the purpose of washing the sand from the seats of the valves *S* before closing the same.

When operating the valves, the water is introduced between the piston and the upper head of the movable cylinder; each piston is supplied with water by a pipe *a'* and a header *b*, having a safety valve *e* thereon, the header being provided with water by pipes *b'* and *c'* leading through a hand pump *c*.

The hydraulic cylinders, by a partial movement, lift the caps which close the tops of the transfer-pipes, and hold them open during the transfer of the filter-bed to the tank; while a further movement of the cylinders operates to raise the valves S^1 and to permit the return of the cleansed material to the filter.

The object of the pump c is to produce an increased pressure in the cylinders where the inlet-pipe will not furnish sufficient force to raise the valve S when required. A guard B^2 is extended across one side of the tank in front of the waste-pipe B^1, by which the impurities are carried from the filter-bed when it is discharged into the tank; and a guard A^3 and waste-pipe A^1 are similarly provided near the top of the filter to discharge the impure water therefrom when the cleansed filter-bed is restored to the filter. The connection of the strainers F with the pipe G in groups is shown in Fig. 198, to illustrate a means for washing the filter-bed in sections where there is not head room sufficient to admit a superposed tank. In such case no transfer-pipes would be used, but the bed would be cleansed within the filter and the impurities discharged with a current of water from the waste-pipe A^1.

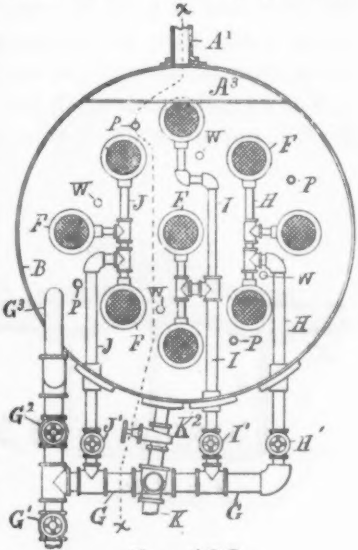


Fig. 198.

By closing the inlet-cock K^2 and opening the waste-cock A^2, and one of the cocks leading to the strainers, as H^1, the strainers connected with the pipe H would receive water under pressure

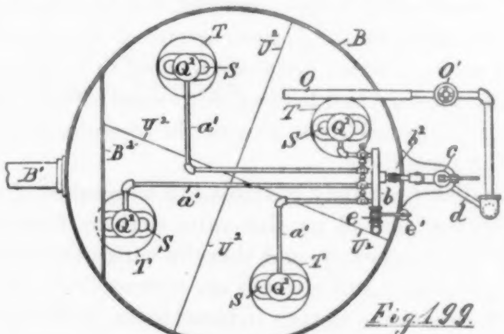


Fig. 199.

and discharge it upward through the filter-bed, loosening and agitating one section of the same and discharging the impurities deposited upon its upper surface (represented in Fig. 197 by the dotted

line *E*). When such section of the bed was cleansed, the water would be cut off from such strainers, and the other sections of the bed be successively cleansed in a similar manner. Finally, the water would be admitted to all the strainers at once to rinse out the bed, and to level its upper surface before using again. The cocks would then be arranged for the filtration to proceed downward through the bed and out by the strainers as before.

The coagulant material, in a liquid form, is sometimes supplied to the impure water by means of pumps which require some

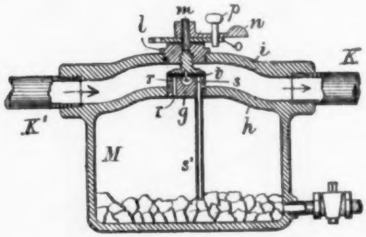


Fig. 200.

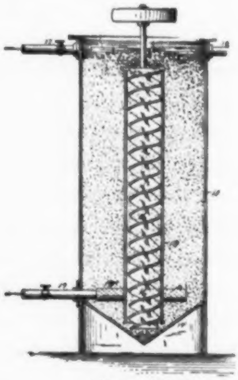


Fig. 201.

motive power, but the device shown in Fig. 200 and connected to the inlet-pipe *K*, in Fig. 197, is entirely automatic in its operation.

M is the receptacle for the coagulant, *K* a passage formed above the top of the same for the entire current of water passing to the filter, and *r* and *s* are two openings formed in a valve-seat at the middle of the passage and leading downward into the receptacle, a pipe *s'* being projected from one of the openings nearly to the bottom of the receptacle.

The valve disk *t* not only controls the admission of water to the holes, but serves, as well as the valve-seat, to form a slight obstruction in the passage *K*, and thereby varies the water pressure above the cpenings *r* and *s*, which are arranged in a line with said passage. A circulation is thus induced through the opening *r* into the receptacle; the liquid forced from the receptacle into the passage *K* being impregnated with the coagulating substance, and the proportion of fluid circulating through the receptacle being regulated by adjusting the valve disk *t*. The disk can be held in

a regulated position by a pin p inserted through a lever n into an index-plate o, and the delivery of coagulant for each setting of the disk having been determined, the proportion of the coagulant delivered per thousand gallons is thus regulated with entire certainty.

Prof. Austen, of Rutgers College, states that less than one grain of aluminic sulphate per gallon will act as a precipitant to the organic matter in drinking water, and will coagulate the suspended inorganic matters so as to make them removable by filtration. A

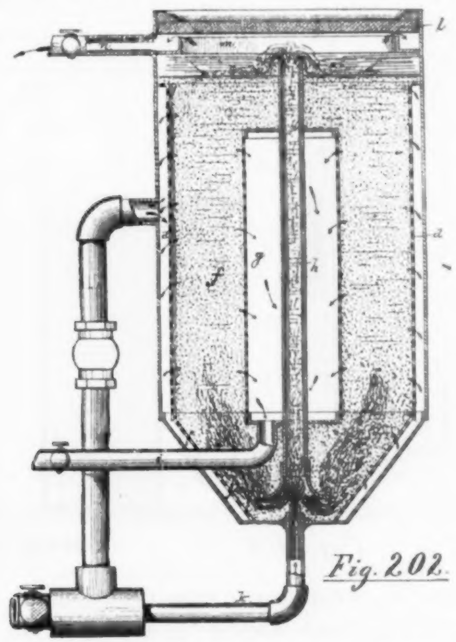


Fig. 202.

much greater amount can be used without any appreciable trace of the alumina remaining in the water. It is entirely combined with the precipitate and removed by filtration.

Alum may therefore be conveniently applied by this means, and may be safely used for drinking water; lime may be applied to remove hardness for washing purposes, and soda for rendering sulphate of lime harmless in water for steam boilers. Where the current of fluid passing to the filter is not supplied with sufficient pressure to transfer or cleanse the bed by any of the methods already shown, a mechanical means may be used as a screw conveyer arranged vertically within a tube in the center of the filter,

as in Fig. 201; the filter in such case being adapted for continuous cleansings and filtration at the same time. Where sufficient pressure is employed, such continuous cleansing may be effected by the means shown in Fig. 202, where the water is admitted by an inlet *e* to a perforated casing *d* at the periphery of the filter and discharged through a perforated cylinder *g* in the middle of the bed into an outlet-pipe *i*. A transfer-pipe *h*, provided with an inverted funnel at its lower end, is passed up through the cylinder *g*, and a jet of water from the inlet-pipe is directed from the bottom of the filter into said funnel. A check-valve is inserted in the pipe *e* and slightly loaded to produce an excess of pressure in the jet-pipe *K*, and thus enables the jet to penetrate the filter and

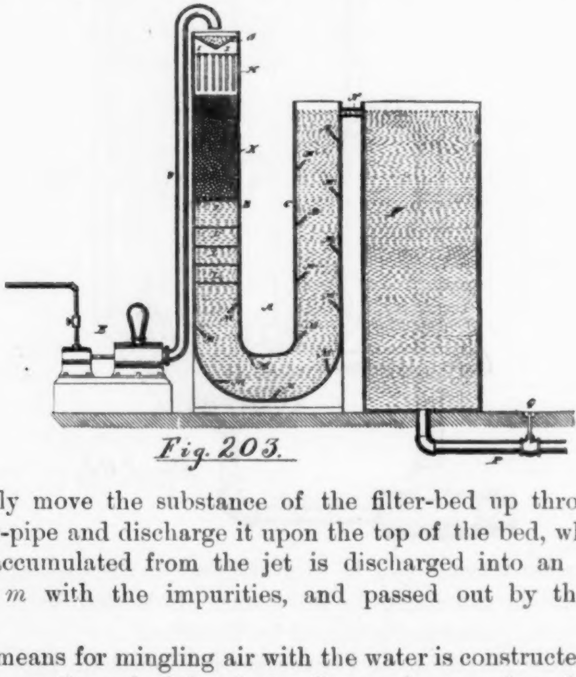


Fig. 203.

gradually move the substance of the filter-bed up through the transfer-pipe and discharge it upon the top of the bed, where the water accumulated from the jet is discharged into an annular trough *m* with the impurities, and passed out by the waste-pipe *n*.

The means for mingling air with the water is constructed essentially upon the principle of the Sprengel pump, by which the contracting vein of a descending current of water forms a sufficient vacuum to draw in the air and carry it along with the current.

The air thus entrapped combines more permanently with the water, by the subsequent application of pressure thereto; and the devices shown in Figs. 203 and 204 are adapted to subject the

mixture to such pressure by a column of water without material loss of head, and thus to secure the effective aeration of the largest volumes of water without the consumption of any power.

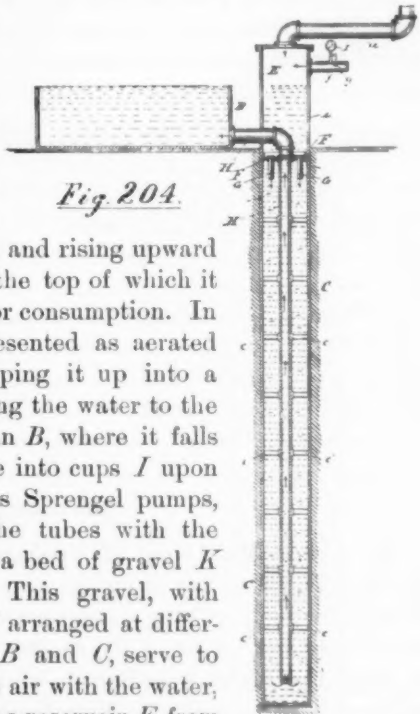


Fig. 204.

In both constructions, two vertical conduits are employed for an ascending and descending column of water, the water passing from one to the other at the lower end and rising upward in the ascending pipe, from the top of which it can be drawn fully aerated for consumption. In Fig. 203, the water is represented as aerated during the process of pumping it up into a reservoir. *D* is a pipe leading the water to the top of the descending column *B*, where it falls upon a screen *G*, and thence into cups *I* upon the tops of tubes *H*, acting as Sprengel pumps, which draw air through the tubes with the water, and discharge it into a bed of gravel *K* resting upon a grating *J*. This gravel, with screens *l*, *l*, and reflectors *N* arranged at different points in the conduits *B* and *C*, serve to intermingle and combine the air with the water, which is then discharged into a reservoir *F*, from which it supplies the distributing main *P* under the necessary head. Supposing the water to be already in some elevated site, to secure the necessary pressure in the distribution pipes, the aerating apparatus may be sunk in the ground, and the water may be led from such site into the descending conduit through a chamber in which the air is mingled with the water by the falling current, as in Fig. 204. In this construction the ascending conduit *H* is placed inside the other, and the Sprengel pumps *G* are fixed below the air-chamber *E*, through which the water falls. The air enters at *g*, through an air-pipe *f*, and a meter is shown attached at *I* to register the volume of air actually admitted. The water, when aerated, rises nearly to its original head, so that but little power is consumed in effecting the aeration. A velocity of four feet per second in the pipes is found sufficient to carry the air downward, and the pressure to which it may be subjected

has obviously no limit except the depth of the mixing conduits.

The following general statement will show the proportions and capacities which have proved successful in such plants, and is added for convenient reference:

Diameter.	Height.	Size of Supply Pipe.	Square Feet of Filtering Surface.	No. of Bushels of Filtering Material.	Min. Capacity in Gallons per Minute.
16 inches.	4 to 6 feet.	$\frac{3}{4}$ inches.	1.4	$1\frac{3}{4}$	4
24 "	7 "	$1\frac{1}{4}$ "	3.1	12	10
40 "	9 ft. 8 in.	2 "	8.7	43	26
50 "	10 "	$2\frac{1}{2}$ "	13.6	68	40
5 feet.	13 "	$2\frac{1}{2}$ "	19.6	98	60
$6\frac{1}{2}$ "	13 "	3 "	33.2	166	100
8 "	13 "	4 "	50.3	251	150
10 "	13 "	6 "	78.5	392	250
15 "	13 "	8 "	176.6	883	530
20 "	13 "	10 "	314	1,570	1.000

DISCUSSION.

Mr. Kent.—Mr. Odell, of Yonkers, New York, a member of our Society, called my attention some weeks ago to the use of the Hyatt filter for purifying water for steam boilers. I would like to hear if any one knows of the success of that method. It consists in using the Hyatt filter outside of the boiler, and establishing a continuous current all the time from the boiler to the filter and back to the boiler again. No matter whether the solids are formed by evaporation or precipitation they will be carried into that filter, and Mr. Odell told me that in their experiment they obtained some wonderful results. I heard of the Hyatt filters two years ago in New Orleans, where they were used in the sugar refineries, and the people there told me that was the best filter they had used. They were using perchloride of iron to precipitate the organic matter. As to alum, if there is no alum put in except enough to combine, I can understand how it would be precipitated, but if there is an excess of alum, that must go into the filtered water.

The Chairman.—I would say, in passing, that as the title of Mr. Crane's paper includes the word "domestic," it might have condescended to treat of such small matters as household filters, which a good many of us have to use, or would like to use if we could get filters that are good for anything. Almost all of the

domestic filters in use profess to have some method of self-purification by reversing, the absurdity of which is so apparent as to hardly need stating: You are pouring water through one end of a mass of charcoal, and when the upper end of that becomes clogged with dirt you reverse it and pour through the other end, thus recovering all the dirt extracted in the first position. That is the principle of the greater part of the ordinary household filters, most of which are worse than useless.

Mr. Crane.—I would like to add to what I said that Mr. Hyatt is now perfecting a boiler filter in which he uses his house filter. This filter has a lever at the bottom which serves to reverse the current through the filter and purify the sand, and then restoring the lever to its original position restores the sand to its original place. He connects that simple filter with the water in the boiler, and without any pump or even a steam-trap, without any mechanism whatever, he induces a continuous current of the water from the boiler through the filter automatically. I am not at liberty to state the means he uses here to-day, as he is about to patent it abroad, but the household filter is just so simple, and so is this mechanism for filtering the contents of the boiler.

Mr. Durfee.—In connection with this general subject, I will call attention to what may be called a substitute for a strainer for use in connection with filtering operations. This expedient I devised about thirty years ago, and have used it in numerous instances. It is well known that a perforated strainer is very apt to be obstructed by floating objects. We will suppose *A* (Fig. 266) to be a cistern through the bottom of which passes the overflow water-pipe *P*. The level of the water is at *L*. In order to prevent any substance floating on the water from going down the pipe *P*, we surround that pipe with a cylindrical tube, *T*. Now, anything which is lighter than water and floating on the surface, will be prevented by that tube from going through the pipe *P*. Anything heavier

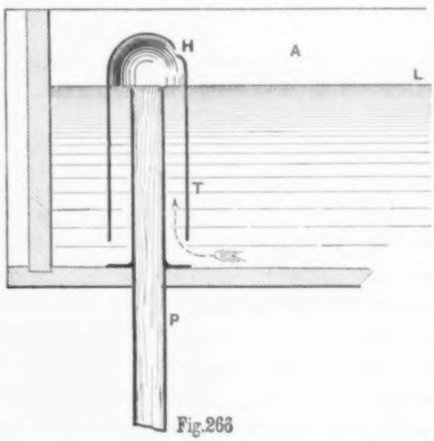


Fig.266

than water will, of course, go to the bottom. In order to prevent the outer tube, *T*, from acting as a cylindrical siphon, its hemispherical end is perforated above the water level by a single small hole, *H;* or the tube, *T*, may be left entirely open at its upper end. I have used both forms. The flow of water is in the direction of the arrow. In this arrangement we have a substitute for a perforated strainer giving the full area of the overflow-pipe, and which is at once simple, cheap, and thoroughly efficient.

CCXVII.

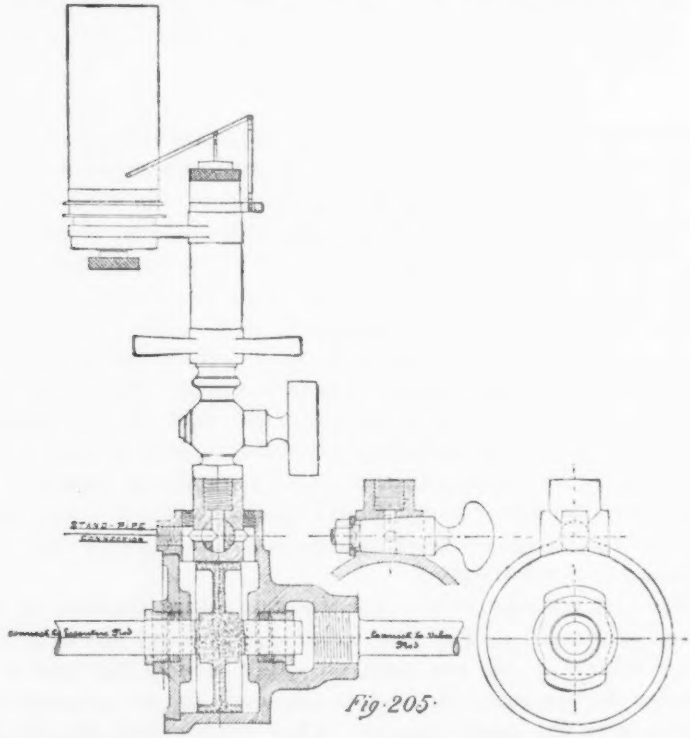
DESCRIPTION OF A VALVE DYNAMOMETER, FOR MEASURING THE POWER REQUIRED TO MOVE A SLIDE VALVE AT DIFFERENT SPEEDS AND PRESSURES.

BY C. M. GIDDINGS, MASSILLON, OHIO.

THE investigation as to the amount of power required to move slide valves has been, like most all mechanical researches, a matter of slow growth. The writer has sought in vain for this desirable information in many of the best authorities on steam engineering, and has found only various and elaborate deductions on a purely theoretical basis on the one hand, or else the individual opinion of over-sanguine inventors of slide valves on the other hand, whose opinions, by the way, were entirely unsupported by tangible facts or experimental data of any kind. He determined, therefore, some time since to enter the field of experimental research, with a view of finding, if possible, just how much power was consumed in moving the slide valves of different engines, and how much that power varied under different working conditions; such as variations of speed and pressure, changes of load and variation in point of cut-off.

The first attempt of an experimental nature resulted in the device illustrated in Fig. 205, which clearly shows the construction. Water or oil was used on either side of the piston to transfer the pressure through a stop-cock to the indicator for recording in the usual manner. After having used this, it was learned that a similar device was tried by Dean Bros., the pump-makers of Indianapolis, but without success. This device was first intended to be used with a pressure gauge, but at the suggestion of Mr. Harris Tabor, an indicator was put on to register the variations of pressure in the cylinder, but the leakage past the indicator piston from the continued action of the engine soon produced a displacement which distorted the action of the valve and reduced the travel. The indicator was then displaced by a diaphragm gauge of low pressure which had the end of the

index sharpened and turned at right angles to the dial. The glass having been removed, this gauge was screwed in the cylinder where a stop-cock is now shown, and a stationary rest provided to support the slide which carried a smoked glass. This was provided to avoid the friction of a pencil on paper, and when the glass was brought in contact with the vibrating point of the index, it described an outline on the glass which was easily preserved by blue printing, and could be made to show fairly well the relative



Fig·205·

power required to move the valve under varying loads. These cards were found to be unworthy of entire confidence, for various reasons. The movement of the index was radial instead of rectilinear, and developed considerable momentum; one-half of the stroke only could be taken at once, and in spite of all precautions displacement would soon show its effects in reducing the travel of the valve when running at rated speed. However imperfect this device and its results, it was a decided step in advance of previous experiments. Having thus found the desirable feature for a valve

dynamometer, it was determined to design and construct one which should possess as many of these points as possible.

1st. It must be sufficiently yielding to feel the variations of strains on the valve stem, and at the same time so nearly rigid, that it would give a very small fraction less than the full travel of the valve as the showing, from a reduced travel or a distorted valve action, would be entirely unsatisfactory. In other words, it must be rigid without being entirely rigid.

2d. It must show accurately the strains on the valve stem throughout the entire revolution of the eccentric, and *not simply*

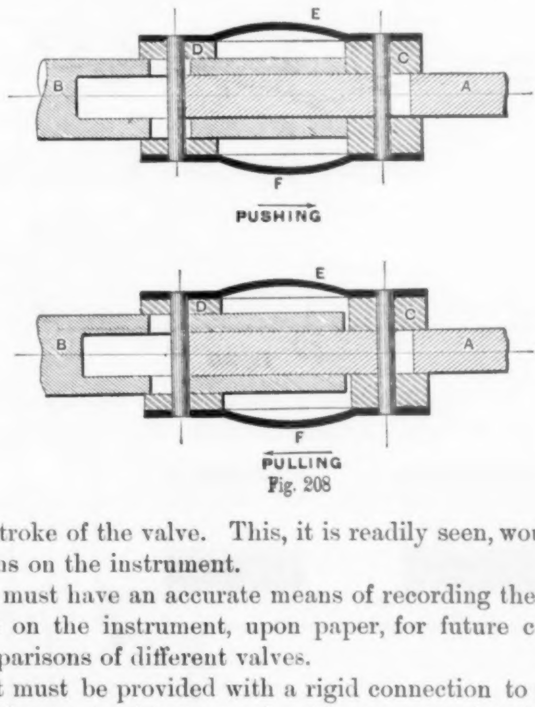


Fig. 208

for one stroke of the valve. This, it is readily seen, would reverse the strains on the instrument.

3d. It must have an accurate means of recording the variations of stress on the instrument, upon paper, for future calculations and comparisons of different valves.

4th. It must be provided with a rigid connection to relieve the springs when not taking a card, with means to detach the same readily when under motion.

5th. It must either have means of varying the tension of the springs for different size and type of valves, or be so constructed that the springs could be easily replaced with others of different tension.

The instrument designed to fill these requirements is shown in Fig. 209. Fig. 208 shows in section the mechanical principles in-

volved in the device, in which E and F are elliptical springs attached at their extremities to the sliding sleeves (C) and (D).

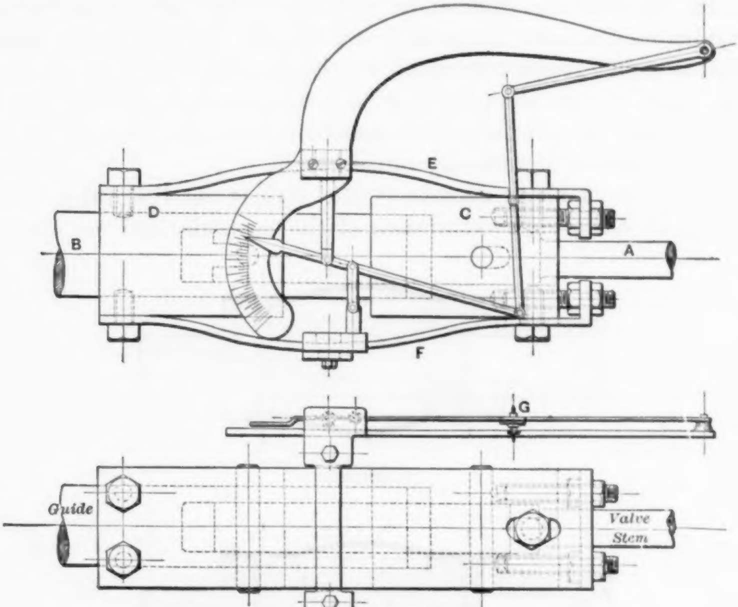


Fig. 209

The former is the valve-stem guide to which the eccentric rod is attached, and the latter is the stem itself. To each spring suitable

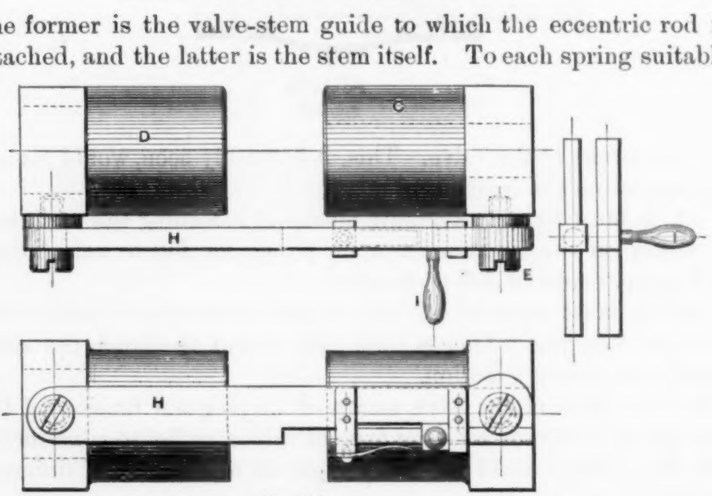


Fig. 210

attachments are made for connecting the pivoted extremities of the parallel motion which carries the pencil (G). A slide having

suitable support was provided, which worked between grooves having an adjustable stop, so that the paper mounted on the slide could be brought in proper contact with the pencil and the stop properly adjusted. Then, in order to take a card, it is simply necessary to bring the paper in contact with the moving pencil during one complete revolution of the engine. A flat-pointed brass wire was attached to the instrument, so that when the paper was brought in contact with the pencil, this point would mark the neutral line, or line of *no* strain on the card, from which all measurements were taken.

It was a comparatively easy matter to design an instrument which would show the *pull* required by the valve, but when it came to showing the *push* required in the same instrument, it was quite another thing. Of course, it was impracticable to push on the ends of the springs, consequently the strain must be taken by the springs on the *pull* through *both* strokes of the valve. How this was done can best be shown by referring to Fig. 208, in which corresponding letters refer to corresponding parts in Fig. 209. D is a sleeve, sliding loose upon the valve-stem guide (B) and attached to the springs, (C) is the sleeve sliding loose upon the valve stem (A). Each of the sleeves has a steel pin fixed in the sleeve, and passing through slots in the center of the valve stem and valve-stem guide respectively. It will be noticed that the valve-stem guide (B) comes to a bearing against the sleeve (C), also that the valve stem (A) comes to a bearing against the pin in the sleeve (D). When in operation, the valve-stem guide first pulls the sleeve (D) by means of the pin, thence the strain is transferred through the springs to the sleeve (C), which pulls the valve stem by means of its pin, as shown. But when it comes to the push part of the stroke, the valve-stem guide (B) first pushes against the sleeve

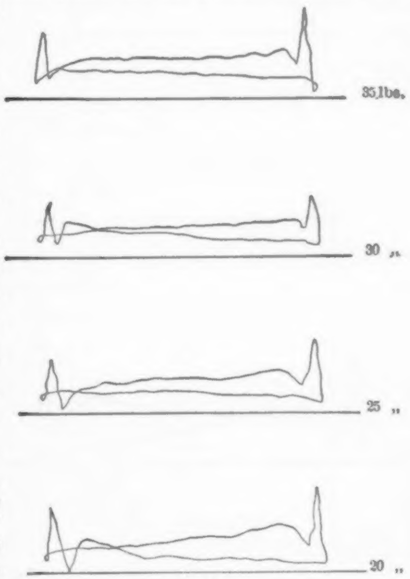


Fig. 206

(C) without moving the valve stem, thence the strain goes back through the springs to the sleeve (D), which pushes on the end of the valve stem by means of its pin, and so moves the valve. Thus, the strain always goes through the springs on the pull, and is then measured and recorded by the instrument.

The rigid connection shown in Fig. 210 consists simply of the bar (H) hinged to the sleeve (D) and hooking over a post (E) on the sleeve (C), and having a sliding catch (I) to hold it either on or off the post (E). When this connection was locked in position, it was intended that there should be no movement of the pencil, but owing to the spring of the parts, the pencil did move so that it could not be used to draw the neutral line. It was found that springs of this kind could be made stiff enough to move the valve without permitting any appreciable reduction of the stroke, and at the same time would be elastic enough to feel the slight variations of the strain, and produce sufficient movement when multiplied by means of the levers to make a good card. But this same quality prevented adjusting the springs equally for valves of different size, and it was decided to use springs of different thickness to meet this case.

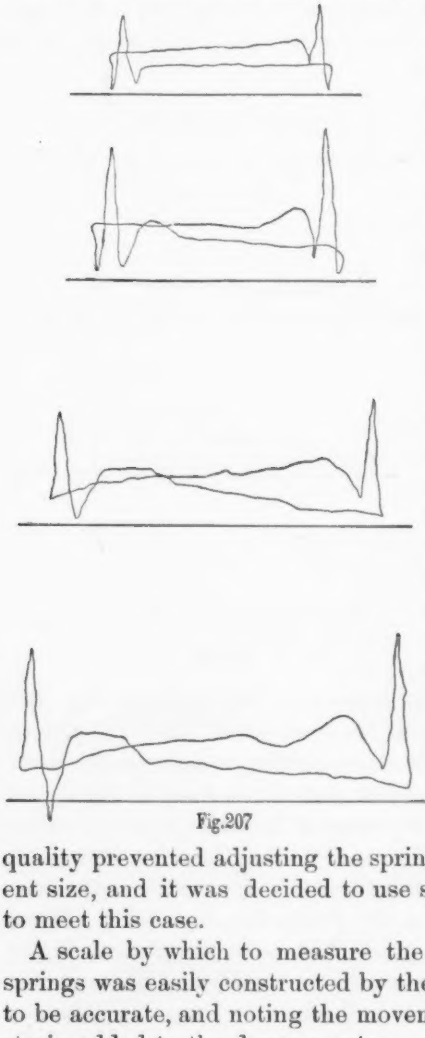


Fig. 207

A scale by which to measure the cards taken by the various springs was easily constructed by the use of a spring scale known to be accurate, and noting the movement of pencil for each 50 lbs. strain added to the dynamometer.

In computing these cards, the height gives the maximum,

minimum and average strain on the valve stem in lbs. This multiplied by the rate of movement gives the foot lbs. of work done to move the valve. In using this instrument, it was found that high speed produced fluctuations in the card, especially if the springs were too light for the valve, but with the proper strength of spring a speed of 250 revolutions per minute was entirely feasible. Fig. 206 shows cards taken with varying loads, and Fig. 207 shows cards taken from varying points of cut-off. All cards taken with any considerable load invariably show one end (and that always the same end) heavier than the other. The cause of this for a time was a mystery, but was fully and satisfactorily explained by taking into consideration the area of the valve stem, which, multiplied by the pressure in the steam-chest, worked against the instrument in one-half of the stroke and with it in the other half, making the difference in strain equal to the sum of the pressures. A table is appended which gives both the total power required to move a special type of equilibrium slide valve, also the percentage of the whole power of the engine which was absorbed by the valve. The comparative tables from different valves which follow may perhaps be found instructive :

6¾" BY 10" ENGINE.

Revolutions.	Load on Brake, lbs.	H. P. to Move Valve.	Power Developed on Brake.	Per cent. of Power Developed on Valve Stem.
125	10 lbs.	$\frac{1}{16}$ H. P.	3 H. P.	2 per cent.
175	30 "	$\frac{1}{9}$ "	9 "	1.2 "
200	40 "	$\frac{1}{5}$ "	13.5 "	1.4 "

9" BY 12" ENGINE, 100 REV. 3 PORTED FLAT VALVE.		9" BY 14" ENGINE, 100 REV. EQUILIBRIUM VALVE.	
Brake Load on Engine.	Percentage of Load on Valve Stem.	Brake Load on Engine.	Percentage of Load on Valve Stem.
5.5 H. P.	4½ per cent.	11.4 H. P.	1.2 per cent.
7. "	3½ "	13.5 "	1.1 "
8.25 "	4 "	14. "	1. "
8.9 "	6 "	15.6 "	1. "
11.1 "	7.3 "		

DISCUSSION.

Mr. Chas. T. Porter.—This ingenious instrument opens a new and interesting field of investigation. There seems to be some room for improvement in it. The diagrams appear to show lost time, some of them a large amount of it. Then the vibration of the instrument seems to have been excessive. The reaction shows the high points at commencement of each stroke to be the effect of vibration. These defects can be remedied. The first one needs to be completely removed.

A diagram ought always to be drawn, showing the force required to overcome the inertia of the valve. This in a locomotive having 5 ins. throw of eccentric, and making 250 revolutions per minute, would, if running in full gear, be 4.433 times the weight of valve and stem at the commencement of each stroke ; and proportionally less than this as the travel of the valve is shortened by cutting off. The data for computing this initial force, namely, the weight of the parts (valve and stem), the revolutions per minute, and the throw of the eccentric, or the actual valve travel, should always be given.

Diagrams should be taken from the same engine at very different speeds, under the same working conditions in all other respects. It seems difficult to imagine a case in which greater skill and care are required to secure really reliable results, than appear to be required in the case presented here.

Mr. Geo. H. Babcock.—This society and the profession generally are under obligations to Mr. Giddings for his researches and his account of the same in a line about which so little has been known hitherto. As engine builders, we have all had our ideas as to the amount of power required to drive sliding valves, but until now it has been a matter entirely of theory or guess-work, instead of positive knowledge. Early in the history of engineering in this country slide valves were discarded and poppet valves made to take their place, for the purpose of avoiding the load of the slide valves upon the engine, and the difficulty of handling engines where the friction of the valves had to be overcome by manual power. Our cousins upon the other side of the Atlantic adhered until quite recently to the barn-door slide upon all their engines, oblivious of the amount of their friction, or considering it a lesser evil than any of the numerous " balanced " valves known to them. Recently,

however, they have adopted the piston valve for their largest engines. Difficulties in keeping the poppet valve tight, when balanced, and the failure of so many attempts at making balanced slide valves, have led to the extensive use of what are known as four-valve engines, of which the Corliss may be considered the principal representative, if, indeed, not the first to adopt this plan. By the use of such four valves in place of one, each valve having reduced throw with periods of rest, the friction due to a large slide has been in a great measure overcome. Still, we have known little, if anything, in regard to the amount of this friction, or whether it was really worth the trouble which has been taken to avoid it, until the researches described in this paper were made.

A number of years ago—in fact, in 1869, at the Exhibition of the American Institute, where we had a 16 × 48 engine on exhibition with a long slide valve and a cut-off valve riding on its back—I attempted to ascertain this point by indicating the engine, unloaded first, with the full pressure in the steam-chest; the engine being regulated for speed by the governor cutting off very early in the stroke, and afterward indicating the engine at the same speed with the steam throttled down to a pressure just sufficient to overcome the friction; thinking that in this way the difference would show the friction of the slide valve due to the additional pressure in the steam-chest. Very much to my surprise, I found that the cards taken from the engine running under the first condition showed a negative pressure, or that it took less than no power at all to drive the engine; but when throttled down so that there was little or no friction in the slide valve, the cards indicated 7 H.P., in round figures, at the same speed. This proved, if it proved anything, that the slide valve took less than no power at all, and was, in fact, a source of power more than sufficient to overcome the friction in the rest of the engine. Some anecdotes related by a well-known engineer of great experience tended also to establish this same point. He stated that on board a steamer of which he was at one time engineer, the back half of the go-ahead eccentric strap was found to have fallen off some time in the night, and was lying in the bilge, while the engine continued to make its regular revolutions, with no apparent distress; the weight of the great slide valve being quite sufficient to overcome its friction and that of the stuffing-box.

His theory was that the valve floated on the steam, and had no friction. He also said that on the steamer *Thetis*, which was fitted

with a large revolving slide valve driven by gear, arranged with stops in a well-known manner, so that, by shifting the gear from one stop to the other, the engine would run forward or backward, it was found necessary to provide a means of fastening the gear upon the shaft, as, whenever the boat was in a sea-way, and the engines raced a little, the momentum of this slide valve was sufficient to cause it to run ahead and so reverse the engine. Notwithstanding, however, the singular results of my own experiments, and these stories of no friction upon the slide valve, I have always considered that it was a loss of power quite sufficient to warrant any reasonable method to overcome it.

The cards shown by the author are very interesting as indicating the difference between the friction of starting and the friction of motion. They also show what would scarcely be expected: that in nearly every instance the friction decreased during the stroke. This was probably due, in some measure, to improved lubrication, owing to the valve riding over portions of the seat which had become wetted by condensation of steam.

I trust that the author will continue his experiments with a view to determining the effect of different conditions upon the friction of the valve, as, for instance, the dryness of the steam, and particularly the use of different metals, as gray and chilled cast iron, steel and bronze upon themselves and upon other metals. My own experience goes to show that the friction of brass or bronze upon iron, when immersed in steam, is very much in excess of that of iron upon iron.

Future engineers, through such investigations as here recorded, will be enabled to avoid many of the mistakes which have heretofore been made for the want of just such knowledge.

Mr. Chas. H. Fitch.—It may be of interest to present some figures of tests of engine valves with the conditions more fully stated than they are in the tables of the paper.

An engine ($6\frac{3}{4}'' \times 10''$) was running with a valve of a partially balanced type designed by the author of the paper. Its operation will be readily understood from the longitudinal section here presented in Fig. 251. Steam is taken in the middle. The outer passages are steam passages giving the advantage of a double admission. The inner cavity, *B*, on either side is an exhaust passage.

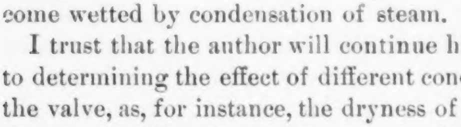


Fig. 251.

Minute orifices, *A A*, permit a slight flow of steam from the live steam passages to the steam-chest, which is in turn relieved by leakage through the minute orifices at *B B*.

The valve was $10\frac{7}{8}''$ long by $6\frac{3}{8}''$ wide. At $\frac{3}{4}$ cut off, 220 revolutions, 39.8 lbs. M. E. P., the work was 520,018 ft. lbs. The valve diagrams showed $1\frac{1}{2}''$ travel, average stress on stem, 76 lbs., work 4,180 ft. lbs. The boiler pressure was about 80 lbs. The initial cylinder pressure was 52 lbs. The steam-chest pressure calculated from the difference of push and pull on a $\frac{5}{8}''$ diameter valve stem was 67 lbs., the average of push being 96 and of pull 55 lbs.

At $\frac{5}{8}$ cut off, 220 revolutions, 39.2 lbs. M. E. P., the work was 512,179 ft. lbs. The valve diagrams showed a travel of $1\frac{1}{4}''$, average stress on valve stem, 63 lbs.—push 77, pull 49, computed pressure in steam-chest 47 lbs. The initial cylinder pressure was 56 lbs. Work of valve, 2,887 ft. lbs.

At $\frac{1}{2}$ cut off, 220 revolutions, 38.5 lbs. M. E. P., the work was 503,033 lbs. The boiler pressure was 93 lbs., and the initial steam pressure was 67 lbs. The valve travel by diagram was reduced to $\frac{9}{10}$ of an inch. The average stress on valve stem was 87 lbs., 103 lbs. push, 72 lbs. pull, computed pressure in steam-chest 51 lbs. Work of valve 2,871 ft. lbs.

At $\frac{1}{4}$ cut off, the boiler pressure being then only 50 lbs., the speed fell to 175 revolutions, diagram showing 13.3 lbs. M. E. P. Work 138,230 ft. lbs. Initial pressure in cylinder, 31 lbs. The valve travel by diagram was $\frac{8}{10}$ of an inch, average stress on valve stem 64 lbs., 75 lbs. push, 53 lbs. pull, computed pressure in steam-chest, 36 lbs. Work of valve, 1,493 lbs.

In shutting down the engine, it was noticed that the stress on the valve stem increased very much, running up to about 300 lbs., where the ordinary running stress was less than 100 lbs.

Comparative diagrams showing this phenomenon are here adduced (Fig. 252). I will not attempt to theorize upon it, but trust that the side-light thrown upon this interesting subject will be of value.

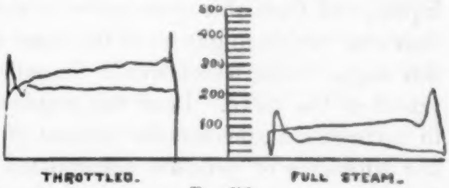


FIG. 252.

Mr. Geo. Schuhmann.—My experience with slide valves has been the reverse of what is related by Mr. Babcock. I have seen them offer so much resistance as to break off heavy rock shafts and rock

arms, and I have had to use a stream of water on an eccentric to keep it from running hot. The instrument described by Mr. Giddings is very ingenious indeed, but it does not tell us all the power wasted by the slide valve, because we must add to it the friction of the eccentric and that friction in main journals which is caused by the thrust of eccentric rod. According to the table given in the paper, the resistance on the valve stem in one case amounted to 7.3 per cent. of the total work of the engine; so that if we assume the travel of the valve to be one-sixth of the stroke of the piston, the pressure on valve stem must be equal to 43.8 per cent. of the total pressure on piston, and if we further consider the large diameter of the eccentric, we will find that this eccentric friction and side thrust friction in main bearings is of some importance.

My idea about ascertaining the total power wasted by the slide valve would be to have the eccentric shaft independent of the crank shaft (but of the same diameter as the latter), and then put a "Webber" or other lever dynamometer in between and measure the power required to drive the eccentric shaft. In order to keep the motion of the eccentric shaft steady, a small fly-wheel would have to be added. Of course, we then have the friction caused by the weight of the wheel and the shaft, but that can be easily eliminated by measuring the power required to drive shaft and wheel alone.

Mr. Wm. Kent.—I will outline a suggestion which Mr. Giddings may take up and think about as a method of determining that strain in the valve stem. Suppose the engine valve stem cut in half, and between the two parts to be introduced Mr. A. H. Emery's hydraulic means of weighing. Let there be two diaphragms held in a solid steel ring, one diaphragm connected with the valve on one side and the other to the eccentric. Fill the space between with some liquid, and from this space carry to another diaphragm of a similar character which might be of the same or of a different area, a very fine copper tube, which would be sufficiently flexible to allow the travel of the valve. Load this diaphragm down to such a point as to introduce a considerable amount of hydraulic pressure, making the difference of pressure transmitted through the stroke a small fraction only of the total pressure due to the solid weight. Weigh the pressure on this second diaphragm either by Mr. Emery's flexion balance, or by a torsion balance. We have there entirely frictionless joints in every case. I think you would get an absolute transmission of the pressure to the record; the elasticity of the flexion or torsion pivots being used instead of the spiral spring of the indicator.

Mr. Babcock.—How would Mr. Kent prevent the pressure between those two diaphragms from operating the valve directly?

Mr. Kent.—It is balanced on both sides.

Mr. Babcock.—Is there a pressure outside as well as in?

Mr. Kent.—The elasticity of the steel diaphragms acts against the pressure of the fluid inside, preserving a constant condition of strain. As there is nothing movable in the whole system, the fluid being under heavy pressure, I think the total amount of motion due to the difference in strain would be slight.

Mr. Babcock.—You would then allow the pressure to spread the diaphragms to the full extent?

Mr. Kent.—To the extent that the elasticity of the diaphragms would allow. I think that could be kept within two or three hundredths of an inch.

Mr. Wilfred Lewis.—Although I have never measured the actual resistance of a slide valve, I have had occasion to study the effect of its resistance upon the speed of an engine, and I have made experiments from which a comparative estimate of it could be formed. In running our Buckeye engine, I noticed that for a constant load, the speed varied directly with the steam pressure, and I thought the cause of the variation could be traced to the resistance of the cut-off valve.

Accordingly I connected the valve-rod with a friction slide and noted the changes in speed corresponding to different loads on the slide.

The greater the friction the greater the speed, and although the pressures applied were not very heavy, certainly not more than one hundred pounds, the variation in the speed of the engine exceeded that which might occur from any change of load.

Mr. J. F. Holloway.—The question of the load on the slide valve is one of very great interest, but at the same time, it is a question about which I think we shall not be able to establish any data which will apply to all kinds of engines. While all information we can obtain upon that point will be of great value, yet it is in the experience of engineers that the load upon the slide valve is influenced greatly by the compression, that is, a valve having a considerable compression under it moves much more easily than one with less compression. It is within the knowledge of many here, no doubt, that in rolling-mill and other engines which were worked by hand with a starting-bar, it would often happen that a young light-weight engineer could handle an engine with ease and

rapidity, while a stronger and heavier man could do nothing with it. Of course, you know that it grew out of the fact that he knew just how much compression to accumulate under the valve before he moved it. If he did not stop the valve in the right time in the stroke, and the valve got locked up over the ports, all he could then do was to shut down, and get the pressure out of the steam-chest before he could start again. It is a sort of knack, in fact, which is illustrated plainly in engines when being worked by hand. Yet that same principle applies to slide valves in engines moved by an eccentric, in relation to the amount of pressure that should be under the valve, to counterbalance the pressure on the back of the valve, at the moment when it is about closing the ports. I do not know whether Mr. Giddings has taken that into account in his paper or not, but it is one of the things which influence the amount of power required to move a slide valve.

Mr. Jesse M. Smith.—The indicator card shown by Mr. Giddings' apparatus, I think not only shows the resistance of the valve, but also the inertia of the reciprocating parts combined. Now, it would seem quite necessary to separate those and see how much is due to the inertia of reciprocating parts and how much is due to the resistance of the valve by the friction on its face; and the apparatus which Mr. Giddings introduces into the valve stem also increases the weight of it. That should be taken into account. The apparatus proposed by Mr. Kent must be necessarily quite heavy. The inertia of that at high speed would be of very great influence. An apparatus should be designed in some way to separate the two influences.

Mr. C. W. Barnaby.—It seems to me that the experiment mentioned by Mr. Lewis was not exactly to the point. On the main shaft of the engine referred to, there is a loose eccentric which is rotated backward and forward by a shaft governor through a range of ninety degrees. The application of a brake to the sleeve of this eccentric tends to turn and hold it back on the shaft with a uniform force drawing the governor levers inward, which is equivalent to changing the governor weights at will while the engine is in motion; or, more properly, to adding a certain amount of initial tension to the springs and thereby adding a constant amount to the centripetal force, which would obviously increase the speed of the engine.

Now, the resistance of the valve does not produce a constant effect by way of rotating the eccentric, by any means. The resistance to be met in operating the valve is due to friction, inertia,

and the pressure acting on the area of valve stem, and acts in two ways upon the eccentric; first, by the direct leverage of the thrust of the eccentric rod acting as a connecting rod upon a crank, and, second, by the friction between eccentric and yoke due to this thrust. The former varies from zero at the dead centers to a maximum at an intermediate point. The latter varies with the thrust and pull of the rod, which variation is due principally to the inertia of the valve and rods, and the passing of the valve over ports. Above a moderate speed the inertia would be in excess of the friction in parts of each stroke, thus exerting a force on the eccentric in the opposite direction, tending to throw it *ahead* instead of *back*. This variation in the force gives the governor a chance to recover itself at two or more points in each revolution. I do not wish to be understood as saying that resistance of the valve has no effect on the governor, but it strikes me that in making experiments to determine this effect it would be preferable to apply a force as near parallel as possible to that actually coming in play.

Mr. Jesse M. Smith.—It strikes me that the experiment of Mr. Lewis shows that the increase in the resistance of the valves, as he says, makes the engine run faster; that is perfectly true, but in the design of every governor, it is necessary to know what the resistance of the valve is, because that resistance acts as a centripetal force in aid of the spring or other means of overcoming the centrifugal force of the flying weights. Of course, if that resistance is variable or is increased from outside sources, the centripetal force is increased to the same extent and the engine must run faster, or the weight of the flying weight must be increased in the same proportion as the resistance of the valve is increased.

Mr. Wilfred Lewis.—The resistance of the cut-off valve affected the speed of the engine directly through the governor as Mr. Barnaby inferred.

The experiment demonstrated that fact, and furnished data for estimating the valve resistance from the speed, but of course not so perfectly as the instrument described.

Mr. J. T. Hawkins.—I think that really the most serious objection to this very ingenious method of determining the work absorbed by the valve is that raised by Mr. Schuhmann, and I think it would be well for the gentleman who made the experiments to take that into consideration. We all know very well what percentage of power is absorbed by what we call the friction of the load in a

large engine, and so far as the rotating parts are concerned, they are the same as applied to a slide valve, and really an accurate measure of the power absorbed in the operation of the slide valve should be made somewhere between the eccentric and the crank-pin. This instrument is very ingenious and complete, I think, although it is open to the objection that the original movement through which the indications are obtained must be so extremely small that there are elements of disturbance arising from multiplying so largely the indications at the point of record, and this is apt to render them unreliable. That should be guarded against in taking the figures from it. I think the most important consideration is, that the whole power absorbed by the slide valve should be measured between the eccentric and the crank-pin somewhere.

Mr. John E. Sweet.—It seems to me that the Society has gained one thing from Mr. Giddings. We know one fact we never knew before, and Mr. Giddings may congratulate himself upon having brought up an apparatus of such interest. In regard to one of his devices, the one which uses the pull of the spring for both the pull and push of the valve, it strikes me as being next-door neighbor to a stroke of genius.

In regard to Mr. Kent's suggestion, he would find very few engines where there is room to put on his device. In regard to one objection raised to Mr. Giddings' device, there are very few parts whose inertia would affect the result at all, inasmuch as nearly all the parts are attached to the eccentric rod end of the valve rod.

The Chairman.—While listening to the discussion, it has occurred to me that some of the objections are well made, and that a device interposed at the point described does not indicate all the resistance developed in the engine by the driving of the valve gear; that Mr. Schuhmann's point is well taken, that there are other resistances between the crank shaft and the point of application of this dynamometer. This has led me to wonder why the sum-total of this resistance might not be ascertained by driving the valve of one engine by another engine, somewhat as in the duplex pump; first taking a card from the engine to be tested with its valve gear in the usual condition and with a constant resistance, and then disconnecting the valve gear from that engine, and driving it by another engine and again taking cards and ascertaining the difference in power between the engine running under the two conditions. It would seem that in this way it might be possible to ascertain very

accurately the total power lost in the valve gear of that particular engine.

Mr. Babcock.—Allow me to suggest that that introduces the inaccuracies of several other instruments which would multiply the trouble materially. I think the better way to carry out Mr. Schuhmann's suggestion would be to mount the eccentric loose on the shaft and drive it through a dynamometer, similar to Mr. Giddings', which would thus register the power required to drive the valve.

Mr. Schuhmann.—This would still leave the side thrust friction in main journals to be accounted for; that is, the friction caused by resistance on the eccentric rod pushing the eccentric shaft hard against the sides of its journals.

Mr. Giddings.—First, taking up Mr. Porter's objection, he mentioned the fact that there are a great many chances for improvement in the instrument. No one is more sensible of that than myself. I assure the Society that with opportunities in the future I hope to show them great improvements. He speaks of lost time; if by lost time he means lost motion in the valve movement, that was extremely slight. I was determined to have ninety-nine one hundredths of the entire travel of the valve and actual measurement of the cards showing the exact travel of the valve, compared with the known throw of the eccentric showed me that little was lost, and in my best experiments it did not amount at the most to over a thirty-second of an inch.

He speaks of vibrations. As I mentioned in my paper, these are unavoidable to a certain extent, and those at the end of the stroke of which he speaks show plainly the tendency of the valve to hug its seat. When once under motion it moves very much more easily.

He speaks about the inertia of the parts. That involves a good deal when we come to think of it. How does the inertia act in affecting the power required to move the valve? As the engine moves the valve in one direction, the inertia acts to increase the resistance to moving the valve through the first part of the stroke. Then, if by virtue of that inertia the valve moves easier during the latter part of the stroke, it works in just the opposite direction to the first part, thereby constituting a decidedly variable quantity.

In regard to the inertia of the instrument that Mr. Kent mentioned, as Prof. Sweet has said, the greater part of that is taken up by being attached to the eccentric rod end of the connection; and but little of that was felt. I realize that great care is required to show the strains accurately, and will say that this is only one step

in the process of arriving at the facts. This is the third instrument which has been made for this purpose during the past three years, and is far from satisfactory to myself in many points. Still, I present it to the Society as a means of setting them to thinking on the subject, and showing them what has thus far been attained, and what they may look for to a certain extent, and what they must avoid.

Regarding the objections brought up by the different speakers, Mr. Babcock speaks of the advantage of showing the friction of different metals. That is something I have had in mind, as well as the effect of different proportions of valves on the same engine. This by means of false seats can be decided easily I think.

Another thing has been spoken of, that the instrument does not show necessarily what is the load on the engine to move its valve or how that load varies with the proportions of the ports of the seat of the valve or the valve itself. I did not try to ascertain this by itself, inasmuch as these were not the conditions which I was trying to measure. I was not trying to measure what friction the eccentric absorbed nor the pillow-block nor the crank-pin. That was not what I was after. A dynamometer placed on a shaft will no doubt serve an excellent purpose, and nobody would be more interested than myself in watching the result of such an experiment. But that would necessarily involve a change in the angular advance, because you must have flexible connections, and with that change of angular advance would come a certain amount of the distortion of the valve movement. I wanted to obtain as nearly as possible the exact showing without distortion, without change from the normal working conditions of the valve.

Mr. Kent speaks of an instrument, and in passing I will say a few words in regard to that. I can imagine that his instrument might be efficient in the matter of push, but I think he would find very unsatisfactory results in the pulling of it. However, it does away with leakage which produces displacement.

Mr. Holloway speaks of the varying conditions of different engines. That is very true, and I would say that he has touched on a very important point. Because one engine shows certain results, it cannot be said that much can be based upon them for other engines. Varying conditions enter into the design and construction of the different engines, so that it would be almost absolutely necessary to have an application to each individual engine of different design in order to arrive at the bottom facts.

Mr. Smith speaks of the inertia of the instrument, which has been touched upon before, and part of which is worthy of consideration, but in regard to the inertia of the valves, I think that has been somewhat covered by helping through part of the stroke and retarding through the balance of the stroke.

Now, if there are any designers of engines here interested in this matter, who want to try this instrument on their engines, I should be happy to afford them any help I can in the way of blue prints, and should be very glad to hear from them as to the results obtained, and if the experiments were made with care it would add to our knowledge in this direction.

CCXVIII.

HANDLING GRAIN IN CALIFORNIA.

BY JOHN H. COOPER, PHILADELPHIA, PA.

THE State of California thirty-five years ago was a land unknown to husbandry or to trade. To-day, with her 99,000,000 of acres of territory, she takes the second place among the largest States of the Union. In the year 1883 she produced 43,000,000 bushels of wheat and 10,000,000 bushels of barley.

In the year 1884, the wheat product, the largest crop ever raised in the State, amounted to 57,420,188 bushels, nearly 3,000,000 of bushels more than was produced by any other State in the Union, and this gathering was derived from 3,587,864 acres of soil. In addition to this grand total, there were raised 5,988,316 bushels of corn; 23,432,240 bushels of barley; 3,050,672 bushels of oats, and 141,015 bushels of rye.

In round numbers, it may fairly be said that one-eighth of the wheat crop of the United States and one-fifth of the grain exports of the entire country have been furnished by this State.

To gather this enormous fruitage from California's empire of grain, to prepare it for haulage and to send it forward properly, from the inital heading process in the field, to the final stowage of the filled sacks in the holds of ships for transportation abroad, either many men must lay a hand to the work in the primitive way of the pioneer farmer, or the ingenious few must devise mechanical means in connection with rail facilities for putting this business into a parallel with modern efficient and economical methods.

But, first of all, glance at the fields which are to be gleaned. In a stage journey from Santa Barbara to Los Angeles the road passes along the border of the San Fernando ranch, which comprises several tracts, amounting in all to 121,000 acres, a part of this, say 12 miles square, is seen to be growing wheat, and the whole of it in full view at once. The question naturally arises, what means are employed for plowing this extended plain? A

furrow 12 miles long in one direct line is a novel sight to eastern eyes, but the second problem seemed more difficult of solution. The methods of securing the grain from this wide expanse in the short time allotted for such work and to do it in good order would appear an impossible task if accomplished by the use of eastern agricultural appliances alone, which in thought we would appoint for this service.

Again, on the rail journey of nearly 500 miles in going from Los Angeles to San Francisco, after circling the giddy heights of the Tehachapi mountains, passing the "loop," where the railway coils upon itself like a serpent, the road descends through a score of dark tunnels and wild cañons till the early dawn brings the traveler in the plain again rolling down the magnificent valley of the San Joaquin. From this on throughout the day the line passes in the midst of a sea of grain, wide as the visible horizon and as long as a rail journey from sun to sun.

If to these be added the equally expanded valley of the Sacramento and further a multitude of valleys of like productiveness and of only lesser area, we shall begin to see somewhat of the wheat-producing surface which goes to make up the more than 3½ millions of acres of California's grain fields.

In order to describe fairly the means now employed for putting the standing grain into sacks, as fully as pictures and words are capable of, three illustrations are given herewith of three different machines, showing the latest improvements which the necessities of broad farming in California have made upon the sickle and the fan.

Since the year 1840 very many attempts have been made to construct a machine which would cut, thresh, clean, and sack ripened grain at one operation.

Up to about the year 1879 these efforts failed of complete success, but at this time Mr. Daniel Houser—a plain farmer living near Stockton, Cal.—undaunted by the failures of others, determined to construct a practically perfect machine to solve the problem of heading, threshing, cleaning, and bagging grain simultaneously and on the run through the field.

Mr. Houser as a farmer, knowing the wants of a farmer and being a natural mechanic as well, proceeded in a common-sense engineering way to construct a machine which would do first-class out-of-door work on the ranch in the grain field.

An illustration is chosen of his combination Header and

Fig. 211.

Thresher (Fig. 211), which will show to eastern eyes what manner of machine it is. During the years since, this harvester has proven itself to be of great value as a labor and grain saving machine, not one having failed to work to the entire satisfaction of its purchaser. These machines are built comparatively light, they are very strong and are well balanced. The best materials are employed in the construction of every part, they are so designed as to do their work as well on uneven as on level ground; they are adapted to cutting through heavy as well as through tangled and lodged straw, and are built capable of making clean cuts of 12, 14, 16, 18, and 20 feet in breadth. They are constructed so as to be pulled or pushed over the ground, and supplied with gearing or belting as may be desired by the purchaser.

To give some idea of the performance of this class of machines, the following miscellaneous data are on record: Four men and sixteen mules, averaged from 20 to 35 acres a day for fifty days; one thousand acres cut in thirty days, and grain put in sacks for 85 cents per acre, including hire of men and team; on fifteen hundred acres of knolly land the grain was sacked for $1.50 per acre, twelve hundred acres in forty days, making 19,250 sacks, with a 14-foot machine using from fifteen to twenty-two horses according to the nature of the land; and other similar records might be quoted, showing that it costs less to sack the grain with these machines than to stack the grain only by the old system, and with less waste of grain than with the threshing machines only.

The "Minges," of which an illustration is given in Fig. 212, is called "The Pioneer Pull and Belt Combined Harvester," and for it is claimed a superiority over geared harvesters.

Its capacity in width of cylinder and separator enables it to handle the grain for a 30-foot cut. As the header part is detachable, any size of header from 12 to 30 feet can be successfully used. The standard size is 14 feet. They require one animal to each foot of cut on hard land.

The 26-foot cut machine harvested during two months 3,000 acres, 1,600 of which were cut in 25 days and with an outlay for extras of less than one cent per acre.

Runaways are rendered impossible, as the machine can be instantly stopped by means of a double brake connected with the driver's seat, and is also within reach of the sack sewer and separator man. The wheel can be thrown into the ground 12 inches in going 10 feet.

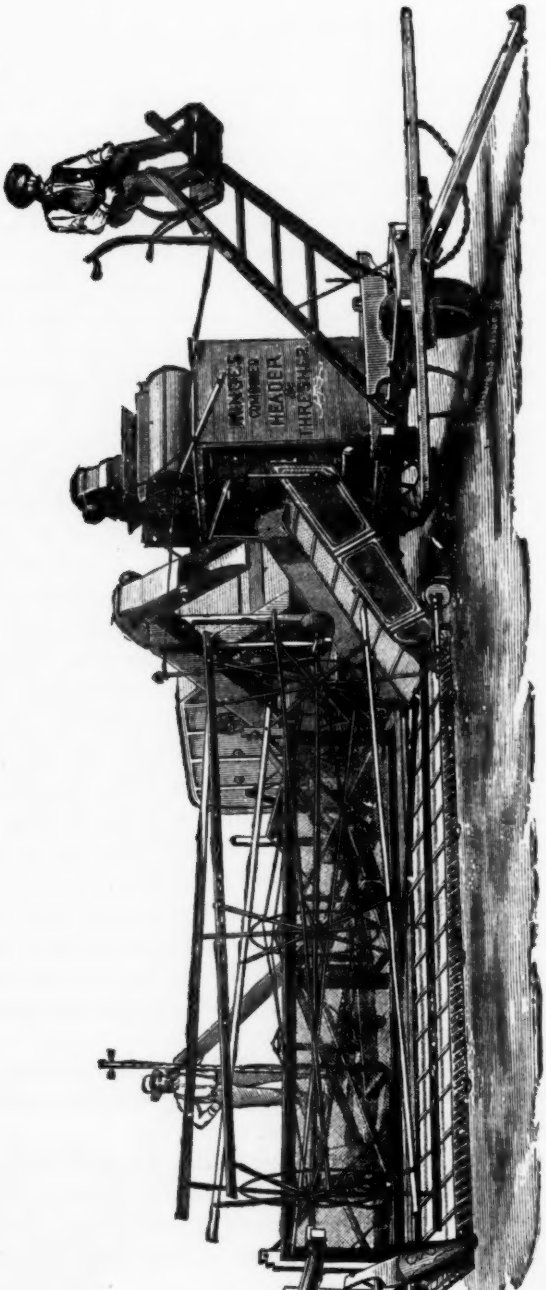


Fig. 212.

FIG. 213.

At a contest of Combined Harvesters, August 13, 1885, near Stockton, before the committee appointed by the State Board of Agriculture to examine harvesters in field work, there was reported for the "Minges" $41\frac{2}{3}$ sacks harvested in 58 minutes with a 16-foot cut.

An illustration is also given of the "Shippee Combined Harvester," Fig. 213, which, like the others, is made up of substantial framing, iron work of best material, steel gearing and shafting. It is claimed to be one of the best "push" machines, reducing the cost of harvesting to less than 3 cents a bushel, and placing the grain in sack at less cost than putting it in stack by the old method.

These machines have saved enough grain over and above the old system of heading and threshing to pay the cost of harvesting. They are the only combined machines which have an elevator to run the straw into the header wagon, saving it all if desired, also the chaff, wild oats, and weeds, and clearing the land of all foul seeds. The platform and sickle adapt themselves to the inequalities of the land, and a low or high cut is easily taken. The improved steering gear enables the man standing back of the platform (shown in the cut of the Minge Harvester) to have complete control of the machine and a near and clear view of the grain and sickle-bar, to guide with ease, to make the width and height of cut, and to avoid all stumps, trees, and rocks. The driver, at ease on his spring seat, gives attention exclusively to his team.

After the bags are filled and sewed up they are dropped from the machine on the stubble, from which they are picked by hands and put into wagons which follow after the header, thence to be hauled to a suitable place in the field and temporarily stacked, biding their proper time to be conveyed to some depôt to await the time of sale and delivery—the extension of the dry season into the autumnal months giving ample time to effect these transfers.

And now comes the needful employment of machines for facilitating what might be called the *relay* stacking of the filled bags in railway station or steam-boat wharf warehouses. To economize space, this stacking is usually carried up to the roof-beams of the building, which argues forcibly the necessity of employing mechanical appliances for extending the arms and strength of men to points beyond their reach. The little, handy, portable machine invented and built by Mr. Ira Bishop, of San Francisco, does this work so readily that a description of it fits well in this place.

This machine consists of a double endless chain belt or carrier

having peculiar sack hooks or rests, mounted by means of suitable driving mechanism upon a portable frame, to which are attached top and bottom adjustable receiving and discharging aprons.

The lower or receiving apron or table is hinged to the sides of the frame to permit it to be turned up out of the way when not in use, or to allow the whole machine to occupy least room in passing through narrow alleys between piles of sacks. The upper or discharging apron is made adjustable to various inclinations, in order to suit the grade of the chute attached to it to any pile of sacks and to the height of the pile it is supplying. The driving mechanism consists of sprocket-wheels engaging the chain carrier, one pair of spur-wheels, and two hand cranks on the pinion shaft. The grain is brought to the machine on hand trucks over the warehouse floor from the field and road wagons, and each sack placed by hands upon the lower apron in succession; as the sack hooks pass up through the slits in the apron, they lift the sack from it and carry it up the incline, as shown in the cut; when reaching the top it is dumped upon the upper apron while the hooks return through the slits cut in the same, the sack meanwhile sliding by gravity down the apron over the chute onto the pile, whence it is placed in position by hands ready to build and bond the pile to size and in proper order.

FIG. 214.

With this machine, Fig. 214, two men can raise 4 sacks of grain, each weighing 140 lbs., in the same time that one was raised by the older methods, and two men can elevate 500 to 600 sacks of wheat in one hour from a warehouse floor to the top of a pile twenty sacks high. It saves the labor of four men to each pile, and will raise sacks up to the tie-beams of any warehouse, the machine working under them being made of different heights to suit. An ordinary laboring man will raise 500 sacks per hour with more ease than he could handle one end of each sack in the usual way. This elevator is mounted on four patent castors, making it easily portable over any warehouse floor, and can be secured at any place by a few turns of four spud-screws in the corners of the frame, the

points of which enter the floor. It is always ready for use, without adjustment or change of any of its parts. It is well braced, of light frame, having a total weight of 400 lbs., and is spoken well of by all warehousemen who have seen it or who have put it on trial.

The bags of grain have now been stacked in temporary depôts located at widely distant points all over this great State, where it must needs await the proper time of shipment by rail to the seaboard.

The choice of location for a principal receiving and discharging warehouse involves certain favorable considerations. It must be on the main line of railway, to secure facility of transportation, and it must be at water edge, to which the largest ships can come and where they can lie in repose from storms and at berth while receiving cargo. The construction of so large a building must be at the least expense consistent with greatest capacity of storage; the price of the ground upon which it is to stand must not be exorbitant; accessibility to both sides and to the whole length of it by rail is necessary, while deep water by its side must be secured and maintained.

A glance at the map of California shows that almost the whole of the water-shed from the Sierra Nevada Mountains, which rise along the entire eastern boundary of the State, is delivered into the beds of two great connecting rivers and valleys: that of the Sacramento coming down from the north, and that of the San Joaquin running up from the south, both areas forming an immense basin and covering a space in the aggregate apparently one-third of the whole State. The one natural outlet for all these waters is through a chain of bays, chief of which is the Bay of San Francisco, extending to the southward from the open way to the sea, and lying nearest to it. The waters first unite in Suisun Bay, which lies to the extreme eastward, thence they pass to north-eastward through the Straits of Karquinez into the Bay of San Pablo, which rounds the northernmost end of the group; from this on the flow is to the southward, meeting the waters of San Francisco Bay, whence all move westerly into the Pacific Ocean by way of the Golden Gate. Somewhere on the shores of these grandest of bays, a shipping point would naturally be selected; not necessarily very near to the city of San Francisco, because the main line of railway does not reach it, nor yet, as they are made, on any available part of the shores of any one of these

bays, because of their very wide and shallow beaches, which would necessitate long and costly approaches and track extensions, providing it is desirable to build a warehouse, as it must be, beside the deep waters.

The location, although 35 miles from San Francisco, is well-chosen; it lies opposite the town of Benicia, on the southern shore of the Straits of Karquinez, two miles north-west of Martinez and one mile south-east of Port Costa; the port of entrance of all west-bound trains, which is made over the back of the *Solano*, the largest ferry-boat, so called, in the world.

The available ground for building is narrow between the place of deep water and the lines of railway which run closely to the bulk of the hills. In plan the littoral edge is curved, with the greater circle at the base of the hills; the radius roughly stated is a mile, having its center located somewhere within the precincts of Benicia. This slender segment of wetted foot-land embraced in the site of building and track departures has a chord of 3,250 feet. Within this crescent figure, curving with its curve to a radius of 4,723 feet, the buildings and approaches have been erected, the whole structure resting upon driven piles, which are surmounted by a solid and even floor above the level of the main lines of rail-

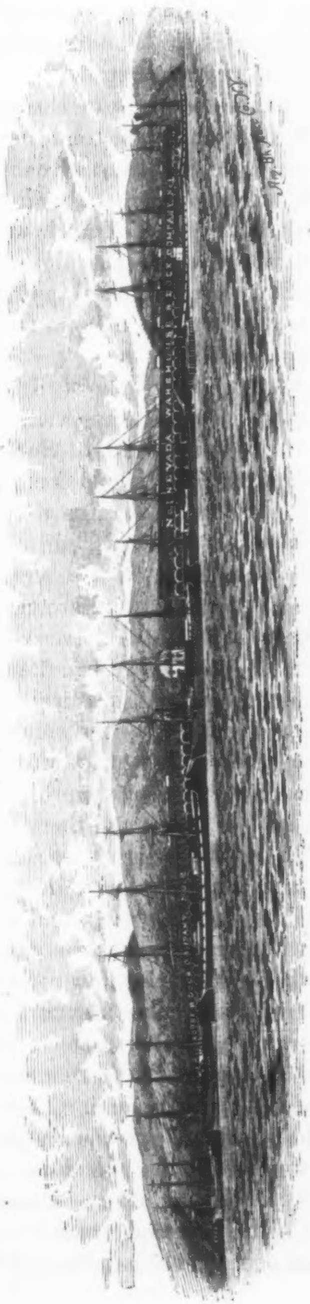


Fig. 215.

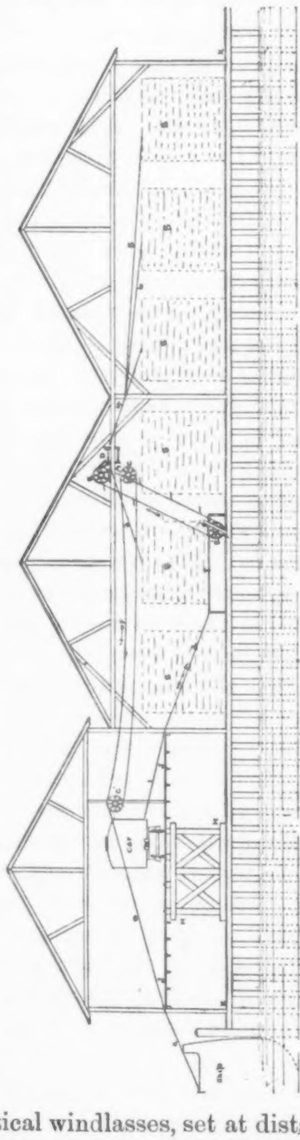
way. The scour of the outflowing waters during the run of the tide through this bend is sufficient to keep it as now, a very deep channel, always open.

A panoramic view of the whole structure is offered in Fig. 215, as it now stands, beyond the waters of the strait, with a line of ships at wharf, trains of cars upon the railway approaches, and with a background of rounded hills, the usual horizon of a California landscape. This is reproduced from a photograph taken from the Benicia side of the strait, and admirably pictures the immense storage and shipping establishment erected by and conducted under the auspices of the Nevada Warehouse and Dock Company.

The improvements of this Company, comprising the docks, buildings, and railway switch connections, cover, under franchise grants of the Board of Supervisors, 3,250 feet in length, and a width from 150 to 300 feet, or thereabouts, of State submerged land. The dock frontage of 2,300 feet will afford space for eight or ten large ships, with facilities for hauling into or changing berth and loading, which are nowhere else to be found. The warehouses, of which there are two sections, one of 770 and the other of 912 feet in length, have in their greater extent a width of from 110 to 170 feet, with over 190,000 square feet of floor area, and 20 feet clear height for storage. Besides which, under the trestle supporting the covered, elevated double railway track, on the water-front side of the warehouses, is an area of over 80,000 square feet, completely protected from weather and largely available for storage should there be occasion to use it for this purpose.

The elevated double track, which by an easy rising grade from the main line of railway brings the grain-loaded freight cars onto a platform 50 feet wide and 11 feet above the floors of the warehouses and wharves, is completely covered and protected from weather. This elevated road is one of the grand features of the establishment. It extends the entire length of the whole warehouse system, whence bags of grain, from the cars, can be slid down chutes to the warehouse floor or into the holds of vessels by aid of gravity alone. These tracks are on the water-front side of the warehouses, while another track extends the whole length of the warehouses on the shore side, all of which connect with the tracks of the Southern Pacific Railway. This accomplishes a meeting of rail and water transportation in a way most favorable to the economical handling of the staples of the country.

Between the approaching ends of the two warehouses is a space of 150 feet. On the elevated railroad side of this and near midway, is a handsome three-story office building with high mansard roof surmounted by an ornate cupola. The first story on the wharf level is fitted up for offices and occupied by grain shipping firms. In the second story, on the level of the elevated railway track, are arranged the counting-rooms of the company. The third story is provided with sleeping apartments and conveniences for the resident manager, while the attic contains capacious water supply tanks. Additional buildings, including a hotel for the accommodation of the large staff of weighers, clerks, shipping agents, and others have been built following the completion of the warehouses and to meet the exigencies of the growing business.



Cross Section of Ware-house No 2 Machinery End
Scale [illegible]
FIG. 216.

The power for driving the grain cleaning and grading machinery, the sack elevators and carriers, the fire pump and other machinery is furnished by a 100 horse-power O'Neil cut-off engine and horizontal tubular boiler, and is carried by a line of shafting under the main floor, through the grading house, across the western end of warehouse No. 2 to the water-side, where it is further employed to operate a swinging crane for discharging ballast and a series of "gipsies" or small, vertical windlasses, set at distances of two or three hundred feet apart between the tracks of the elevated railway, and around which a turn of rope can be thrown as

they revolve, to draw loaded freight cars to any desired point, when there is no locomotive at hand, to haul ships into the dock, change their berths, and perform other pulling service.

The grading-house is provided with the best and most improved grain cleaning and grading machinery and has a capacity of 600 sacks per hour, or 800 tons in 24 hours.

Lines of 6″, 5″, and 4″ iron water-pipes extend from the steam pump in the engine-room the entire length of the warehouses and dock range ; these are furnished with 33 hose connections, at distances of about 128 feet apart and on each side of the warehouses, to which sections of standard San Francisco Fire Department hose are attached ready for use. An auxiliary fire-engine pump and boiler for rapid steam generation are located and arranged for quick use in case steam is not " up " in the larger boiler.

A cross sectional view of warehouse No. 2, cut through the end near the space between the two where the motive machinery is placed, is given in Fig. 216, showing, in a general way, and without much regard to exactness of dimension or fullness of detail, the mechanical appliances for elevating the bags of grain to a higher level and for conveying them back and forth and across the warehouse. The relative positions of ships at wharf and the cars upon the elevated railway are plainly seen.

The elevators, of which there are seven, are located at nearly equal distances along the line of the main conveyer *D*. They consist preferably of broad heavy belts to which steel brackets or rests are riveted in cross lines of 3 each, each set properly distanced to give time for hands to place the bags upon them, as also to enable bags to get out of the way of one another in transit from the elevator to the conveyer. The upper pulley *E*, carrying the elevator belt, is secured upon a shaft which is driven by the main line of shafting through counter-shafts and bevel-gears. The lower elevator pulley *E′* is carried in a tightener slide beneath the table *T*, upon which the bags are placed, ready to be slid onto the rests of the elevator belt over a grating in the table-top, through the slits of which the rests pass in their upward flight. The carrying capacity of these elevators is 20 sacks per minute.

The conveyer *D* is an endless double chain of iron links embracing steel axles, with a flanged wheel running loosely upon each end of each ; the links are connected crosswise by strips of wood securely bolted, three to each pair of links. Upon these strips of wood the bags are delivered lengthwise at *D* by the elevator *E*.

Continued double tracks, composed of light iron bars, "edge laid" upon wooden stringers, extend above and below the roof tie-beams nearly from end to end of warehouse. The conveyer is capable of being driven in either direction at a speed of 165 feet per minute, by proper open and crossed belts upon counter-shaft pulleys; spur-gears being employed to overcome the great resistance of so long a linkage laden with weight of many bags of grain at once.

The belts run on large pulleys at high speed on a level with the tie-beams and within command of hands at warehouse floor by convenient strap shifters.

The cross conveyers, of which there are four, run from the main conveyer to points over the elevated railway, delivering the bags to a height about level with the top of a car body. They are run at a speed of 220 feet per minute, and are composed of heavy belts with cross strips of wood, the ends projecting beyond the edges of the belt and resting upon and are guided by ways framed to and supported by the building. The return fold of the belt is allowed to sag freely, and by its own weight produces sufficient grip upon the 5-foot driving pulley *C*, which is located beneath the main conveyer so as to receive bags therefrom and send them across the warehouse in the direction of the arrow *F* over the belt-returning pulley *C'*. This pulley is carried on adjustable bearings in a frame-work supported upon the floor of the elevated railway.

The gravity transfer of the bags is conducted on inclined chutes, on which, when worn smooth by use, and having guiding strips on each edge, the bags are found to run on a descent of about one in four.

These chutes are portable and adjustable every way, and are shown in some of the positions in which they are used. Nos. 1 and 2 extend from car door to table *T*, where the bags are placed in succession against, and to be taken by, the up-going belt of the elevator. Nos. 3, 4, 5, and 6 are placed to right and left from the conveyer to any stack, *S*. No. 7 is fixed in position between the main conveyer and the cross conveyer, and Nos. 8 and 9 conduct the bags into the hold of a ship from the pulley *C* of the cross conveyer.

The discharge of the bags from the main conveyer at any desired point is effected by a smooth-faced board placed upon edge close over and at an acute angle with the line of the conveyer. The head end is firmly secured, and the tail end rests loosely in a

vertical slit. This arrangement, simple as a railway switch, acts as well in practice. It yields to the bags at first contact, and then straightens out automatically with the persistence of a spring, throwing the bags from the conveyer "end on," to finish their journey down the chutes, thus illustrating the ease with which movement is made after motion begins, when the difficulty of "starting" friction is overcome.

Bags of grain can be brought from any part of the warehouse to be cleaned and graded, if necessary, and returned to be re-stacked after this process has been completed; they may be sent from either end to the other extreme end, taken over the distant cross-conveyer and put into car or ship, or they may be sent from the middle of the warehouse to either end.

This machinery will deposit bags of grain into hold of ship as fast as they can be taken from stacks, and weighed and trucked over the warehouse floor to the nearest elevator.

The number of bags that can be gotten out of a car, slid down the chutes, and delivered upon the conveyer by one elevator in one hour is about 1,200.

The main conveyer has a longitudinal movement in either direction throughout the whole length of the warehouse connecting with four conveyers moving transversely toward car or ship, and provided with seven elevators extending from floor to the tie-beams of the roof, equally spaced along the main line of transfer. When these are taken in connection with the elevated railway, upon any part of which cars can be placed, with the wharf extending the whole length of the warehouse where ships can lie in berth, and the facility with which numerous chutes can be placed in descending lines from any chosen number of elevated points, it can be readily seen that the whole area of the warehouse floor, the entire lines of wharf and railway, are within easy reach. Finally, it may be added, that to any point desired on these a continual stream of bags of grain can be automatically sent and delivered.

At present this system of handling bags of grain is confined to warehouse No. 2, but a similar plant can be extended throughout the entire area of warehouse No. 1, by carrying the conveyer and shaft lines for driving the elevators across the intermediate space between the two under a covered way and over a supported framework at even height of present conveyer and in same line therewith.

The following statement will show the tonnage of grain handled at Nevada Dock, Port Costa, from June 1st, 1884, to June 1st, 1885:

Flags of Vessels Loaded.	Number.
American	55
British	96
German	5
Norwegian	1
Dutch	1
Italian	2
French	1
Total number	161

Total quantity of grain in warehouses on storage at any one time during the year, 78,000 tons.

Total quantity of grain put on board of vessels out of cars, craft, and warehouses for the year, 327,688 tons.

These warehouses were erected under the superintendence of Mr. Ira Bishop, who also invented and patented the bag-handling machinery, which was planned and made at the shops of the San Francisco Tool Company, at the time under the management and superintendence of Mr. John Richards.

The engines, boilers, and main lines of shafting were made and erected by the Union Iron Works.

It is an agreeable task to write of things seen in a land, the visiting of which was a delight, the leaving of which was a regret, and the retrospect of which is more than a remembrance. When so many substantial evidences are seen, as now, of prosperity in agriculture, in manufactures, and in commerce, we may lament the waste of work and life in the rage for getting *grains of gold*, but we can feel glad that this has given away to the more peaceful and profitable pursuit of raising *golden grain*. No longer does glittering promise alone tempt the fortune-seeker from eastern homes and occupation. The insignificant nugget which first met the eyes of James Marshall in the saw-mill-race at Sacramento, fancy may form into the first link of a golden chain which now

represents $1,500,000,000 of the world's wealth of precious metal. Vast as is this sum, it is trivial when compared with the value of the yield of the soil, and the worth of this is not to be measured wholly by the money which it brings in the world's market.

DISCUSSION.

Mr. T. W. Hugo.—On reading Mr. Cooper's paper, any person at all conversant with the method of handling grain on this side of the Rockies must be struck with the great difference between such handling and that described, and would be apt to be critical. But when we consider the difference in the climate, and find that this climatic difference is the reason why we cannot make use of such machines as are described in Figs. 211, 212, 213; or why, instead of the mammoth elevators to which we are accustomed, it is possible to use, to advantage, the low warehouses of Figs. 215, 216, we can see that a comparison of the two systems, so widely different, is useless, because each is the best suited to its particular conditions. Speaking of the immense wheat-fields of Minnesota and Dakota, the home of the "Hard" varieties of wheat, it is not possible to bag our grain at the same time it is cut. It must be allowed to dry for some days after all chance is cut off of moisture being transmitted from the root to the berry. A period of dry weather is not sufficient; the drying process must take place after the grain is cut. After being threshed the grain is hauled, generally in bags (but hopper wagons are coming more and more into use), to the grain market, and stored in small elevators of from 10,000 to 40,000 bushels capacity. These elevators are an adjunct to almost every village on a line of railroad, the grain being stored in bulk by grade, but sometimes in special bins. From these small elevators the grain is shipped in bulk in cars to Duluth, St. Paul, Minneapolis, etc., where it is again stored in bulk by grades, or in special bins, where it may remain, if in good condition when received, for years. Some two-year-old "No. 1 Hard" handled in Duluth this spring was adjudged by State and private inspectors to be in perfect condition, and worth more than that of a more recent crop of the same grade. Owing to our intensely cold winters and general cool weather, even in the hottest days, a loaded elevator is a pleasant refrigerator, and as late as in August it is a painfully cold job to be obliged to work in a bin of wheat which has not been disturbed since the winter.

Now let us trace a cargo of, say, 90,000 bushels on the steamship *Onoko* (which cargo has been weighed up in one hour and twenty-eight minutes, and the vessel completely loaded in two hours and five minutes), which in 100 hours after leaving Duluth is being unloaded into an elevator at Buffalo, whence it is shipped by railroad or by canal-boats to New York. The canal-boats carry about 8,000 bushels, and are propelled by either steam or animal power. At New York it is again stored previous to shipment, always, up to this time, in bulk. From the Atlantic ports some grain is shipped in bulk, but the larger amount is bagged previous to shipment. Some of this grain may go to Montreal via Welland Canal, stopping at Kingston, Ont., for lightening to river draft. What reaches Montreal on the propellers is very often elevated, by means of a floating elevator which is placed between the ocean vessel and lake craft, or the floating elevator has a bagging attachment. The grain being elevated by the floating elevator out of the hold of our lake steamer drops into the bagging hopper, is bagged, and skidded into the hold of the ocean vessel. Oftentimes the wheat is going into the vessel on one side while the process of unloading is going on from the other onto the dock.

In the elevator system described by Mr. Cooper, one thing is very noticeable—the small amount of power provided, a 100 horse-power engine being sufficient. The paper says the total quantity of grain in warehouses, at the place mentioned, at any one time is 2,600,000 bushels, and presuming that the 100 horse-power engine supplies power for that amount of storage capacity it is a remarkably small showing. If it were for one of our elevators of that capacity an engine capable of developing from 500 to 1,000 indicated horse-power would be necessary.

But the most interesting portion of this interesting paper to me is the description of conveyers for conveying grain in bags wherever needed. In a paper read before this Society at the Atlantic City meeting, on "Belts as Grain Conveyers," I described the Duluth system of conveying grain in bulk, by means of belt conveyers, necessitating an 1,800 feet belt in some cases. The power required to drive those long belts is a very difficult matter to determine with any degree of accuracy, as so many disturbing factors step in to perplex: the humidity of the atmosphere; the temperature; the lubrication of the hundreds of bearings; the tightness of the belt and the amount of grain on it, as well as the length of the stream of wheat, all make a combination of seemingly innumerable possibilities of

variation, so that I would never hazard a guess as to how much power would be required unless I could specify the kind of weather, what the oiler should be fed upon, and be able to control all the other factors, and then I would be better satisfied if I could measure it before committing myself. To give a general idea of the power required, the following data are given, referring to the description given in "Belts as Grain Conveyers" for further information in regard to the elevators and conveyers mentioned:

Elevator B, lower conveyer, speed 760 feet per minute; 11,000 bushels per hour.

Power to overcome friction of engine and 13″ shaft, and 300 ft. of 6″ to 4″ shafting upstairs in cupola....47 I.H.P.
Power to drive conveyer belt light....13 "
Power to drive belt when above amount of grain is on....38 "

Elevator B, upper conveyer, speed 775 feet per minute, 13,500 bushels per hour.

Power to overcome friction of engine, etc., as above....45 I.H.P.
Power to drive conveyer belt light....24 "
Power to drive conveyer when above amount of grain is on....60 "

Elevator E, lower conveyer, speed 720 feet per minute, 8,300 bushels per hour.

Power to overcome friction of engine and shaft, and 185 feet of 6″ to 4″ shafting upstairs in cupola....36 I.H.P.
Power to drive conveyer belt light.... 8 "
Power to drive conveyer when above amount of grain is on....33 "

Elevator E, upper conveyer, speed 800 feet per minute, 8,300 bushels per hour.

Power to overcome friction of engine and shaft, etc., as above for E...36 I.H.P.
Power to drive conveyer belt light....20 "
Power to drive conveyer belt when above amount of grain is on.......52 "

To those of us who are not familiar with California surprises, Mr. Cooper's paper reads almost as a romance, so very different is the method of handling grain in the North-west, and when we consider the millions of bushels handled by each system, each the outgrowth of necessity, we have another instance of the greatness of this nation, where such great diversities of practice are demanded by natural requirements, and all under the one flag.

Mr. Henry R. Towne.—Those acquainted only with New England methods would find it difficult perhaps to credit the statement, that this paper illustrates a harvesting apparatus which requires at times from fifteen to thirty head of cattle to draw it, cuts a swath fifteen to twenty-five feet in width, mows the grain down, threshes it, winnows it, puts it into bags and delivers the bags on one side. Such things can, of course, only be used on fields which are measured, as the author says, "by the rising and setting of the sun."

CCXIX.

THE RELATIVE VALUE OF WATER GAS AND GAS FROM THE SIEMENS PRODUCER, FOR MELTING IN THE OPEN-HEARTH FURNACE.

BY FREDERICK W. TAYLOR, NICETOWN, PHILA., PENN.

SINCE 1876 a large number of articles have been published in pamphlets, in the manufacturing and scientific journals, and in the proceedings of engineering societies, setting forth the advantages of water gas for heating purposes. Almost any engineer, after reading these papers, unless he himself carefully examined all the calculations, would conclude that water gas was quite as economical for all heating purposes, such as the generation of steam, house warming and cooking, etc., as coal used in "direct firing," and that it was far more economical than any other gas used for heating purposes. One pamphlet, which has had a wide circulation, after using data which the writer believes to be erroneous in determining the calorific effect of water and Siemens' gases, and the amount of gas produced per pound of coal burned in the producer, states as a conclusion, that, the cost of a given quantity of heat obtained from the Siemens gas is to that of the same quantity obtained from the Strong gas (water gas) in the proportion of 2.5 to 1.

Some time since the management of the Midvale Steel Co., with which the writer is connected, contemplated an addition to their open-hearth furnace plant, and it was in determining what sort of gas it would be advisable to use in such furnaces that he had occasion to read as much literature on water gas as he could find, and make the calculations, of which the results are given beneath. As his conclusions, after having made his own calculations, differ very materially from those which he had formed after reading the water-gas articles, they are here presented, as they may be of interest to some of the members of the Society.

The writer in this paper has confined himself, first, to estimating the cost of a given number of heat units produced by the combus-

tion of water gas, as compared with that of the same number of heat units produced by the combustion of Siemens' gas, giving incidentally as a matter of interest the results of some calculations about Pennsylvania natural gas, pure marsh gas, pure carbonic oxide gas, and pure hydrogen gas. In determining the above cost, he has entirely left out of consideration the labor used in making the gas, and the interest on plant, etc., obtaining merely the cost of the combustibles used in generating a sufficient amount of either gas to produce a given number of heat units.

Second, to determining the theoretical flame temperatures obtained in the open-hearth furnaces by the combustion of water and Siemens' gases.

Appended to this paper will be found a table giving a summary of the results of some calculations on the heating properties of several kinds of gases. It will be noticed that the sample of natural gas is of considerably better quality than the average Pennsylvania natural gas; also that the samples of water gas taken had passed through the purifier, and that it was of better quality than would probably be obtained for heating purposes.

The flame temperatures recorded in the table would, of course, never be attained in practice, as the gases would have reached their points of dissociation before arriving at so high a heat. These figures are, therefore, only useful for purposes of comparison.

In calculating the quantity of heat developed by the various gases, the writer has used the valuable tables compiled by Mr. Magnus Troilius, and published in "Notes on the Chemistry of Iron." They are based on the experiments of Messrs. Favre & Silberman.

By reference to the appended summary, it will be seen that in every respect the water gas is superior in quality to the Siemens gas, developing 2688 heat units per cubic meter, or 302 B. t. u. per cubic foot of gas, as against 1123, or 126 B. t. u. per cubic foot, yielded by the Siemens gas, and giving a theoretical flame temperature of 2764 degrees C. (5007° F.) against 1800 degrees C. (3272° F.) Siemens. So that were the question of whether to use Siemens' or water gas to be decided by these figures, there would be no doubt as to the answer. By reference, however, to the relative cost of the two gases given further on in the table, it becomes doubtful whether the advantages obtained from the superior quality of the water gas are not much more than counterbalanced by its excessive cost. In estimating the cost of the Siemens gas, the writer has taken the gas made at the producers in use at the Midvale Steel Co.

as an average sample. These producers are not of the most improved style, having been built before 1871, so that the gas which is taken here as the basis of our calculations is by no means of the very best quality.

The analysis given beneath is selected as being about the average of a large number of analyses made by Mr. Troilius of samples of gas taken from the above producers. It is as follows:

$$\begin{aligned} CO_2 &= 1.5 \text{ vol.} \\ CO &= 23.6 \text{ vol.} \\ H &= 6.0 \text{ vol.} \\ CH_4 &= 3 \quad \text{vol.} \\ N &= 66.9 \text{ vol.} \end{aligned}$$

The data used in estimating the cost of the gas are as follows: One ton (2,000 lbs.) of Pennsylvania screened slack (bituminous coal, which the Midvale Steel Co. have found, on the whole, the most economical to use for gas making) costs them delivered at their producers \$2.90. The ash and coke produced (which were weighed for several months) are together 14.4 per cent. of the coal charged in the producer. The tar and soot deposited in passage-ways leading to the furnace also determined by weight are .92 per cent., and it is assumed that one per cent. of the gas made leaks out. From the above data it is found that one lb. of coal will produce 83.04053 cubic feet of gas. This is readily calculated in the Siemens producers, as the gas analysis shows the proportion of combustible present in the gas, and none of the gas, except that which makes the .92 per cent. of tar and soot mentioned above, goes to waste before reaching the place in which it is burned. One cubic foot of Siemens gas develops 126.288 heat units (lb. Fahrenheit), so that one dollar spent on coal will pay for 689.64 lbs., which will yield in burning 7,228,834 heat units (lb. Fahrenheit).

The cost of water gas cannot be so readily obtained, as the analysis of the gas does not afford the means of calculating the coal required to produce it. During that part of the process of making water gas in which the air is blown through the coals, the products of combustion either pass to waste, or, in the most improved plants, yield a part of their heat to a regenerating chamber of some description. No calculation can of course determine the amount of heat which actually goes to waste under these circumstances.

After carefully sifting all of the articles on water gas which he has been able to obtain, and having seen the several types of water-

gas furnaces in operation, the writer has come to the following general conclusions:

1st. That water-gas producers of the modified "Strong" type are to be preferred to other types for making gas to be used for heating purposes, since in the former the products of combustion formed while air is being blown through the coal, as well as the hot-water gas itself, yield part of their heat to regenerating chambers through which the steam and air afterward pass on their way to the hot coals, while with other types of producers a part of this heat at least goes entirely to waste.

2d. That although in some cases the heat contained in the waste gases above referred to, might be used in generating steam or for some other useful purpose, the large part of the water-gas producers would be so situated as to make it impracticable to do so.

3d. That it is necessary, in order successfully to use a fuel in any water-gas producers which have yet been constructed, that it be broken into lumps certainly not finer than egg coal; that it be of such a quality that when subjected to a high heat it will neither decrepitate nor cake, and that it be entirely free from dust.

4th. That about 50,000 cubic feet of gas can be made from 2,240 lbs. of anthracite coal (this includes also the coal used in generating steam to be used at the producers).

The only fuels available at Philadelphia for making water gas, which the writer knows of, are anthracite coal and coke. The cost of the latter places it beyond consideration. Anthracite egg coal would cost delivered at the works of the Midvale Steel Co., $3.90 per ton (of 2,000 lbs.). One lb. of this coal will produce 22.32143 cubic feet of water gas, which will yield in burning 7147.509 heat units. In obtaining this figure the writer has assumed that water gas produced from the above coal will be of the same quality as that recorded in the table appended to this paper. It would, of course, not be as good as the Lowe gas which had been freed from carbonic acid by passing through the purifier. From the above data we find that $1.00 spent on coal will pay for 513 lbs., which will yield in burning 3,666,672 heat units. Using these figures to compare the commercial value of Siemens' and water gas at Philadelphia, we find that $1.00 spent for coal for the Siemens producer will pay for as many heat units as $1.97 spent for coal to be used in the water-gas producers. If a suitable quality of anthracite coal could be bought for the same price per ton as bituminous coal, then $1.00 spent for coal for the Siemens producer would pay for as

many heat units as $1.46 spent for coal to be used in the water-gas producers.

Even supposing that with a mixture of an inferior coal consisting of $\frac{1}{3}$ dust and $\frac{2}{3}$ egg anthracite costing at Philadelphia $3.20 per ton, we could make 70,000 cubic feet of water gas (which the writer believes to be the largest estimate offered by any of the writers on water gas), we should then be able to make as many heat units from $1.00 worth of coal burned in the Siemens producers, as we could from $1.15 worth of coal burned in the water-gas producers.

The temperature, however, at which the two gases burn in the open-hearth furnace cannot be left out of consideration. In open-hearth furnaces the incoming gas and air are not unfrequently heated to a temperature which will melt cast iron. Assuming this to be 1,670° C., we find that while Siemens' gas burns in the open hearth (after both the gas and the air have been heated so as to enter the furnace at a temperature of 1,670° C.) at a theoretical flame temperature of 3,668° C.; water gas burned in a furnace in which the air is pre-heated to 1,670° C. (the gas being admitted at the temperature of the atmosphere), will burn with a theoretical flame temperature of 4,271° C. This makes it possible to construct an open-hearth furnace with smaller regenerator chambers, if water gas is used instead of Siemens' gas.

It is possible that the higher flame temperature of the water gas might enable us to melt steel with the use of fewer heat units than are required with the Siemens gas. It is also possible that using the same number of heat units combined with the high temperature of the water gas, we would be able to melt more rapidly with water than with Siemens' gas; but it seems scarcely probable that the increased efficiency of the water gas would overcome the difference of 97 per cent. in cost for a given quantity of heat in favor of the Siemens gas.

There still remains to be considered the difference in the cost of the Siemens and water-gas plants, and the price of the labor involved in using the two systems. In view of the greater simplicity in all respects of the Siemens plant, it is safe to say that the Siemens system would again have the advantage here.

The following are the conditions under which it might be advisable to use water gas in an open-hearth furnace:

The fuel must be such that it will neither decrepitate nor cake when subjected to a high heat; it must be broken into lumps not

SOME CALCULATIONS ON THE HEATING PROPERTIES OF VARIOUS GASES, WITH ESTIMATES OF COST.

Name of the Gas.	Where Sample was taken from.	Chemical Composition.	Theoretical flame temperature of the gas when burned with just enough air to insure perfect combustion.	Theoretical flame temperature of the gas when burned with pure oxygen.	Heat units (Kilogramme Centigrade) developed per cubic meter of gas consumed.	Number of heat units (Kilogramme Centigrade) required to raise the temperature of the gases, resulting from the complete combustion of one cubic meter of the combustible, one degree (Centigrade) in temperature.		Heat units (lb. Fahrenheit) developed per cu. ft. of gas consumed.	Number of heat units (lb. Fahrenheit) required to raise the temperature of the gases, resulting from the complete combustion of one cu. ft. of the combustible, one degree (Fahrenheit) in temperature.		Relative volumes of the various gases which have to be used to produce the same QUANTITY (not degrees of sensible temperature) of heat, taking natural gas as a standard.	Cost of coal at the works of the Midvale Steel Co., Philadelphia, per 1,000 cu. ft. of gas made.	Number of heat units (lb. Fahr.) developed by the combustion of the amount of gas which would be produced by $1 worth of coal at Phila.	Cost of the coal at Philadelphia necessary to generate a sufficient quantity of gas to produce a given number of heat units.
						Burned in Air.	Burned in Oxygen.		Burned in Air.	Burned in Oxygen.				
Natural Gas of Pittsb'gh.	Analysis made by Dr. G. Hay.	Co = 1. vol. CH_4 = 95. " H = 2. " O = 1.30 "	2,333° Cent. 4,231° Fahr.	7,100° Cent. 12,812° Fahr.	8141.	3.489	1.180	915.	.2177	.0736	1.			
Water Gas.	Sample of gas taken from Lowe's gas producers, after passing through purifier at the Novelties Exhibition, Phila., Oct. 16 '85.	Co = 44.5 vol. H = 50.9 " O = .7 } air. N = 2.8 } 1.1 undetermined.	2,764° Cent. 5,007° Fahr.	6,849° Cent. 12,360° Fahr.	2688.50	.9736	.3925	302.	.0607	.0244	3.02	8.72 cts.	36,66672	$1.97

Siemens' Open-hearth Gas.	About an average sample taken from Siemens' producers at Midvale Steel Co., Nicetown, Phila., 1883.	Co_2 = 1.5 vol. Co = 23.6 " H = 6.0 " CH_4 = 3.0 " N = 65.9 "	1,600° to 1,800° Centigrade, 2,912° to 3,272° Fahrenheit, according to the quality of the gas.	2,700° to 3,000° Centigrade, 4,892° to 5,432° Fahrenheit, according to the quality of the gas.	1123.41	.6227	.3673	126.	.0388	.0229	7.25	1.745 cts.	72,28834	1.00
Marsh Gas.		CH_4	2,331° Cent. 4,229° Fahr.	7,133° Cent. 12,871° Fahr.	8482.	3.645	1.180	953.	.2275	.0742	.96			
Carbonic Oxide.		Co	2,894° Cent. 5,241° Fahr.	7,075° Cent. 12,767° Fahr.	3007.	1.039	.425	338.	.0648	.0265	2.7			
Hydrogen Gas.		H	2,665° Cent. 4,829° Fahr.	6,944° Cent. 12,531° Fahr.	2665.	.996	.3823	299.	.0621	.0238	3.5			

finer than egg coal, and the same amount of carbon (or its equivalent) as is contained in one ton of the best anthracite coal, must not cost more than 70 per cent. of the cost of good bituminous coal. This would enable us, as far as the cost of the fuel is concerned, to make heating gas as cheaply by the water gas as by the Siemens process. Such a fuel as the above might be the refuse coke from puddling furnaces or from retort gas works. Prof. Egleston, of Columbia College, informed the writer of the successful operation of a water-gas plant which he had seen at Essen (not in Krupp's works), in which they used refuse puddling furnace coke. This, the writer believes, is the only case in which water gas has been commercially successful for heating purposes abroad.

After all, the question still remains, as to whether any fuel, which is suitable for making water gas, cannot be made to yield more heat units by being turned into Siemens' gas.

I would call attention to two facts which have come under my observation since preparing the foregoing.*

* Contributed in MS. at the meeting at which the printed paper was read.

A few weeks since the Midvale Steel Company began the use of gas made from Phillips' producers in the open-hearth furnace. This gas has many points of resemblance to the water gas. It is made from hard coal by forcing air through the bed of hot coal with a steam blast, and if it did not contain the nitrogen from the air, it would have a chemical composition very closely resembling that ot water gas.

Analyses of a number of samples, taken just before the gas entered the furnace, showed it to have a higher calorific power than the Siemens gas made from soft coal in producers adjoining it. But in spite of this, the amount of coal used per pound of steel melted was very high, running sometimes as high as one pound of coal per pound of steel, and the furnace worked very slowly. Thinking that the trouble might lie with the furnace, we changed to the use of Siemens' gas made in our older producers, and obtained satisfactory results. We then returned to the use of the Phillips gas, and studied more carefully into the causes for its unsatisfactory working. We believe that there have been two of them.

First. The gas burned with an almost colorless flame, so that it was exceedingly difficult to judge when the proper mixture of air and gas had been attained. Doubtless at times a great excess of gas went through the furnace, while at others an excess of air was used.

After having analyzed the waste products of the furnace, and learned better how to regulate the supply of gas and air, there still remained the second objection, which we have not succeeded in overcoming. This is, that the gas burns with a short flame. The combustion is very rapid and intense for a short distance beyond the point at which the air and gas meet, so that the brickwork is there heated nearly to the melting point, while the other end of the furnace is comparatively cool.

The Siemens gas, although not burning with as high a flame-temperature, contains a considerable amount of marsh gas and carbon, mechanically held in suspension, which causes it to burn with a luminous and a long flame. The luminous flame enables the melter to regulate the relative quantities of air and gas used with great nicety, while the long flame renders the heat throughout the furnace more uniform. We are now using a mixture of Siemens' and Phillips' gases in the furnace with economy, and without injury to the furnace.

It would seem that the experience which we have had with Phil-

lips' gas would be repeated in the use of water gas in an open-hearth furnace of ordinary construction, and that the objections would be even increased. It might, however, be possible to construct a furnace in which the air and gas could be projected with force upon the bath of hot metal, and in that way save the roof and entrance to the ports from damage.

DISCUSSION.

Mr. Wm. Kent.—There is one method of calculating the relative efficiencies of the two gases, which I think Mr. Taylor might look up with advantage. It is that the efficiency would depend on the amount of heat rejected at the chimney. Suppose we have a given amount of fuel and convert it into water gas. It has a certain analysis. Convert it into Siemens' gas; it has a certain other analysis. Take each of these gases and burn them thoroughly. Burn all the carbon into carbonic acid, and all the hydrogen into water; make the calculation of the amount of the resulting gases, and the amount of heat they carry into the chimney. I think a complete theoretical calculation can be made in that way to show which is the most efficient. I have no data to controvert the statements made in Mr. Taylor's paper, and I am inclined to think they are right.

In regard to melting steel in the greater temperature obtained in water gas, I do not think we are able to obtain a better result, because with the Siemens gas we can obtain the temperature of melting fire-brick. Any higher temperature than that we would have to do without. We would have to control the gas so that the temperature would be below the temperature of melting fire-brick.

Mr. Geo. Schuhmann.—Some months ago a paper was read before the Iron Masters' Association of Germany describing a water-gas plant at Essen, which I suppose is the same plant which Prof. Egleston saw in successful operation. Strange to say, the author of that paper starts out with the assertion that America is the home of water gas, owing to the anthracite egg coal being the fuel best adapted for making water gas, as it does not clinker so much as the inferior fuel with which they have to content themselves in Germany, and now in the paper read before this Society we are told that the only commercially successful water-gas plant for heating purposes is this very plant at Essen. According to that German paper the plant there has been in successful operation for over three years and, as stated by Mr. Taylor, they use puddle-furnace coke

for fuel. They use the gas for heating purposes and for lighting, but for the latter they do not rely upon its own illuminating power, but they utilize the high heat of its combustion to heat so-called magnesian ribbons to incandescence. The paper also describes a plant in which the producers are used alternately as a Siemens' gas producer and then as a water gas producer; that is, instead of burning the gas, generated during "warm blowing," in the regenerative chambers, it is led through dust catchers into a receiver, and the water gas generated during "steam blowing" or "cold blowing" is also led into the same receiver, thus mixing both gases. An analysis showed the mixture to consist of:

$$
\begin{aligned}
CO &= 30.8 \text{ vol.} \\
CO_2 &= 2.2 \text{ vol.} \\
H &= 11.2 \text{ vol.} \\
N &= 55.2 \text{ vol.}
\end{aligned}
$$

It is further claimed that the same amount of fuel which is necessary to generate one cubic meter of water gas will generate five cubic meters of the above gas mixture (Mischgas), and that one cubic meter of the latter will develop 1,400 heat units at a theoretical flame temperature of 2,200° C. When used in a gas engine five cubic meters of the mixed gases developed as much power as two cubic meters of water gas, but the five cubic meters of the former do not cost any more than one cubic meter of water gas.

The reason why they have made heating with water gas a financial success may be because they use it for a different purpose than for melting steel in an open-hearth furnace; they use it there for welding Fox's corrugated boiler flues, and I believe that the flame is more concentrated, more on the blow-pipe principle, than in an open-hearth furnace. A member of this Society has made some successful experiments, welding sheet iron and sheet steel with a water-gas blow-pipe. Another gentleman who has made many experiments with water-gas also told me that it costs considerable more per heat unit developed than Siemens' gas, and the figures he showed me come very close to the figures presented by Mr. Taylor. To my question: "Why do you use it then?" he replied that on certain goods it does much cleaner work and also increases the output of the furnace. A similar factor may also help to make the Essen plant successful.

The paper and its discussion are published in full in *Stahl und Eisen* of January, 1886.

Mr. F. W. Taylor.—I should like to say that a friend who has recently returned from Europe, a Swedish engineer, whose cousin was engaged in running this same water-gas plant at Essen, advised me particularly against saying anything very strongly in favor of it. He said his cousin had written him stating that the financial success of it was extremely doubtful, and that even if it were a financial success that the waste products used at Essen are of such rare occurrence that they could seldom be obtained in a position close enough to where the gas would be wanted to make it desirable to use them.

CCXX.

SUBSTITUTES FOR STEAM.

BY GEORGE H. BABCOCK, NEW YORK CITY.

HAVING been called upon a number of times within a few years past, and twice quite recently, to witness and report upon the working of engines using some other fluid than steam, each supposed, or at least claimed to be a great advance in economy, over steam—which claims, it is scarcely necessary to state, were wholly unsupported by the facts—I have thought it worth the while to give a brief history of such attempts, discuss their possible advantages and their evident disadvantages, and inquire what can be hoped for in that line.

The steam-engine had scarcely left the hand of Watt and demonstrated its practical usefulness, before efforts began to be made to supersede it by engines driven by some other fluid, having fancied elements of economy. Such efforts have not always been made intelligently, and up to the present time have not met with any marked success, excepting, perhaps, in air and gas engines for lighter powers. As is well known, water has the highest specific heat (with two exceptions only, bromine and hydrogen) of any known substance, and in passing from a liquid state to a vapor it renders a larger quantity of heat "latent" than any other known liquid. These two facts have seemed to point to a probability of economically supplanting water as a fluid for vapor-engines, and at first sight it seems plausible that a fluid boiling at a lower temperature, and having a lower specific heat, would possess decided advantages. Before the dynamic theory of heat had been developed, and its principles applied to heat and heat-engines, it is not strange that Sir Humphrey Davy and many other well-informed men thought that such would be the case, but it is scarcely creditable that at this late day engineers and scientific men can be found who assert the same fallacy, or that a late standard work, *Spon's Encyclopedia*, should contain such a statement as this: "The bisulphide [of carbon] is easily evaporated to a

dense vapor; the heat absorbed for evaporization being about 280° F., that of steam about 1,000° F., a saving of seventy-one per cent. in the fuel."

VAPOR-ENGINES.

The first use of other vapors than that of water for producing power may be traced to Rev. Dr. Edmund Cartwright, the ingenious inventor of power-looms. In 1797, he patented an engine (Fig. 217) in which he proposed to use "ardent spirits or ether, or any other spirit more volatile than water, either wholly or in part." He also proposed to attach this engine to a still to utilize the vapor therefrom, first for power, and afterward when condensed, for sale, or, possibly, home consumption. It is also interesting to know that this same Dr. Cartwright invented the automatic cut-off, and patented it as early as 1801. The next to

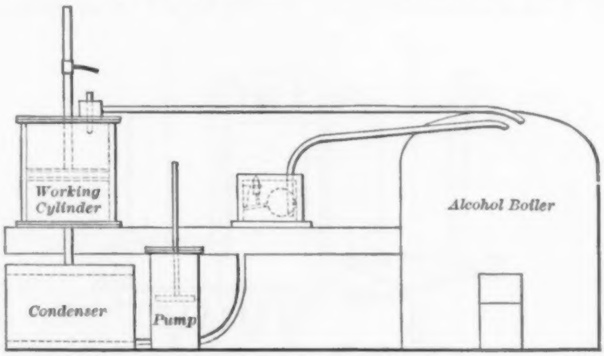


FIG. 217.

attempt the use of such vapors was Mr. Thomas Howard, of London, who patented, in 1825, and made between that and 1830 an engine (Fig. 218) in which alcohol was vaporized at each stroke, by being injected upon the surface of a highly heated fluid, as oil, the pressure being transferred by the oil to a piston in a communicating cylinder. The vapor was then condensed by "a large surface of metal surrounded or covered with flannel or some other porous substance continually absorbing water, and at the same time acted on by a stream of atmospheric air."

The following glowing statement appeared in a London magazine in 1826, and must have referred to the latest new motor of that day: "One of the greatest discoveries yet made in navigation has transpired. . . . Three-quarters of the fuel now used in navigation will be saved! . . . The vapor of quicksilver is

substituted for steam with similar machinery. . . . The saving of stowage will be very considerable, and a ton of quicksilver will be sufficient for propelling a vessel to India and back again, with an engine of 140 horse-power." I have been unable to trace this quicksilver engine beyond this paragraph, and it possibly referred to a plan of Howard's for substituting mercury for the oil in his vapor-engine.

Du Trembley, in 1842, patented, in France, an engine (Fig. 219) in which ether was substituted for water, being evaporated in a vessel placed within a common steam-boiler. Some five years later he brought out his "binary engine," which had better claims to be an advance in engineering than any previous attempt in that line, and which, in fact, was put into more extensive practice than any before or since. He used steam in an ordinary engine, condensing it on a surface in contact with liquid ether, transmitting

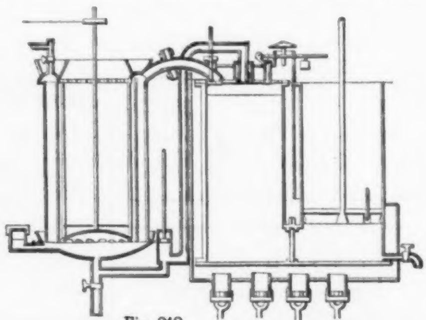


Fig. 218

its latent heat thereto. This, at the temperature of say 15 inches vacuum in the condenser, produced an elastic vapor of ether having a pressure of about 50 lbs. above atmosphere, which vapor was used to develop power in another cylinder and was itself condensed at as low a temperature as possible. This was claimed to double the power with no additional expense. A number of engines were put to work successfully, and up to 1859 no less than eight vessels were fitted with them, viz.: The *Du Trembley*, 651 H. P.; *France*, 600 H. P.; *Brazil*, 300 H. P.; *Kabyle*, 200 H. P.; *Sahel*, 200 H. P.; *Ville de Lyon*, 420 H. P.; and *Amerique*, 420 H. P.

A government commission examined into their working, and made a very favorable report, stating the consumption of coal to be 2.8 lbs. per hourly horse-power, which was much below marine steam-engines of that day. The vessels, however, came to grief,

from the dangerous character of the vapors, which no care could confine. One, the *La France*, was burned in the harbor of Bahia from the escape of vapor, others were compelled to employ Davy safety-lamps in their engine-rooms, as it was impossible to stop the leakage at the joints. The result was that the engines were removed from them all, notwithstanding the statement of the inventor that chloroform or chloride of carbon could be substituted for the inflammable vapors, and this source of danger avoided. One of Du Trembley's engines was brought to this country in 1851, and erected at the Novelty Works, in New York, for driving the shops, where it created some flutter of anticipation; but when it

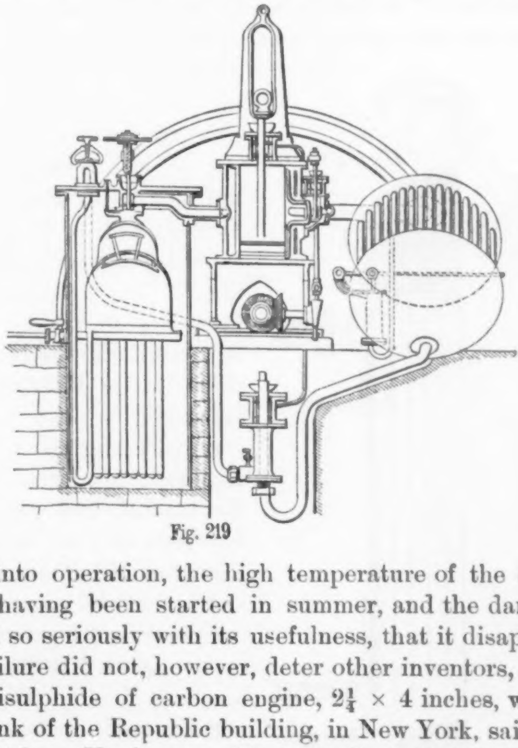


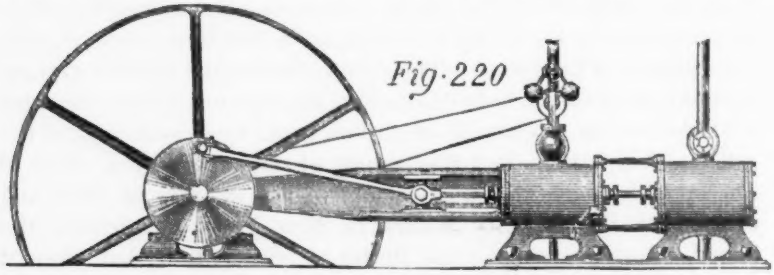
Fig. 219

was put into operation, the high temperature of the condensing water, it having been started in summer, and the danger of fire interfered so seriously with its usefulness, that it disappeared.

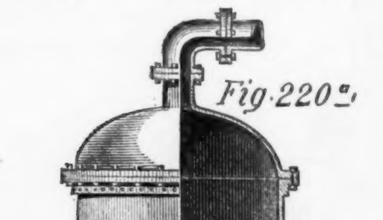
This failure did not, however, deter other inventors, for in 1855 a small bisulphide of carbon engine, $2\frac{1}{4} \times 4$ inches, was running in the Bank of the Republic building, in New York, said to be the invention of one Hughes; and in 1857, Prof. Carl F. F. Salomons had one running in Baltimore, which was tested by a commission of engineers appointed by Secretary Toucey, which reported that it was deserving a more thorough and perfect trial. Another was tried, later, in Brooklyn, but was not heard of afterward.

In 1872, there appeared a new prophet seeking for profits in

the same line, J. H. Ellis, copying Du Trembley, with slight variations, but using instead of ether, carbon bisulphide, which



he had succeeded in producing at reduced cost. He claimed as high as 166 per cent. gain of power with the same fuel. A small engine was tested showing a considerable saving, as compared with a wasteful steam-engine, and a company, styled the Ellis Vapor Engine Co., was formed, with a large nominal capital, to introduce the "invention." The Atlantic Works, East Boston, Poole & Hunt, Baltimore, and the Haskins Machine Co., of Fitchburg, Mass., embarked in the business of manufacturing the Ellis Vapor Engines (Figs. 220 and 220*a*) for the market, and great savings were reported in several cases. But after Mr. Poole had nearly lost his life by an explosion of the vapor, and much capital had been expended in the business, it quietly died a natural death, notwithstanding that there were plenty of certificates from engineers and users, testifying to its wonderful economy. Like many another untruth, however, it would not stay dead, and three or four years ago the old fallacy was resurrected by certain parties, who this time went back to the idea of Dr. Cartwright and used alcohol in their



boiler mingled with the water, though they kept the nature of the fluid a profound secret. Wonderful claims were made for its economy, which were said to be proved by tests made on a miniature engine. A great stock company, it was claimed, had been formed, and that there was the prospect of "millions in it." Capitalists, however, wanted more "facts," and as the selected engineer would not report favorably from experiments on the miniature engine, a tug was fitted up in Boston harbor for a convincing trial. Unfortunately, however, the trial had scarcely begun when the vapor caught fire, the tug burned and went to the bottom, where I suppose the stock of the company has followed it. Even this did not finish the old fallacy, however, as, since that time, two attempts have been made to form stock companies, on the prospects of great gains to be obtained from the use of bisulphide of carbon in place of water; and at the present time one with a capital, said to be $25,000,000, is seeking for investors. As a demonstration of its wonderful possibilities, they exhibit a plant in which a first-class automatic engine 12 × 30, having for a load 49 electric arc lights with $\frac{5}{16}$-inch carbons, is driven 118 revolutions per minute by the bisulphide vapor. The consumption of coal is claimed to be $\frac{3}{4}$ of a ton in ten hours. As each light probably requires less than half a horse-power, it will be seen that the saving is probably represented by a very large minus quantity.*

There is really no excuse for engineers who at this day indorse such schemes. So long ago as 1830, Mr. Ainger showed, in a paper read before the Royal Institute, that there could be no gain

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* Fig. 221 is a card taken from this engine when driving the load stated. Its scale is $\frac{1}{24}$ inch per lb., and it figures 29 horse-power. The vacuum shown, 8 inches, corresponds with 99° F. temperature, while that of the exhaust-pipe was said to be 104. The highest pressure shown is 35 lbs., corresponding to a temperature of 170° F. Its ideal efficiency would therefore be 11 per cent. The stated consumption of coal makes 5 lbs. per hourly horse-power, or an actual efficiency of 3.6 per cent. of the total heat used. A steam-engine, working under the same pressure and the same temperature of condenser, should use 2.5 lbs. of coal, or one-half the quantity used by the bisulphide engine.

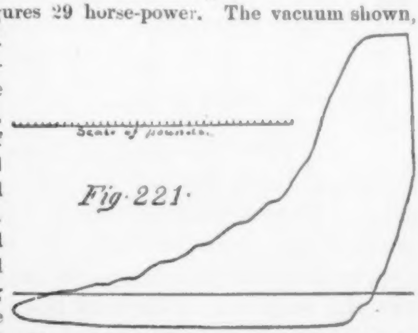


Fig. 221.

by using any other vapor in place of steam. Craddock, in his "Lectures on the Steam-Engine," in 1847, pointed out the fallacy in the claims for the Du Trembley engine. Rankine, in his "Prime Movers," in 1859, proved that "The binary engine is not more economical than steam-engines designed with due regard to economy of fuel; but," he remarks, "by the addition of another engine, a wasteful steam-engine may be converted into an economical binary engine." And Messrs. Gantt and Maury, in *Van Nostrand's Magazine*, for November, 1884, demonstrated clearly that under all ordinary conditions of working, "none of the non-aqueous vapors will ever successfully compete with steam."

CARBONIC-ACID AND AMMONIA-GAS ENGINES.

As these gases are not properly what is popularly known as "vapor," at ordinary temperatures, I have placed the attempts to utilize them for power in a group by themselves, although they are based on the same principle as the vapor-engines. No sooner had carbonic-acid gas been liquefied by Faraday, in 1823, than numerous inventors seized upon the great force stored in the liquid as a means of developing power, forgetting that it could give out no more than had been put into it, and that as there were no means of producing a cold sufficient to again condense the gas, this could only be done at the expense of a considerable back pressure on the engine, while the cost of the material quite forbade it being thrown away after once using. One of the first persons to seek to apply the new force to mechanical work was the well-known engineer, Sir M. Isambard Brunel, who, by the way, commenced his career as an engineer in this country, and was the architect of the Old Bowery Theatre. He patented an engine to be driven by liquefied gas in 1825 (Fig. 222), and for ten years he and his son spent much time, labor, and money trying to

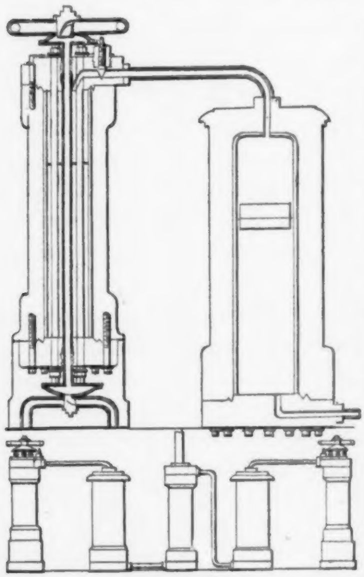


FIG. 222.

carry out the idea, but finally had to abandon it. He confined the gas in strong receivers, in which it was alternately exposed to temperatures of 120° and 60°, the difference in pressure—some 40 atmospheres—being transmitted to the piston through vessels of oil. Several other inventors followed in his wake, but none were successful.

Ammonia gas has received considerable attention, because, by its great solubility in water, it may be condensed with a less degree of cold. The liquefied gas was used about 1860 by MM. Tellier and Flandrin to propel omnibuses in the streets of Paris; the gas, after expansion, was absorbed by water and subsequently was redistilled and condensed for future use. It was claimed that in the absorption of the gas all the latent heat

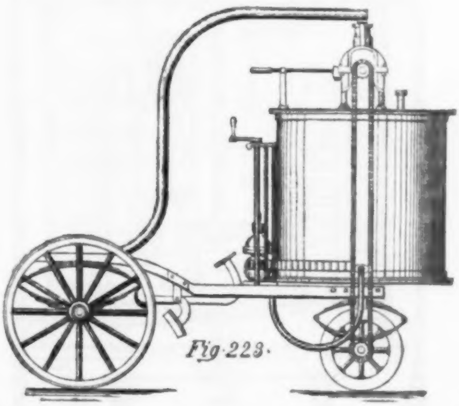


Fig. 223.

was regained, so that there was no loss. Figs. 223 and 224 show Tellier's carriage. Subsequently, street cars were propelled in New Orleans by the same means. This, however, was not properly a substitute for steam, but, rather, a convenient method of storing and regaining power. M. Frot had an ammonia engine in the Paris Exposition, 1867, in which a solution of one part of ammonia in four parts of water was used in a boiler, the mingled gas and vapor from which was used in the cylinder of an engine, and then discharged into a condenser (Fig. 225), where it met a spray of ammoniacal water, drawn from the boiler and cooled, which, because of its lower temperature, re-absorbed the ammonia, aided by external refrigeration, after which it was pumped again into the boiler. The inventor claimed that it used but one-quarter the fuel required by a steam-engine of the same power!

The mode of action of this engine was identical with that of Delaporte's of 1859 (Fig. 226). President Barnard, in his report to the U. S. Government upon the machinery at the Paris Exposition, took pains to point out and demonstrate clearly that there was no advantage in such an engine except the possible saving of waste in the furnace. This might amount to five per cent.; not more.

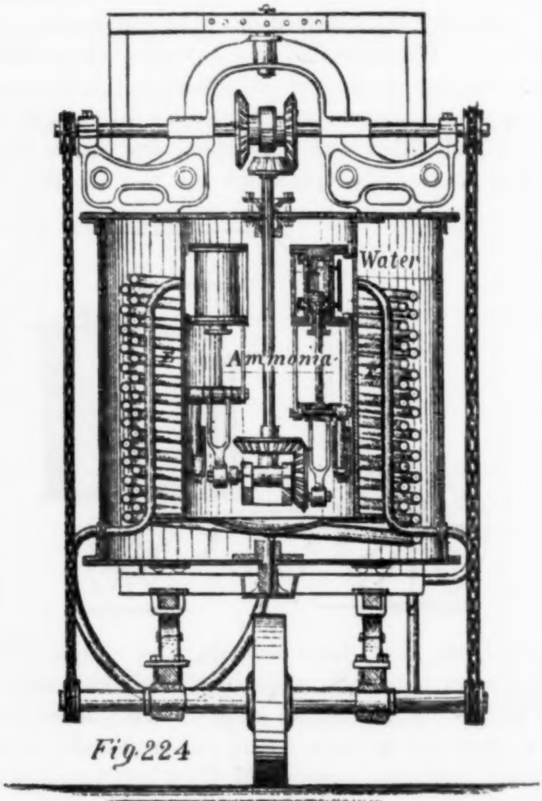
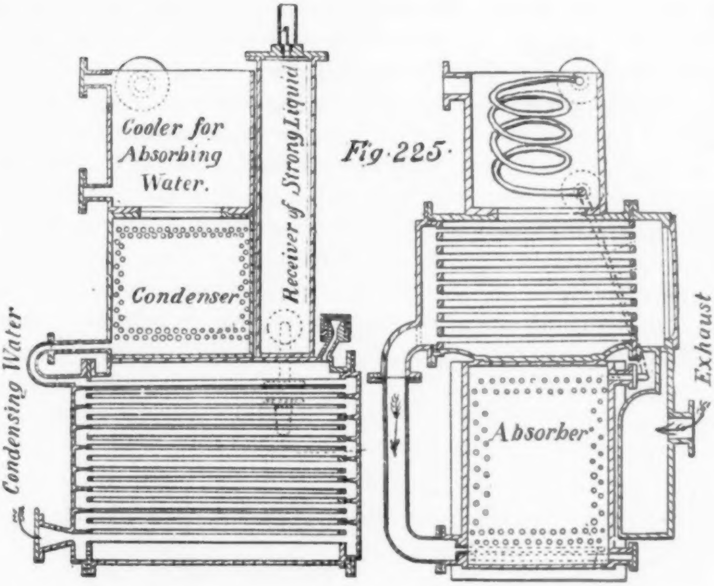
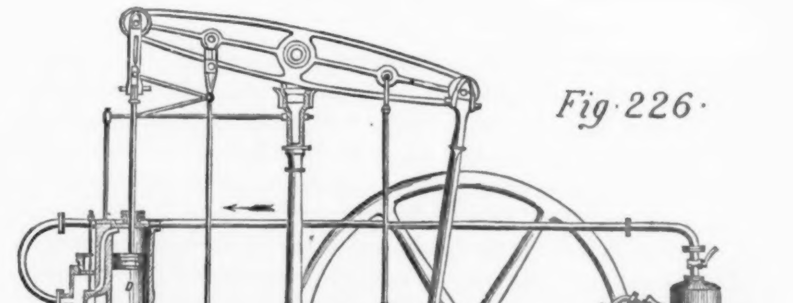


Fig. 224

A few weeks ago, I was invited to witness some experiments with a "new" motor in New York, which proved to be this same cycle revived. In connection with this latter engine, a claim is made that the latent heat of the gas is utilized by its absorption into water, and that by a jacket of cold water, absorbing a cold gas, the vapor in the cylinder is superheated. As, however, the temperature of the water and gas require to be kept down by external means, in order to secure the absorption, it is

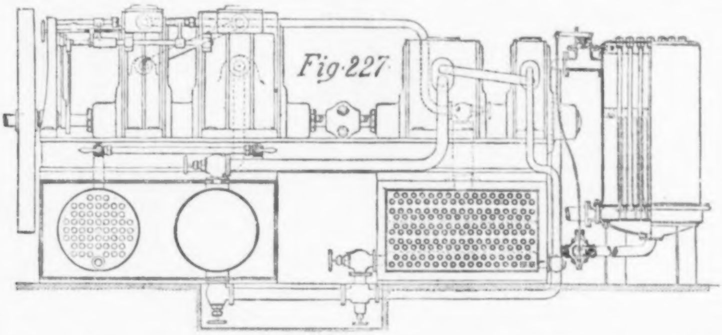


Fig·225·



Fig·226·

not apparent where the benefit, if any, comes in. In the same line, and but a little more unscientific, if possible, is the "*zero motor*" (Fig. 227), regarding which about five years ago the reading world were startled by the announcement that the days of steam-engines, and, in fact, all engines deriving power from the combustion of fuel, were doomed, and that thereafter power was to be obtained in abundance from the "heat of the environment." The new miracle-worker was invented by Professor Gamgee, at Washington, and its claims were indorsed by prominent officials of the U. S. Navy. A zero motor was said to have been at work for months in the Washington Navy Yard, using liquefied ammonia, in a "boiler" which absorbed



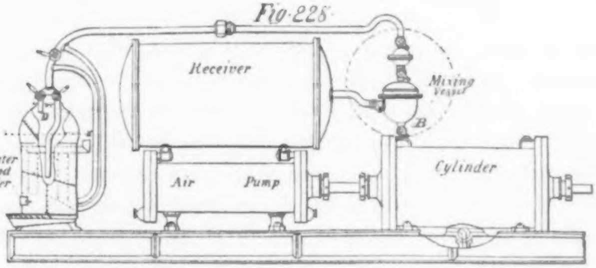
its heat from the air, the generated vapor expanding in a working cylinder, transforming its heat into work, until it was condensed into a fluid at a temperature "approaching absolute zero," after which it was pumped back into the boiler and the cycle completed. By this wonderful invention a vessel was to cross the ocean, with no expenditure of heat save what she drew from the surrounding water. It is needless to say that this promising engine disappeared very soon, doubtless congealed by its own refrigerative power, and its very mention at this date is sufficient to inaugurate a freezing coolness in quarters where it was once so warmly received.

CLOUD ENGINES.

More than thirty years ago, as many of you will remember, considerable interest was excited by the fabulous saving reported to have been made by the "Cloud Engine" of Wm. Mount Storms. This saving was certified to over the signature of Horatio Allen, who reported that by careful trials with an engine in practical

use, he found a saving of over 50 per cent., and added: "But one conclusion can be made from these trials, and that is, that by the Cloud combination a very large saving will be effected." The trial on which this conclusion rested was made with an engine 8 inches diameter of piston and 12 inches stroke. It was run for a short time with steam of 70 lbs. pressure cut off at 2 inches. The engine was then changed so that the piston drew in 1½ inches of atmospheric air before the steam-port opened, after which two inches of steam was admitted, to mingle with the air. The load was obtained by a brake; the consumption of steam was assumed to be the same, the saving being indicated by the increased number of revolutions. The idea of the inventor was that there was a production of "visicular vapor" within the cylinder (see Fig. 228). The air was admitted cold, and was expanded by heat

Fig. 228.



received from the steam, which, it was claimed, greatly increased the pressure and effectiveness. A steamer, the *Novelty*, was subsequently fitted with engines on this principle, but was not given a public trial. Some four years later, however, we hear of another small engine by the same inventor in Troy, running 4½ days on the same coal which it took to run it 2½ days, with steam alone, a saving of 45 per cent., notwithstanding which it has long since ceased to have a place, except in the record of unfulfilled hopes.

It is hardly necessary to say that Mount Storms was not the first experimenter in this line. Previous to 1830, one William Wilmot Hall, of Baltimore, patented a steam and gas engine in which the products of combustion from the chimney were to be pumped into the steam space of the boiler, to add, by their heat and expansive force, to the economy of the engine. And, in fact, the same idea had occurred to Richard Trevithick as early as 1811, as described and sketched by him in a letter of his dated January 11 of that

year (Fig. 229). He had found by experiment that his chimney delivered "four and a half times the quantity of heated air at nearly four times the temperature of heat that there is of steam produced from the same fire and delivered to the cylinder." He therefore proposed to make a tight furnace and blow the air in at such a pressure that "the whole of the air driven into the fire-place, with all the steam raised by its passage up through the water in the boiler, must go into the cylinder." He, however, did no more than make the suggestion. In 1829, Wm. Gilman published in the *Mechanics' Magazine* a plan for accomplishing the same thing, which, he says, he had in operation in 1826. In 1837, John Isaac Hawkins patented a similar engine, and in 1847 the same idea again occurred to W. E. Carrett, who also published a design for such an engine in the *Mechanics' Magazine*. Ten years after Mount Storms, Rogers & Black patented an engine in which, by means of an injector fitted in the steam-pipe, air was taken in and mingled with the steam on its way to the engine. In 1857, the steam-boat *John Faron* was fitted with an engine quite like Hall's, but claimed by F. B. Blanchard, and was run

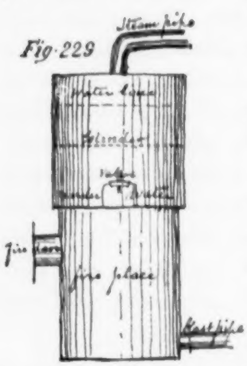


Fig. 229

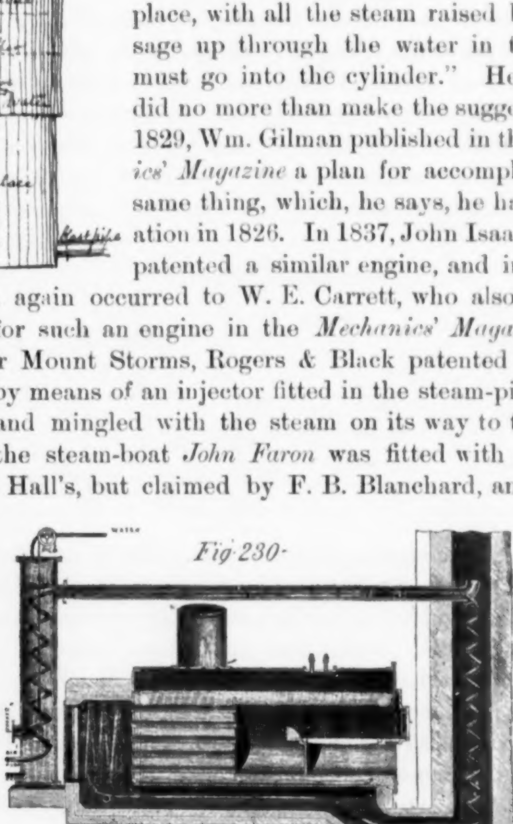


Fig. 230

upon the North River for a time. She was 155 feet long, 25 feet beam, and made 13 miles an hour on a consumption of about 400 lbs. of coal. In 1867, George Warsop, of Nottingham, England, worked out a similar idea. He, however, heated his air by the exhaust, then by the waste gases of the chimney, after which it was mingled with the steam in the boiler (Fig. 230). A paper

was read upon the wonderful promise in this invention before the British Association, claiming a saving of 47 per cent. One of these engines was in operation at the International Exhibition of 1871, and a locomotive was running on the Lancashire and Yorkshire Railway from 1868 to 1873, which used 16 per cent. less coal per mile run than the average of five other engines, but, unfortunately for accurate comparison, no record was kept of the loads drawn by each.

An exhaustive discussion of the results to be attained under various conditions in these engines—or, rather, in the first and best of the series, Trevithick's or Hall's, in which the products of combustion were pumped into the boiler—was made by J. A. Henderson as a graduating thesis at Stevens Institute, and published by Prof. Thurston in his report on "Mechanical Engineering at the Vienna Exposition in 1873." This discussion showed that the whole possible gain must arise from the saving of the usual losses in chimney gases, the capability of carrying a higher temperature with the same pressure, the saving in condensation in the cylinder due to this high temperature and to the mixture of air and steam. Prof. Reynolds, F.R.S., found by experiment that from 5 to 10 per cent. of air in the steam effectually prevented condensation. These possible savings, however, would not give, after making reasonable allowance for imperfection in the machine, as great a useful effect per pound of coal as has been attained practically with steam alone in the best modern engines. Where a condenser is inadmissible, however, a small gain might be hoped for.

AIR-ENGINES.

In this term are included all engines using atmospheric air for the fluid medium, through which heat can be transformed into work, whether the air be heated by external means or by internal combustion of solid, liquid, or gaseous fuel. It would be well if the term "gas-engine" could be confined to those engines using other gases than atmospheric air as their actuating fluid, such as carbonic acid, or ammonia, or nitrous oxide; but it is doubtless too late to establish such a nomenclature at present. If we call a machine a "gas-engine," simply because it uses gas for fuel, what should we, to be consistent, call motors using coal or wood?

Passing those projects for utilizing the explosive force of gunpowder, which preceded the steam-engine, as mere suggestions not

put in practice, and the air-engine by which the priests of On are said to have caused the statue of Memnon to speak at the rising of the sun, supposed to be the first instance in which heated air was used for motive power, we come to the patent of John Barber, in 1791, in which he mixed "gas generated from wood, coal, oil, or other combustible matter with atmospheric air in an exploder," to be "fired by the application of a match or candle." Water was to be injected "to prevent the melting of the inward pipes and mouth of the exploder by the issuing flame." The issuing stream of inflamed gas acted upon an "engine" which was to "be applied to grinding, rolling, forging, spinning, and every other mechanical operation . . . or passed out the stern of any ship . . . or other vessel . . . against the water . . . driving the vessel with its contents in any direction." This latter idea, however, was taken from the previous

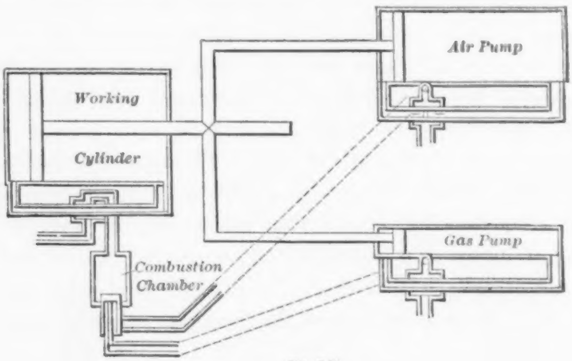


Fig.231

patent, 1790, of James Rumsay, an ingenious American, who originated many inventions claimed by others years afterward.

It is somewhat amusing to see how closely this Barber's patent describes the latest "great invention" which is to revolutionize navigation; the only difference being that in the modern one the gas is generated from petroleum, is mixed with air at a considerable higher pressure than the atmosphere, and is ignited by electricity. By virtue of the latter fact, the "invention" has been described in some of the papers as the application of electricity to marine propulsion!

The engine of Mr. Robert Street, which appeared in 1794, was actuated by the explosion beneath a piston of a mixture of common air and the vapor of turpentine, the latter being generated

within the cylinder by injecting a small quantity of the liquid upon red-hot iron. Mr. Street also employed, at this early day, the method of igniting the charge which is most extensively used at the present time, by means of an external light, the flame from which was conveyed to the gases within the cylinder at proper intervals.

M. Lébon, in 1799 and 1801, patented in France a gas-engine, in many of its details anticipating later inventions up to the time of Otto (Fig. 231). As illuminating gas was not then in public use, he manufactured it by a process quite like those employed by the great city gas companies before the advent of water-gas. A proper portion of this gas he pumped into a chamber mixed therein with a proper proportion of atmospheric air, supplied by another pump, and ignited therein by the electric spark. These products of combustion were then expanded in a cylinder, driving the engine. This engine also had self-regulating devices, and had it not been fifty years ahead of its time, might have made a mark in the world. De Rivaz, in 1807, patented, in France, an engine which, it is said, he applied to a small locomotive. This engine is remarkable from being the first to use a flying piston, elevated by an explosion, and doing work in its descent, a plan which, re-invented by Otto and Langen two-thirds of a century later, inaugurated a new era in gas-engines. It is also notable that this locomotive, driven by gas, was only three years later than the first successful attempt at steam locomotion by Oliver Evans, and only two years later than Trevithick's first steam-carriage. Rivaz's cylinder was about 5 inches in diameter (Fig. 232), and was fitted with a piston carrying a rod and rack, or chain. At the bottom of this cylinder was a smaller one, fitted with another piston, which was used to draw in the charge of air and mix it with one-half its bulk of hydrogen, from an inflated bag, through a three-way cock. The inventor proposed four methods of ignition, namely, by a portion of the cylinder kept red-hot, by the sudden compression of oxygen, by the use of phosphuretted hydrogen, and by electricity. He used the latter in practice.

In 1820, a Mr. Cecil gave a detailed account in the *Transactions* of the Cambridge Philosophical Society of an engine invented by him, in which he used a mixture of hydrogen and oxygen in the proportions to form water, the explosion of which would actuate a piston by the expansion of the gases, and afterward, by the condensation of the vapor into water, a vacuum

was produced, by which the return stroke was performed under the pressure of the atmosphere. The cylinder was worked with a pressure of about 12 atmospheres, and the noise and shock of the explosion were so great that the inventor proposed putting the cylinder down a well to deaden the sound! A later inventor. James Johnson, patented the same idea in 1841, and has generally been credited with being the first to suggest the use of these two gases for this purpose.

The first attempt we can hear of in this country for applying

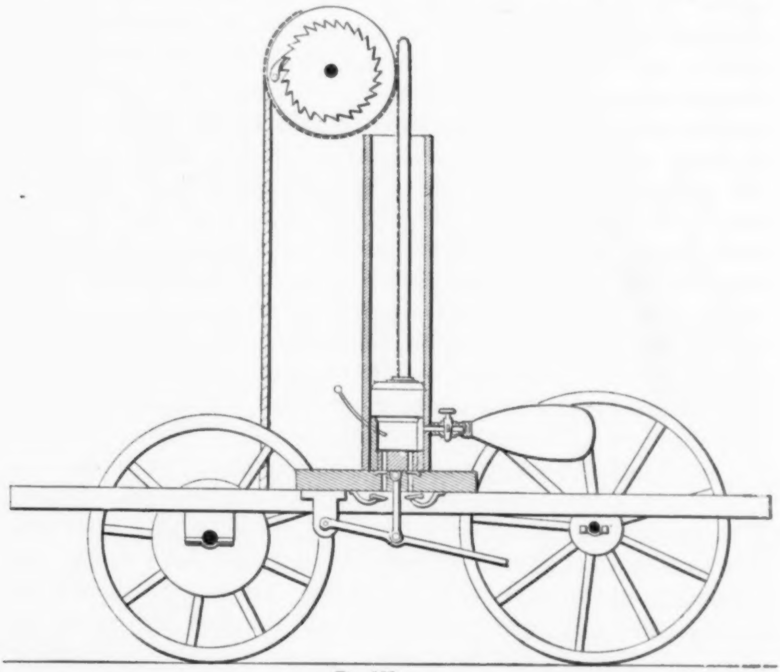
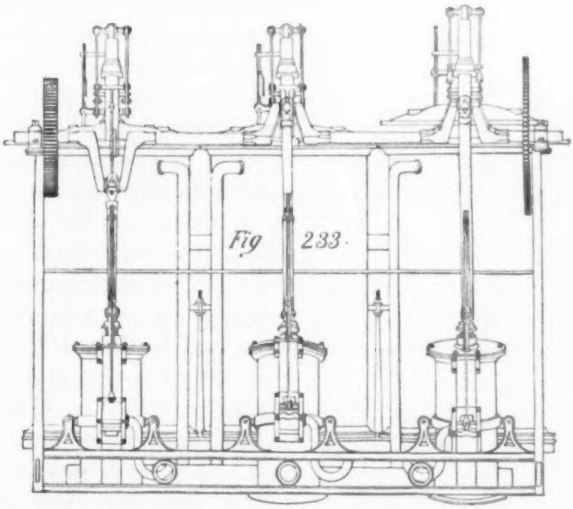


Fig. 232

the explosive effect of a mixture of gas and air as a motive power was by an unknown inventor, who showed, in 1814, at Philadelphia, a model of his apparatus to Thomas P. Jones, afterward editor of the *Franklin Institute Journal*. He proposed to make his gas by the distillation of wood, claiming that the resulting charcoal would pay the expenses. In 1826, Samuel Morey had an engine at work in New York or Philadelphia by means of a vacuum produced by the explosion of a mixture of atmospheric air and vapor from proof spirits mixed with a small portion of

spirits of turpentine—the "camphene" of thirty years ago, the explosive character of which is doubtless familiar to many present. Two cylinders fitted with pistons were alternately filled with the mixture, which, when lighted by a taper, drove most of the contents out through a valve in the bottom, the vacuum thus created drawing down the pistons and setting the machine in motion. Great expectations were entertained of this engine. The *Franklin Institute Journal* and *Silliman's Journal* predicted that "if no unforeseen difficulties present themselves in its operation on a large scale, this will be the greatest improvement in many years, . . . as the weight of the materials to keep it in



motion for a considerable length of time will be so small as not to be worth mentioning."

Meanwhile, Mr. Samuel Brown, of London (1823–5), had patented in this country and England the same idea (Fig. 233), which at first he used for raising water to actuate a water-wheel, but afterward made a piston-engine. The *Mechanics' Magazine* says of this engine: "Mr. Brown has tried gas for the production of a vacuum, and by employing the *extinction of a peaceable flame* instead of a *violent explosion*, he has succeeded—has furnished his country and the world with a power which, judging from the *first* machine (and what would have been the judgment from the *first* steam-engine?), will be convenient in its operation and cheap and safe to use beyond all precedents. . . . The engine can be

adopted in any case by sea or by land where a power is required, while even in its present infantile state, the expense either of the engine itself or the working of it bears no proportion to that of the steam-engine. . . . One cubic foot of gas raises 300 gallons of water "—but the height was not stated. A company called the "Canal Gas-Engine Company" was formed for testing it, offered a prize for the best plans, and built a boat to be propelled by gas. After spending £5,000 for the patents and experiments, their boat, according to an official report, "started from Blackfriar's Bridge, went at the rate of from seven to eight miles per hour, with all the regularity of steam-boats . . . and it appeared that the power could be sustained for any length of time by gas as well as by steam." But the chairman having reported that "the expense of procuring gas would entirely supersede its application as a prime mover instead of steam," the company dissolved in 1827. In 1832, however, one of Brown's engines was at work on the Croyden Canal for raising water from a lower level to a higher. It had a cylinder 42 inches in diameter and 22 feet high. Air and gas were admitted into this cylinder and exploded, lifting a cap and expelling the greater portion of the air. Water was then injected to cool what remained, and the partial vacuum raised the water into the cylinder, from which it was discharged at the higher level. Estimates were published of the cost of working this engine, which stated that besides the work done, the running of the engine yielded a net profit of £102.18 per annum, from the sale of the coke and tar, which were by-products of the manufacture of the gas. It is needless to say that even such a profit did not extend its use very largely, or long keep the Croyden engine at work.

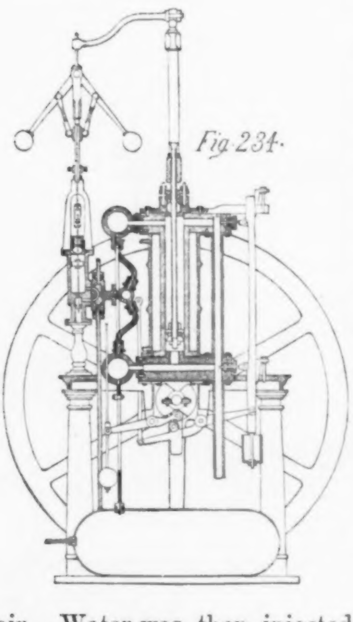


Fig. 234.

A very great advance in gas-engines was made in 1833 by Samuel W. Wright, in whose engine was first used a charge of

compressed explosive mixture, fired within a chamber behind the piston, when the crank was on the center, and acting by direct expansion within a cylinder kept cool by a water-jacket. This engine (Fig. 234) was somewhat complicated, but had almost all the elements which secure success in modern gas-engines. The gas was compressed into one reservoir, and the air into another by pumps actuated by the engine when running. A proper charge of each was measured out at each stroke, and transferred by valves to a globular vessel connecting with the end of the cylinder, and in which the mixture was exploded. The quantity of air and gas for each stroke was determined by a governor, varying the size of the measuring vessels. It will be seen that with a proper pressure in his reservoirs, and closing the exhaust at a proper point, an effect precisely similar to that of the Otto engine might be produced.

The patent of Stuart Perry, of New York, in 1846, shows sev

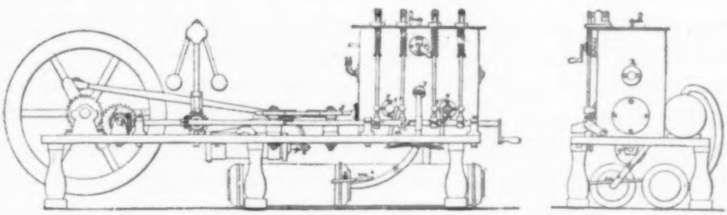


FIG. 235.

eral novel devices, among them incandescent platina lighters, which were uncovered at proper intervals by valves. He also introduced an auxiliary receiver, for storing some of the surplus power exerted by the engine when running, for the purpose of starting it again after stopping (Fig. 235). The large engine of Dr. Drake, exhibited in the Fair of the American Institute in 1856, and those of Lenoir and Hugon, rival exhibitors at the Paris Exposition in 1867, were little different from those which preceded them, save that Drake used for his igniter a surface of cast-iron, kept incandescent by an external blow-pipe flame, and uncovered by the travel of the piston. Lenoir was the first to make a business in gas-engines. A large number of his were sold and put into use.

The decision of the President of the "Canal Gas-Engine Company," in 1827, was applicable with equal force to all gas-engines for forty years at least, or up to the date of the atmospheric engine

of Otto & Langen, which first appeared in 1867. In this, as in that of Rivaz, the explosion lifted a weight, which in falling exerted its stored-up power. Previously, no such engine consumed less than 90 cubic feet of gas per hourly horse-power, and usually they consumed considerably more. The small Otto & Langen engine exhibited at Paris in that year ran for 38 cubic feet, including the igniting burner, an immense saving over former engines, but even this would require gas to be but 40 cents per 1,000 feet in order to equal the cost, at $5 per ton, of the coal used by a steam-engine working at six pounds per hourly horse-power.

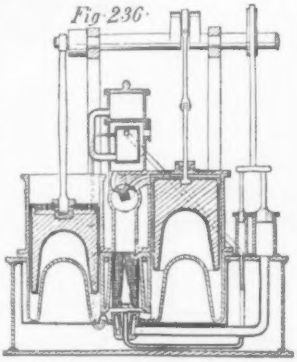
The history of the gas-burning air-engine since the later invention of Otto, in which a compressed charge is ignited just as the crank is passing its center, thus not only greatly increasing the power, but reducing the shock and causing the engine to run as quietly as a steam-engine, is well known to every one present. Many thousands of them have been sold and are everywhere at work doing good service, and to a large extent displacing small steam-engines wherever gas can be readily obtained. Though the cost of the gas consumed is greater than the cost of coal for a steam-engine of equal power, the greater convenience, the reduced cost of attendance, and the fact that the expense ceases the moment the engine stops, and does not begin again until it is started, are sufficient in most cases to overcome the extra cost of the fuel.

In 1862, M. Beau de Rochas published a paper in Paris, entitled, *Studies on the Practical Conditions of the Greatest Utilization of Heat, and of Motive Force in General*, in which he proposed a novel and ingenious cycle of operations—in brief, the making of the steam-engine an auxiliary to the gas-engine for the purpose of utilizing the waste of the latter. This paper has become somewhat famous recently from the fact that it describes minutely the four-stroke cycle, which has become known as the "Otto cycle." De Rochas proposed to burn his fuel in a gas producer, turning it into carbon-monoxide, which gas, after passing through a steam-boiler, to utilize the heat while reducing its temperature, was to be mixed with a proper quantity of air, and used in a gas-engine, then exhausted through a separate series of tubes in the same steam-boiler, the feed-water of which he proposes to pump through the water-jacket of the gas-engine. The steam generated in the boiler was to be used for driving an auxiliary engine, which not only added its power to that of the gas-engine,

but served to put it in motion at the start. If we assume the distribution of the heat in this gas-engine be the same in the Otto engine, it is easily shown that the evaporation from the boiler would be about 25 per cent. of that from the same amount of coal burned in the usual manner, and if the efficiency of the steam-engine was $\frac{1}{10}$, this would amount to 2.5 per cent. of the total heat. The 17 per cent. efficiency of the gas-engine would be reckoned, however, upon the heat developed by the combustion of the carbon monoxide only, equal, say, to two-thirds of the thermal value of the coal. This would give $17 \times .66 + 2.5 = 13.87$ per cent. as the total efficiency, which is not enough better than a good steam-engine to warrant the additional plant, ingenious as is the suggestion.

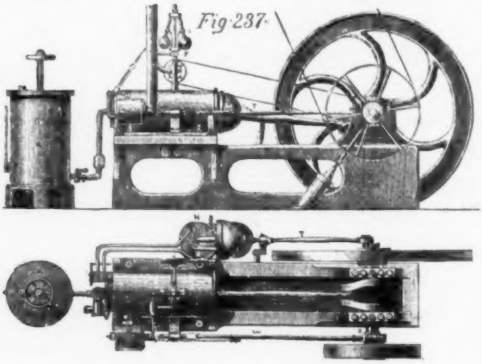
Analogous to the gas-burning air-engine are those which burn liquid fuel within the cylinder. This, in fact, was the idea of Robert Street, in 1794, but was not available until the supply of petroleum became plentiful within the last thirty years. The first practical engine of this kind was made in 1860 by Stephen Wilcox, a member of this society (Fig. 236). He employed the waste heat of the exhaust to evaporate petroleum, the vapor from which was forced in proper quantities, together with air for its combustion, by pumps into the engine, where it was ignited and burned as it issued instead of exploding. A charge of air was taken into the engine at each stroke, and partially heated by the exhaust before reaching the burner. This was doubtless the most economical engine relative to the heat used yet made of its class; but the high price of petroleum at that early day, difficulty with the residuum in the evaporating vessel, and the different temperatures required at different stages of the vaporization, caused its disuse. Julius Hock exhibited an engine at London in 1874 in which a spray of petroleum was injected into the cylinder at each stroke, the mixture of the vapor with air being ignited by a flame forced in through a self-acting valve (Fig. 237). This engine was said to have superseded steam-power at the Imperial Printing Works in Vienna. In 1874, Geo. B. Brayton patented his well-known petroleum engine, in which the fuel is sprayed into a cur-

Fig. 236.



rent of condensed air on its entrance into the cylinder, wherein it burns with a steady flame, increasing the volume but not the pressure; after which the products of combustion are permitted to expand like the steam in a steam-cylinder. Many of these engines

Fig 237



have been built, with varying success. The best economy claimed for them has been four horse-power for one hour for each gallon of oil, and in some cases they have given not more than one horse-power per gallon. At the former rate, compared to coal at $5 per ton, petroleum, at as many cents per gallon as the steam-engine uses pounds of coal per hourly horse-power, would be equally as cheap a fuel. This engine could therefore compete only with the smaller and more wasteful steam-engines. If, however, petroleum could be substituted for gas in the modern gas-engines, with equal efficiency, it would give ten horse-power for each gallon used per hour, and might then compete with steam on equal footing and at market rates.

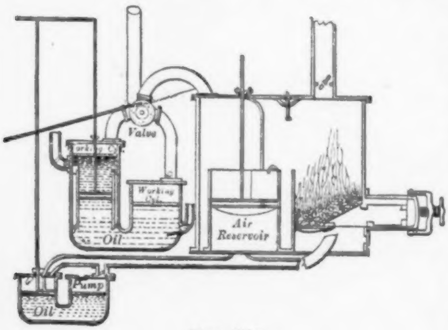
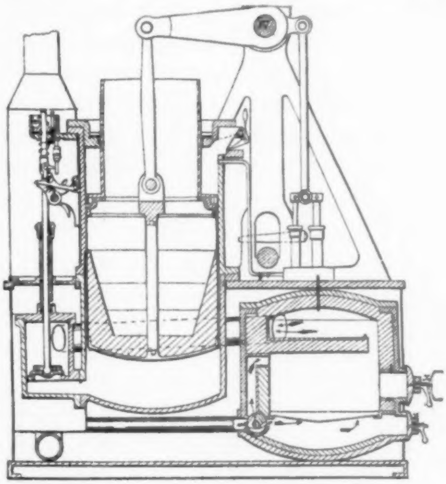


FIG. 238.

A product-of-combustion engine, in which coal was used for fuel, was clearly described by Sir George Cayley in a communication to *Nicholson's Journal* in 1807. Though the construction there shown was impracticable (in fact, he stated that the one he made was so imperfect that it would not work), he did not abandon

the plan of such an engine, but as late as 1825 and again in 1837, he patented modifications of the same idea, several of which he built, but with no marked success. Subsequently to Cayley's suggestion, Dr. Neill Arnott, the celebrated scientist, took up the same idea, and patented, in 1821, an air-engine in which, by ingenious devices, air was pumped through a closed fire, and the products of combustion were used to actuate a piston working in one leg of a syphon containing oil, a peculiar valve directing the hot gases first to one leg and then the other (Fig. 238). The piston upon which these gases worked was, in fact, the surface of the oil, so that it was not affected by the ashes and heat which upset the later built engines of Sir George Cayley. Numerous inventors have followed in the footsteps of these noted men, among them Stephen Wilcox, S. H. Roper, and Philander Shaw, each of whom built and sold a number of engines which were put into practical use. One little engine, built by Mr. Wilcox, in Providence, in 1865 (Fig. 239), with a 6-inch cylinder ran itself 360 revolutions per minute, for several days driving a 12-inch fan blower at the rate of 3,000 revolutions upon a consumption of one pound of coal per hour. The actual horse-power was not measured. Shaw's engine, exhibited at the Paris Exposition, 1867 (Fig. 240), ran an average of over 20 horse-power upon a consumption of 1.4 lbs. coal per hourly horse-power—a better result than had ever been attained by a steam-engine. The difficulty, however, in providing against the abrading action of the ashes,



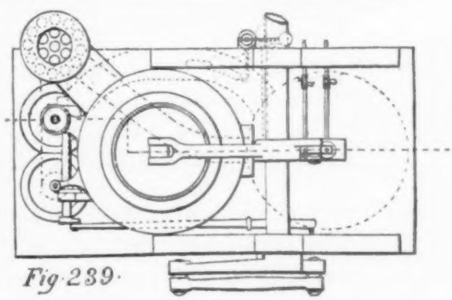


Fig. 239.

and the overheating of the valves in such engines, have prevented their coming into extensive use, notwithstanding their economy.

This class of engines can boast of a stupendous failure, next in magnitude to those of Ericsson's ships. In 1856, the Novelty Iron Works, in New York, built a large locomotive called the *Vampire*, weighing 44 tons and costing $40,000, for parties in St. Louis. It was intended to be propelled by the products of combustion, mingled with steam from water injected into the fire, and also steam made by a water-jacket surrounding the fire. The hot mixture was first conveyed around the cylinder, then into it, and after being exhausted, gave up its heat to the incoming air. This locomotive, the invention of which was attributed to one P. Bennet, was tried on the Erie Railway, near Paterson, and succeeded

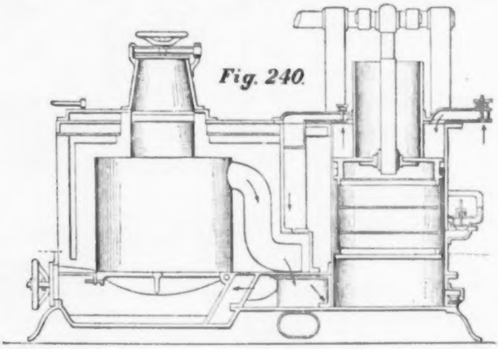


Fig. 240.

in running itself into a ditch, after making a mile and a half at the rate of twelve miles an hour. The cylinders, cut by the ashes and heat, were rebored, and the engine refitted once or twice, only to end in an ignoble failure and the junk shop.

Still another and altogether distinct form of air-engine was originated by Rev. Robert Stirling in 1816. His first successful engine was built in 1827, and one afterward ran a foundry in Dundee for three years, developing 20 horse-power by the brake, on a consumption of 50 lbs. coal per hour, an account of which was presented before the Institute of Civil Engineers in 1851. In this engine (Fig. 241), the same volume of air was alternately heated and cooled, producing a variation of pressure which actuated a working piston. The heating and cooling were affected by changing the air by means of a plunger, from end to end of a cylinder, one end of which was kept hot by a fire and the other cool by water. On its way from end to end the air passed through

a passage partly filled, near the hot end, by thin plates of metal, and near the cold end by small tubes filled with water. The thin plates alternately absorbed the heat from the air, and gave it back on the return, so that the refrigerator only had to carry off what heat could not be thus extracted. This was the first application of the "regenerator" or "economizer," a very important element in the problem of securing extreme economy. This engine failed through the giving out of the heaters, which required to be kept red hot, a difficulty which has attended all efforts to heat air

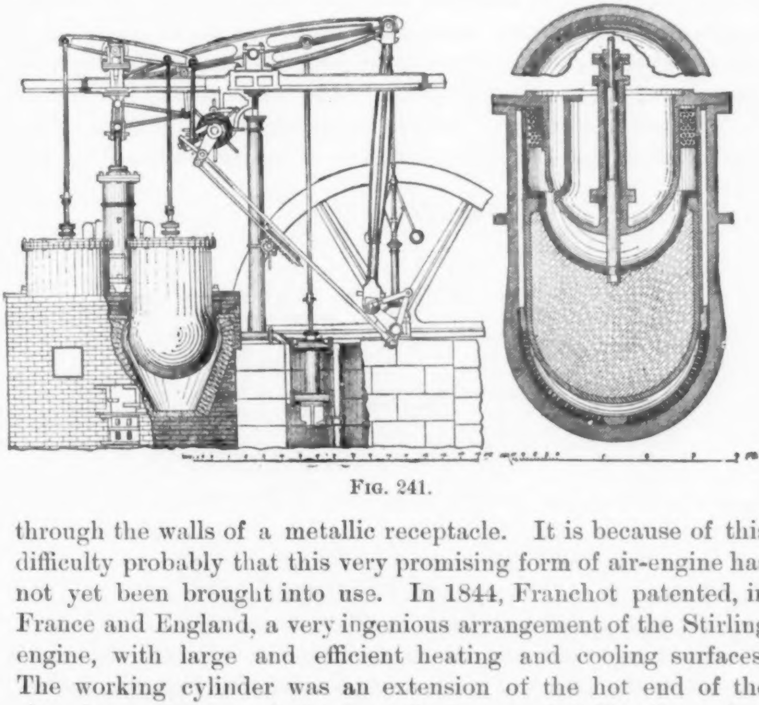


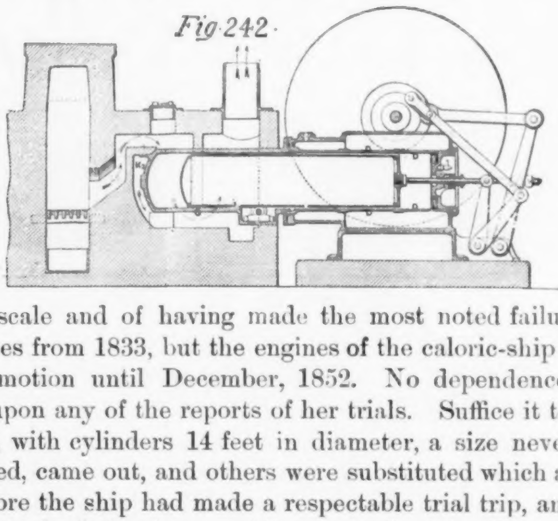
FIG. 241.

through the walls of a metallic receptacle. It is because of this difficulty probably that this very promising form of air-engine has not yet been brought into use. In 1844, Franchot patented, in France and England, a very ingenious arrangement of the Stirling engine, with large and efficient heating and cooling surfaces. The working cylinder was an extension of the hot end of the changing cylinder, and the heated air expanded directly against the working piston without being first cooled, as in Stirling's engine. The spaces over the two working pistons, which were connected to opposite ends of a beam, were inclosed and connected by a pipe, thus transferring pressure, relieving the beam, and preventing leakage around the piston. Rankine thought so well of the Stirling engine that he, in conjunction with Napier, attempted to improve its weak point, the heating surface; and Prof. Jenkin also spent much time and money in endeavoring to

perfect it, mainly in combining its advantages with those of the Cayley type. Lauberau, in France, made a number of small engines upon the same principle—one of which (Fig. 242), working to $\frac{4}{5}$ of one horse-power, tested at the *Conservatoire des Arts et Metiers*, in 1863, used 10 lbs. coal per hourly horse-power, which is doubtless as well as would be done by any steam-engine of equal power. Engines upon the same principle are now being built in Boston, and are said to work very well.

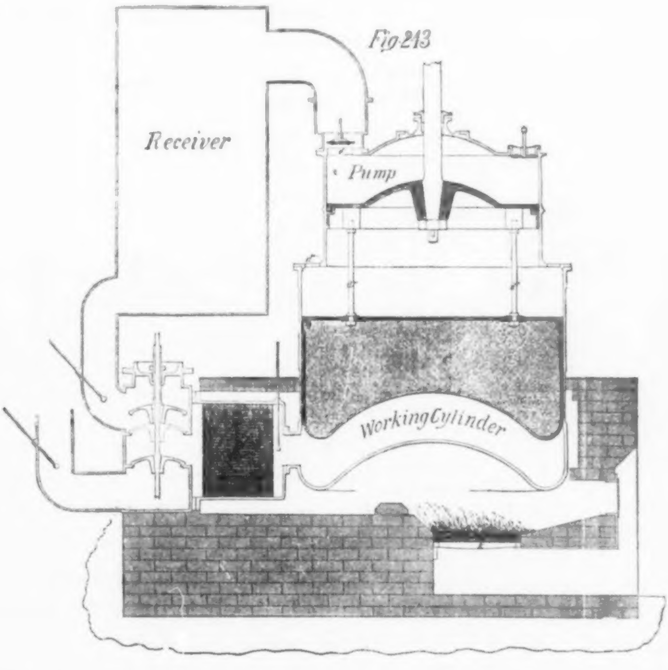
The engines of Captain Ericsson are somewhat different still, drawing their supply from the atmosphere at each stroke, heating it, allowing it to expand while doing work, and then exhausting it again into the external air. On the large engines of the air-ship *Ericsson* an economizer was employed to retain part of the heat of the exhaust and return it to the incoming air (Fig. 243). This class of air-engine enjoys the notoriety of having been built on the

Fig. 242.



largest scale and of having made the most noted failure. The idea dates from 1833, but the engines of the caloric-ship were not put in motion until December, 1852. No dependence can be placed upon any of the reports of her trials. Suffice it to say the engines, with cylinders 14 feet in diameter, a size never before attempted, came out, and others were substituted which also came out, before the ship had made a respectable trial trip, and steam-engines took their place. Afterward, Captain Ericsson made another attempt to drive a vessel by an air-engine. The *Primera* was built and fitted with horizontal engines drawing their supply from and exhausting into an artificial atmosphere of high pressure. But, as in the former attempt, the heating surface was inadequate and the available pressure was too small to give much power, so that again steam was substituted after a short trial. Many small engines of Ericsson's design, but quite different in details from the air-ship, and without economizers, were sold and

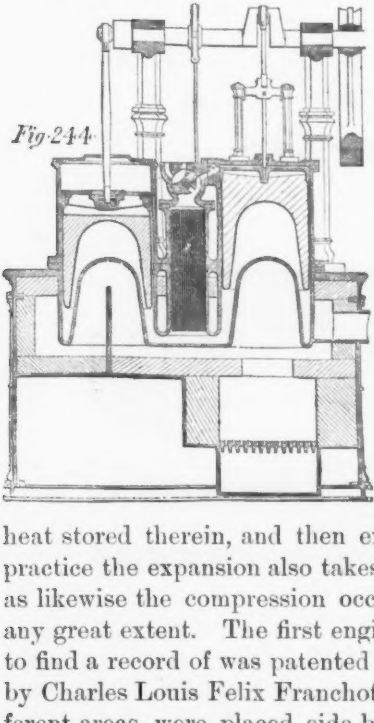
put to work, some of which are still running, giving good satisfaction. Few of them exceeded five horse-power, and they used a quantity of fuel not less than steam-engines of similar size, but, requiring no water or licensed engineer, they were preferred in many cases. One working to two horse-power was tested at the Paris Exposition of 1867, and found to use 10 lbs. coal per hourly horse-power, though one is reported as running in a printing-office in New York for 18 years burning but 4½ lbs. The Wilcox engines, of which a large number were made, and which received



a medal at the London Exhibition, 1862, were of much the same character (Fig. 244), but had a peculiar supply cylinder which took in the air with little resistance, and changed it to the hot end by passing it through an economizer. Some of these ran as low as 3 lbs. and under of coal per hourly horse-power when exerting as little as five horse-power. All this class of engines had to meet the same difficulty noted under the former, or Stirling type, of the burning out of the heating surface, and few of them are now to be seen.

The "compression engine" is a distinct class from those which have been hereinbefore described. In these a given quantity of air is constantly changed in volume, being compressed while cold and expanded while hot. There are usually two cylinders; one cold and kept cold by a water-jacket or other means, and the other hot and kept heated by external means. The piston in the hot cylinder is generally timed from one-sixth to one-quarter of a revolution in advance of that in the cold cylinder, whereby the air is first changed into the cold cylinder, sometimes through an "economizer"—then compressed therein, then changed to the hot cylinder back through the economizer, taking up again the

Fig·244·



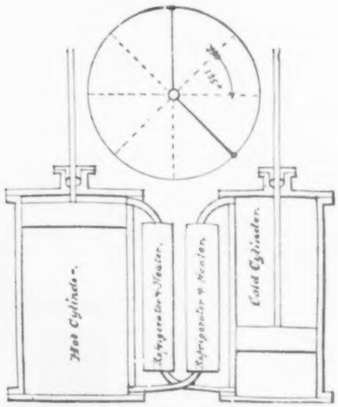


FIG. 245.

heat stored therein, and then expanded in the hot cylinder. In practice the expansion also takes place partly in the cold cylinder, as likewise the compression occurs partly in the hot, but not to any great extent. The first engine of this kind I have been able to find a record of was patented in France and England in 1853 by Charles Louis Felix Franchot. Hot and cold cylinders, of different areas, were placed side by side with pistons therein connected to cranks 135° apart (Fig. 245). The bottom of the cold cylinder was connected to the top of the hot cylinder and *vice versâ*, through heating and cooling chambers in which Stirling regenerators were to be placed, though they are not shown, nor is any means shown for heating or cooling the air. The arrangement of the pistons and cranks was such that the air is compressed

in the cool cylinder, passed through the regenerator into the hot cylinder, where it is expanded, then is transferred to the cold cylinder through the cooling chamber, and the cycle repeated. From four to six cylinders, each double-acting, were proposed to be combined in a series. A model of one of these engines was exhibited at the Paris Exhibition of 1855, and attracted much ttention from scientists. In this, cranks 90° apart were connected to pistons in two cylinders, each double-acting, their adjacent ends communicating freely through Stirling regenerators. All this engine lacked of "perfection" (according to Carnot's cycle) was a means for keeping the two cylinders at constant temperatures, and supplying the necessary heat without loss and at proper times. This same principle was embodied in an engine patented by Sir William Siemens in 1860. He employed four cylinders (Fig. 246), each hot at one end and cold at the other, all connected to one shaft through a wabbling disc, at equal quarters of the revolution, and so arranged that the hot end of one communicated through an economizer with the cold end of the next in order. The heat was to be supplied by hot products of combustion from burning producer gas in a chamber connected with the hot ends of each cylinder, while the opposite ends were supplied with refrigerators. The noted inventor neglected, however, to put the machine into practical use, his time being occupied with his metallurgical investigations and inventions. As he subsequently experimented with a different and inferior class of air-engines, he

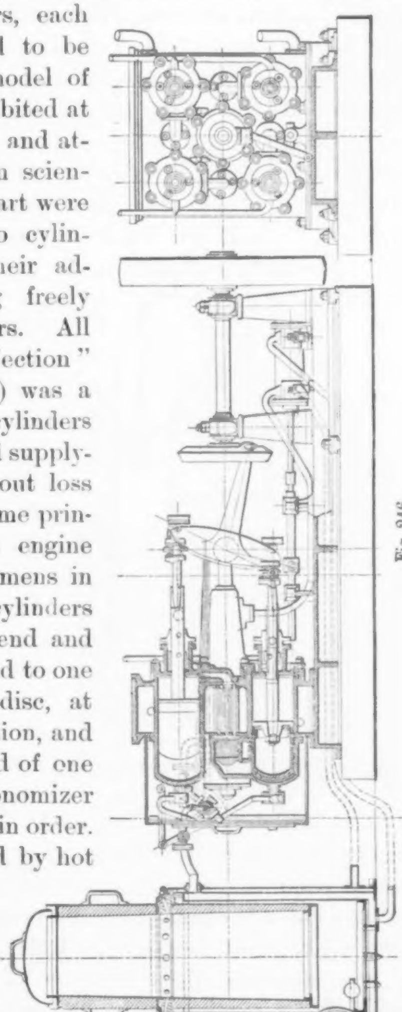
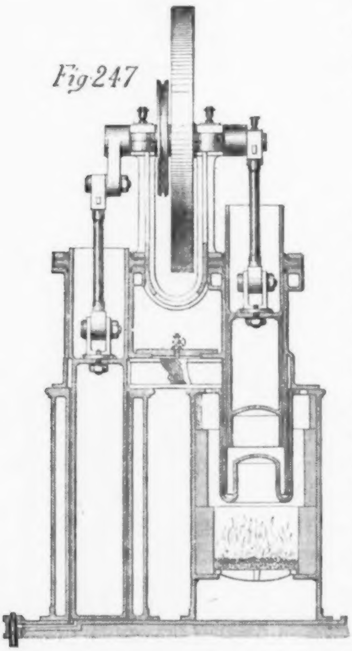


Fig. 246

evidently encountered difficulties in this one not easily overcome. This class of air-engines has, however, been made very efficient for small powers by A. K. Rider, of New York, whose small pumping-engines (Fig. 247) are well known to most of the members of this society. No other form of air-engine offers so many advantages, but it has also its peculiar difficulties. If the latter can be overcome, it is likely to become the air-engine of the future. An effort to this end is now being made, with fair prospect of success.

Fig 247



THEORY.

We will now consider briefly the principles which underlie thermo-dynamics, and their application to the inquiry. In any heat-engine it is essential that there should be, 1st, a working fluid; 2d, a source of heat; and, 3d, a receptacle for unexpended heat, both of which latter must be external to the working fluid. In its operation there must be a reception of heat by the working fluid, at a certain temperature, a conversion of heat into work, and a discharge of unconverted heat at a lower temperature than that at which it was received. The difference between such higher and lower temperatures is called the "range of temperatures," and the engine is called a "perfect engine" when the whole heat corresponding to its range of temperature is converted into work. Sadi Carnot, in 1824, seems to have been the first to enunciate the principle, now universally recognized, that the ratio of the maximum mechanical effect in a perfect heat-engine to the total heat expended upon it, is a function solely of the two constant temperatures, at which respectively heat is received and rejected, and is independent of the nature of the intermediate agent or working fluid, though at that day the dynamic theory of heat was not known, and Carnot supposed that all the heat re-

ceived in the boiler, or its equivalent, was transferred to the condenser. Subsequent researches of Joule, Rankine, and others, have established the following propositions:

1st. *In any heat engine the maximum useful effect* (expressed in foot pounds or in percentage) *bears the same relation to the total heat expended* (expressed in foot pounds or as unity) *that the range of temperature bears to the absolute temperature at which heat is received.*

2d. *In any heat engine the minimum loss of heat bears the same relation to the total heat expended as the temperature at which the heat is rejected bears to the temperature at which it is received*, both being reckoned from absolute zero, 460° * below the zero of Fahrenheit's scale.

These two propositions, expressed in algebraic formulæ, are:

(1) $U = H\frac{\tau_1 - \tau_2}{\tau_1}$, which, if $H = 1$, becomes the well-known equation $U = \frac{\tau_1 - \tau_2}{\tau_1}$; and,

(2) $L = H\frac{\tau_2}{\tau_1}$ in which also, if $H = 1 \; . \; . \; . \; . \; L = \frac{\tau_2}{\tau_1}$. But as $L + U = 1, \therefore \; U = 1 - \frac{\tau_2}{\tau_1}$, which is identical with (1) differently written.

At this point we need to divest ourselves of an idea which is common, and which naturally comes from the terms used, that "latent" heat is necessarily wasted heat—or, in other words, that if all the heat received was expended in elevating the temperature, instead of a large share of it going into the "latent" condition, we should be able to turn a larger percentage of it into power. It has been upon this erroneous supposition that most of the searches for substitutes for steam have been based. To show its fallacy, practically, it is only necessary to consider the action of an engine using steam as a gas without expenditure of latent heat, and compare it with the results attained in engines in which the latent heat is expended in the boiler and discharged in the condenser. We will assume that steam be supplied at 100° temperature—1 pound pressure, or 28 inches vacuum nearly—that it be worked through Carnot's cycle between that

* This is not only a convenient number to use, but the nearest approximation in whole numbers to the latest established absolute zero. Rankine used 461.2, but later physicists have settled upon 373.1 Cent., equivalent to 459.6 Fah.

temperature and 320°—the temperature of saturated steam at 75 pounds gauge pressure. The efficiency of this cycle would be, by above formula, $= \frac{780-560}{780} = .28$. The heat expended per pound of steam would be $220 \times .475 \times 772 = 80{,}674$ foot pounds of energy, of which the engine would utilize 28 per cent., or 22,588 foot pounds. There would, therefore, be required $\frac{1{,}980{,}000}{22{,}588} = 87.6$ pounds steam per hourly horse-power, and that in a perfect engine; but, working within the same limits, in a very imperfect engine, using water with its large latent heat, in actual practice, a horse-power is obtained for from 16 to 18 pounds, or about one-fifth the quantity of fluid. Latent heat must, therefore, be an efficient source of energy as well as sensible heat. That it is just as much so when working between the same limits of temperature, was demonstrated by Rankine in a series of articles published in the *Engineer* in 1857. And, in fact, it may be said there would be no available energy if there was no latent or specific heat.

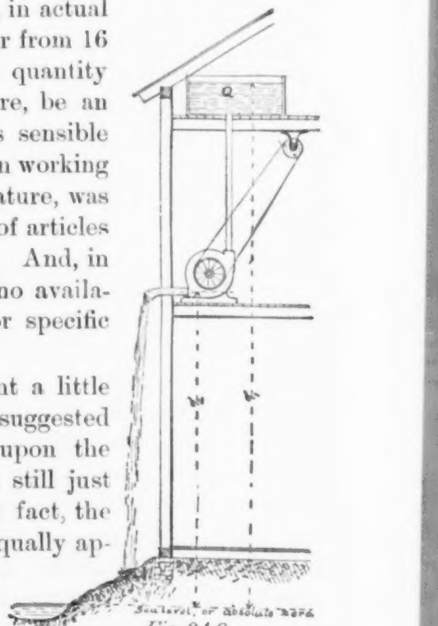


Fig. 248.

We may, perhaps, make this point a little plainer by means of an illustration suggested by Carnot, which, though based upon the theory of the materiality of heat, is still just as true under the correct theory. In fact, the second law of thermo-dynamics is equally applicable to a ponderable body as to heat, and may be summed up in the well-known adage, "Water will not run up-hill." Fig. 248 is a section of a building in which is situated a tank of water, or any other fluid, which is used to drive a water-motor upon a floor below, after which the fluid is discharged, whence it may or may not find its way to the sea-level—the line of absolute zero. Now it is evident the greatest possible effect obtainable in the motor-engine is represented by the weight of fluid, Q, multiplied by its fall to the point of discharge. The height of the surface of the tank above sea-level is τ_1, and the height of its discharge from same datum-line is τ_2, while its fall is $\tau_1 - \tau_2$, and the greatest efficiency of the motor is expressed by $U = Q(\tau_1 - \tau_2)$.

But the total energy of the fluid is represented by $Q\,\tau_1$, and the efficiency of the motor expressed in terms of total energy is $U = \frac{Q(\tau_1 - \tau_2)}{Q\,\tau_1} = \frac{\tau_1 - \tau_2}{\tau_1}$. It is evident that the same law holds good whatever be the character of the fluid in the tank. Now, the quantity Q,—which may represent the latent heat, while the height, τ_1, represents temperature—may be greater or less with the same height. If $Q = 0$, then there would be no available energy, for there would have been none expended. It will also be seen that if in the supposed steam-engine as above calculated, 0 be substituted for .475, the specific heat of the steam, there would be no energy in the engine.

From the mere inspection of the above formulæ, in view of this illustration, it is readily seen:

1st. That the useful effect can only equal the total heat expended when the temperature at which it is rejected is absolute zero, in which case it matters not at what temperature the heat may be received.

2d. That with a given minimum temperature, the higher the maximum temperature the greater will be the proportion of total heat converted into useful work.

3d. That it is of greater importance to lower the temperature at which heat is rejected than to raise that at which it is received.

There are, however, practical limits to these several values:

1st. The temperature of rejection cannot be carried below that of the substance into which it is rejected—in practice it must be several degrees above it—and is independent of the fluid employed. As there is, in practice, nothing available colder than the air or water, τ_2 cannot easily be less than 100° Fahr., 560° absolute.

2d. The temperature of reception cannot be greater than the highest temperature of combustion, nor greater than the surfaces of the piston and cylinder will stand; nor greater than will produce in the given fluid the highest allowable pressure.

3d. The highest pressure is limited by the strength of the mechanism and safety of its operation, and is also independent of the fluid. As all fluids except mercury and turpentine attain this limit of pressure before the limit of temperature, the pressure is the practical limiting condition in this direction.

Obviously, then, as the limits of lowest available temperature and of highest practical pressure are the same for all vapors, it

becomes evident that the fluid having the highest temperature at the limit of pressure, other things being equal, has the advantage, theoretically, in possible economy. Of all available liquids, water fulfills this condition best, and therefore it is useless to search for another vapor as a substitute for steam, unless it can be shown that the losses incidental to the use of the latter are necessarily enough greater than those incidental to some other fluid, to more than counterbalance this advantage. That there are such compensating advantages is not probable, and they would, indeed, need to be very great to offset the cost of fluid, water being free of cost in nearly all situations.

Were we, however, to look for a fluid to use in a "binary system," it must necessarily be among those which vaporize at a higher temperature than water, rather than, as has heretofore been sought, at a lower. In this way it might be possible to attain a higher range of temperature, and thus gain something theoretically in economy, unless, perchance, the additional loss in the furnace should offset to a large degree any such possible gain.

It becomes evident, therefore, that a successful substitute for steam in motive power cannot be found among vapors, and most probably, if found at all, it must be among permanent gases. By a bountiful provision of nature, an equally free and exhaustless supply of a perfect gas—atmospheric air—has been provided which has several elements of value in the problem. As it requires 500° of heat to double its pressure at 39° Fahr.—the point of greatest density of water—it may be heated to a very high temperature before it reaches a practical limit of pressure. It is a poor conductor of heat, and does not become fluid when cooled; therefore it will suffer less loss from being used in a cool cylinder. Being a supporter of combustion, the fuel may be burned within the working fluid, and the loss due to the furnace avoided. It also offers the opportunity of recovering a larger share of its rejected heat to be used again. In a steam-engine, the only use to which this rejected heat can be put is in heating the feed-water and air for combustion, and only a small fraction, not over $\frac{1}{6}$, can be utilized in that way, while with air Rankine estimates that as much as 90 per cent. of the heat in the exhaust may be retained for use, by a device invented by Stirling, known as the "economizer" or "regenerator." There are disadvantages, however, peculiar to air, among which are its bulk and the necessity of initiating motion by external power.

The former is overcome by compression before heating, and the latter may yet be provided for in some simple manner.

FUTURE POSSIBILITIES.

Air, then, gives the best promise for an economical substitute for steam in pressure engines. The development of its advantages involves many difficulties, but these are fast being overcome. The air-engine of Stirling, of forty years ago, equaled in economy any steam-engine of its day, while the Shaw air-engine, of 1867, equaled in economy of fuel the largest and most perfect steam-engines of to-day. The Otto engine, and others of similar character, exhibit an economy of heat double that of our first-class steam-engines, but they are handicapped by the necessity of using a very expensive fuel, and are necessarily confined to small powers and special circumstances.

It is a singular coincidence that the best gas or air engines attain to nearly the same percentage of their theoretical efficiency as the best steam-engines. Thus, a condensing engine working under 75 lbs. pressure of steam, and using 18 lbs. water per hourly H. P., has a theoretical efficiency of .28, and an actual efficiency of .12 for the engine, or .09 for both engine and boiler, which latter is equal to 32 per cent. of the theoretical. An Otto or a Clerk engine, according to various tests, utilizes 18 per cent. of the expended heat in power, working ordinarily between the limits of 2,700 and 900° Fahr. This would give a theoretical efficiency of .57, and these engines, therefore, attain to 31.6 per cent. of their theoretical efficiency, or practically the same as is done by the steam-engine. *A priori*, other vapors than steam cannot come nearer their theoretical economy, in all cases less than that of steam.

In this connection it may be of interest to note the rate at which the increase of the temperature of rejection, demands an increase in the maximum temperature to maintain the same efficiency as, for instance, the temperature and pressure of steam at which a non-condensing engine would equal a condensing engine under ordinary conditions. From equation (1), we obtain $\tau_1 = \frac{\tau_2}{1 - U}$, and by assuming $\tau_2 = 212 + 460$, with $U = .28$ as above, we have $\tau_1 = 933°$ or $473°$ Fahr., which corresponds with a pressure of about 550 lbs.—quite inadmissible in practice. It is not unusual to hear men say that the advantages of a condenser

can be obtained by carrying fifteen pounds more pressure on the boiler. The above shows the fallacy of this statement.

The prospect of much further economy in steam-engines is not bright. By means of a non-conducting lining for the cylinder, a saving might be effected of, say, 25 per cent. in fuel, bringing the efficiency up to 13 or 14 per cent., and by running the pressure up to 250 lbs., we might attain, possibly, an efficiency of 17 per cent., or the same as is now attained in gas-engines. In these, however, as now built, over 50 per cent. of the total heat is lost in the jacket, and 17 per cent. in the exhaust, of which losses there is no good reason why we may not expect to save one-half, so that the air-engine may attain to, say, $\frac{2}{3}$ its theoretical efficiency. At the same time, by reducing the lower limit of temperature, the theoretical efficiency may be raised materially. Thus, if the minimum be lowered from 900° to, say, 300°, leaving the maximum at 2,700 as before, we will have a theoretical efficiency of .75, and if two-thirds of that be realized, there will result an actual efficiency of 50 per cent. as that which may fairly be looked for in air-engines. If coal can be utilized directly for fuel, this will give a horse-power for an hour for $\frac{1}{3}$ lb. of coal, a saving of 78 per cent. upon the best results yet attained with steam. If, however, it is necessary to turn the coal into water-gas before it can be used, double the amount of coal would be required, or $\frac{2}{3}$ pounds per hourly horse-power, which is still a large advance on any possible attainment with steam.

Are there still further possibilities of economical development of power? Probably not in the line of pressure-engines. But science already points to the possible conversion of heat directly into electricity, and if that can be done without too great a loss, the electrical engine may yet become a prominent rival of the steam or air engine. The conversion of 90 per cent. of electrical energy into mechanical work is not beyond reasonable expectations, even if it is beyond present attainment, and if the heat of combustion can be converted into electricity with a loss of only 10 per cent.—which is supposable—we then should get a horse-power for .22 of a pound of coal per hour. This would be a saving of 30 per cent. over the best probable result with air-engines, or 85 per cent. over the best results yet attained with steam.

With these possibilities before us and the still further possibility of the direct conversion of heat into available power with not over 10 per cent. loss—by some means yet to be discovered—

who shall say there is not ample scope for invention and experimental research? We may, however, safely conclude that it lies not in the line of "binary engines," or the substitution of other liquids for water, or other gases for air.

Is steam, therefore, doomed to be superseded? By no means. Even if robbed of its position on the throne of power, it must ever remain one of the most useful servants of man. Its large specific and latent heat render it the best attainable medium of transferring heat within a certain range of temperature, from the furnace to the place where it may be wanted for various processes, and even now it fills a larger field in that direction than it does as a prime mover. So far as now appears, it need fear no successful substitute in that field.

DISCUSSION.

Mr. Thomas S. Crane.—Having been recently called upon to examine a carbon bisulphide engine operating an electric plant at Lowell, Massachusetts, I have thought it would interest the members, in connection with this discussion, to learn some of the facts observed during this examination.

The plant at Lowell consisted of two 14×28-inch engines coupled together and supplied with CS_2 vapor at 80 lbs. pressure by two generators containing liquid bisulphide.

These generators were jacketed with live steam, as well as the vapor conduit to the engines, and the cylinders and steam-chests of the latter.

Steam at 40 lbs. pressure was furnished by a boiler having less than 800 feet of heating surface (estimated at 60 horse-power), and was fed at a regulated pressure of 20 lbs. per square inch to the jackets of the CS_2 generators, and at a pressure of 40 lbs. per square inch to the jackets of the engine cylinders and vapor conduits. The CS_2 vapor operated in a closed circuit, passing through a surface condenser and being returned by a pump to the generators.

The engines operated two lines of heavy shafting and six 25-horse dynamo-electric machines at the time of this examination; and during the greater part of the time furnished a current to 138 arc lights of 2,000 candle power each, through 21 miles of wire, developing 174 horse-power by the indicator cards. The consumption of coal was 2,012 pounds (containing 1,634 pounds of combustible, with a new fire) during the entire period of the examination, 8½ hours.

The consumption of coal per horse-power per hour was therefore $1\frac{4}{10}$ lbs.; but as the engines, when first started, were not operated with their full load, an allowance for such lighter load would increase the expenditure of fuel per actual horse-power to $1\frac{37}{100}$ lbs. per hour per horse-power.

I would like to ask, first, whether, in view of these facts, it is reasonable to say that no substitute for steam can be found to develop greater economy? secondly, whether any horizontal tubular steam-boiler having less than 800 feet of heating surface is anywhere operating a steam-engine with a load of 174 indicated horse-power?

Prof. Wm. P. Trowbridge.—I consider the paper of Mr. Babcock one of the most complete and valuable contributions to the history of the classes of heat-engines discussed that has been presented. As a concise statement, or monograph, of the gradual development of such engines and the true theory of heat applicable to them, it must prove valuable to all who have not had the time or the opportunity to consult the various authorities who treat of these subjects. Such a paper is needed just now, and will probably always be of service as a guide and warning to those who are looking for impossibilities in attempts to secure extraordinary economy of power in heat-engines by the use of some other vapor than that of water. A recent number of the *School of Mines Quarterly** contains a short paper by myself, giving the results of computations of the relative efficiencies of steam and bisulphide-of-carbon vapor engines as the latter is employed in the recently constructed engines to which I have no doubt Mr. Babcock refers.

His paper will be efficacious in preventing the periodical re-invention of this bisulphide-of-carbon engine which presents itself at intervals with the announcement that it will save one-half to two-thirds of the fuel consumed by the steam-engine, but which succeeds only in causing large amounts of money to change hands under the excitement of ill-advised speculations.

The close relation which my paper had to the subject discussed by Mr. Babcock will, I trust, be considered a sufficient reason for my asking the privilege of giving here the principal results of my calculations. The following sketch illustrates in a general way the parts or organs of the bisulphide apparatus:

In Fig. 270, drawn in the most simple manner for illustration, S represents an ordinary steam-boiler, which delivers steam through

* April, 1886.

a pipe, P, to a large cylindrical chamber, A B. Within this chamber or shell is another, C, which contains liquid bisulphide of carbon. When steam is introduced into the larger chamber A B, the heat of the steam vaporizes a portion of the bisulphide, the vapor passing through a pipe, E, to an ordinary engine, where it acts like steam to give motion to the piston.

The exhaust from this engine is carried to a condenser G, from which the condensed liquid bisulphide is pumped by a pump, *h*, back to the bisulphide boiler through a pipe, I. The condensed steam in the shell A B, which comes from the liquefaction due to the heat imparted to the bisulphide of carbon, is pumped back to the steam-boiler by a pump, P.

The steam thus makes a circuit from the boiler S to the large

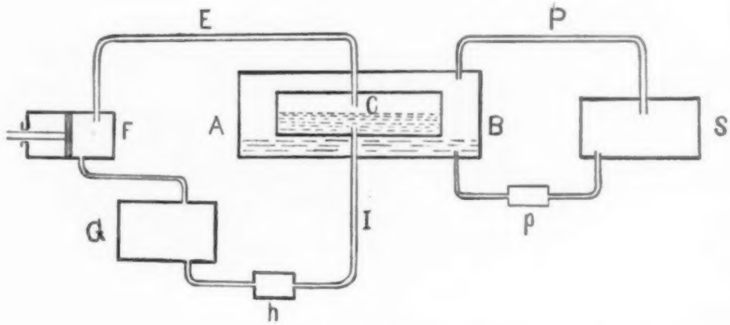


FIG. 270.

shell and back without entering the working cylinder, while the vapor of the bisulphide makes another circuit, as shown in the sketch, through the cylinder, performing work, and back to the interior bisulphide boiler C, without mingling with the steam. If no leakages occur, the same water and the same bisulphide may thus work on indefinitely, each making its own circuit.

Fig. 271 illustrates the general appearance of the most recent form of these designs as actually set up.

It will be seen that this apparatus may be changed into a condensing steam-engine by carrying the steam directly from the steam-boiler S to the cylinder of the engine, thence to the condenser G, from which the condensed water may be pumped back to the boiler S—by taking away, in fact, the intermediate shell A B and the interior bisulphide boiler C.

To determine the relative efficiencies of the two arrangements,

i.e. of the bisulphide arrangement, and of the apparatus used simply as a steam-engine, it will be sufficient to investigate the economical working of both under similar conditions, or by operating the apparatus as an ordinary condensing engine, and then as a bisulphide engine.

In the actual apparatus the boiler pressure in S is reduced by a reducing valve before it enters the shell A B, while, at the same time, the cylinder F is steam-jacketed with steam direct from the boiler S. The bisulphide vapor then rises through the pipe E at a lower temperature than that of the steam in the boiler S, but at a high pressure; and is to some degree superheated in its passage

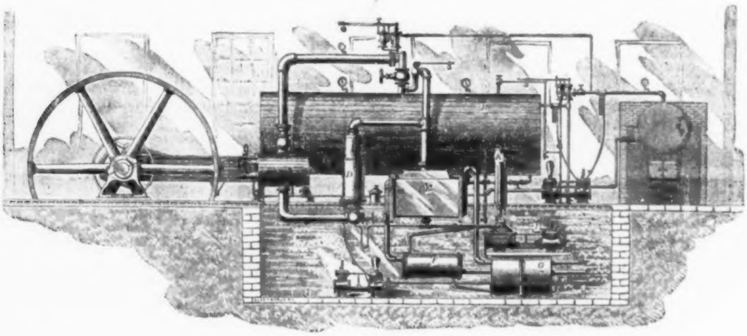


FIG. 271.

to the working cylinder, inasmuch as the pipe E is also steam-jacketed.

To ascertain the efficiency of the bisulphide-of-carbon engine thus described, the following formula of Rankine is employed in what follows:

The work in foot-pounds performed by one pound of the vapor in expanding and performing work between the absolute temperatures T_1 and T_2 is given by the expression,

$$W = a \text{ hyp. log. } \frac{T_1}{T_2} + b\,(T_1 - T_2) - \frac{c}{2}\,(T_1^2 - T_2^2) + V_2\,(p_2 + p_3)$$

in which the constants a, b, and c, are taken from the expression,

$$H = 1077732.62 + 139.35\ T - 0.\ 2118\ T^2$$

or

$$H = a + b\ T - c\ T^2$$

which represents the latent heat of evaporation of one pound of bisulphide of carbon in foot-pounds.

T_1 is the initial absolute temperature.

T_2 is the absolute temperature, corresponding to p_2.

V_2 the volume of one pound of vapor at the pressure p_2 (at the end of the expansion) and p_3, the back pressure.

The table on the following page, giving the temperatures, latent heats, specific weights, and specific volumes of steam and bisulphide of carbon in British units, has been prepared from corresponding tables, giving the same quantities in French units, from Zeuner's work on the Theory of Heat.

This table serves to determine the quantities employed in the formulæ.

From p_1, the initial temperature, we get from the table $t_1°$, from which is easily obtained T_1.

If r be the ratio of expansion and v_1 the initial volume of pound of vapor, then we have $v_2 = rv_1$, and from v_2 we find from the table p_2 and T_2.

An engine recently constructed and kept in use for several months, was observed to be working under the following conditions:

$p_1 = 70 + 14.7 = 84.7$ lbs. per sq. inch $= 12196.8$ lbs. per sq. foot.

$r = 4.$

$p_3 = 10$ lbs. per sq. inch $= 1440$ lbs. per sq. foot.

$v_1 = 1.034$ (from table) cubic feet.

$v_2 = 4 \times 1.034 = 4.136$ cubic feet.

$t_1 = 233.92.$

$T_1 = 693.32.$

$t_2 = 132.14°.$

$T_2 = 591.54°.$

$p_2 = 19.93$ lbs. per sq. inch $= 2869.92$ lbs. sq. foot.

$$\frac{T_1}{T_2} = 1.172.$$

$$T_1 - T_2 = 101.78.$$

$$T_1^2 - T_2^2 = 130673.05.$$

$$p_2 - p_3 = 1429.92.$$

$$\text{hyp. log. } \frac{T_1}{T_2} = 0.1588.$$

Putting these numerical values for the quantities which represent them in the expression for $\overline{W}$, we have

$$\overline{W} = 23340.6 \text{ foot lbs.,}$$

TABLE.—*Showing the temperatures, specific heats, heat of liquid, latent heats, specific volumes, and specific weights of water* (H_2O) *and carbon bisulphide* (CS_2).

Temperature.		Pressure in lbs. per sq. "		Heat of liquid. From 32° F.		Latent heat of evaporation. Eng. Units.		Specific volume. Vol. of 1 lb in cubic feet.		Specific weight. Wt. of 1 cubic foot in lbs.		Specific heat of liquid.		Specific heat of vapor.	
C.	F.	H_2O.	CS_2.	H_2O.	CS_2.	H_2O.	CS_2.	H_2O.	CS_2.	H_2O.	CS_2.	H_2O.	CS_2.	H_2O.	C_2S.
0	32	0.09	2.47	0.00	0.00	1091.70	162.00	2274.80	28.18	0.0004	0.0356	1.000	0.236	Volume of 1 lb of liquid carbon sulphide = 0.128 cubic feet. Volume of 1 lb of liquid water = 0.016 cubic feet.	Spec. grav. of liquid CS_2 = 1.25; " water = 1.00
10	50	0.18	3.83	18.00	4.25	1079.19	160.31	1738.34	18.78	0.0060	0.0532				
20	68	0.34	5.76	38.01	8.53	1066.66	158.44	940.70	12.88	0.0010	0.0777		0.238		
30	86	0.61	8.41	54.02	12.83	1054.12	156.38	533.00	9.07	0.0018	0.1102		0.239		
40	104	1.06	11.94	72.09	17.17	1041.61	154.15	314.65	6.54	0.0031	0.1529		0.241		
50	122	1.78	16.58	90.15	21.53	1028.99	151.76	193.06	4.81	0.0052	0.2078		0.242		
60	140	2.87	22.52	108.25	25.94	1016.39	149.17	154.41	3.613	0.0064	0.2767		0.245		
70	158	4.59	30.02	126.36	30.35	1003.77	146.41	80.28	2.756	0.0120	0.3629		0.245		
80	176	6.84	39.31	144.51	34.81	1001.11	143.46	54.17	2.136	0.0190	0.4681		0.248		
90	194	10.14	50.65	162.69	39.27	978.42	140.35	37.34	1.679	0.0277	0.5956		0.249		
100	212	14.67	64.31	180.90	43.79	965.70	137.05	26.44	1.336	0.0378	0.7484	1.0132	0.251		
110	230	20.75	80.53	199.15	48.32	952.94	133.58	19.15	1.077	0.0522	0.9285		0.252		
120	248	28.78	99.58	217.46	52.90	940.13	129.92	14.05	0.876	0.0712	1.1415		0.254		
130	266	39.18	121.68	235.79	57.51	927.28	126.09	10.53	0.721	0.0950	1.3869		0.256		
140	284	52.45	147.16	254.19	62.14	914.37	122.09	7.99	0.599	0.1251	1.6669		0.257		
150	302	69.12	175.92	272.63	66.80	901.40	117.90	6.17	0.502	0.1620	1.9920	1.0262	0.259	0.480	0.1596

for the work performed by one pound of bisulphide of carbon under the conditions given.

The heat expended in foot-pounds to produce the same is given by the equation,

$$h = J\,c\,(T_2 - T_4) + a + b\,T_2^\circ - c\,T_2^2 + W - V_2\,(p_2 - p_3).$$

In which $J = 772$.

$T_4 =$ absolute temperature of the condenser.

Giving the proper numerical values to these quantities from the tables, as before, we have

$$h = 139986.24 \text{ foot lbs.}$$

The efficiency is found by dividing the work by the heat in foot-pounds required to produce it.

$$E = \frac{W}{h} = \frac{23340.6}{139986.24} = 0.167.$$

For a condensing steam-engine, under the same conditions, we may assume that the boiler pressure can be carried at 80 lbs. guage or 95 lbs. absolute pressure, and that the pressure in the condenser is 4 lbs. We will then have for the steam-engine, using the same apparatus, with the bisulphide attachments left off, and employing the same formula:

$p_1 = 13680$ lbs. per square foot.
$t_1 = 323.7^\circ$.
$T_1 = 783.1$.
$V_1 = 4.5644$ cubic feet.
$V_2 = rv_1 = 18.2576$ cubic feet.
$t_2 = 231.3$.
$T_2 = 690.7$.
$p_2 = 21.29$ per square inch $= 30657.6$ per square foot.
$p_3 = 576$ lbs. per square foot.
$r = 4$.
$p_4 =$ pressure in condenser $=$ lbs.
$t_4 = 124^\circ$.
$T_4 = 583.4$.

Substituting these numerical values, with the proper values of a,

b, and c for steam in the formula for the work, $\overline{W}_1$, we have for the steam-engine

$$\overline{W}_1 = 152888 \text{ foot lbs. of work per lb. of steam.}$$

And for the heat in foot-pounds expended,

$$h_1 = 971581.66.$$

From these expressions we get

$$E^1 = \frac{\overline{W}_1}{h_1} = \frac{152888}{971581.66} = 0.157$$

for the efficiency of the steam-engine.

These two determinations differ by only about one per cent., or one hundredth part—a remarkable coincidence, when it is considered that each of these results depends on separate experimental determinations of the latent heats and the relations between the vapor-temperatures and pressures of each of them.

The comparison verifies, as was to have been expected, the law that theoretically the two engines are equal in efficiency.

There is one further consideration to be introduced, for which science gives us no precise practical solution at present. It is known that in the ordinary use of steam there is a waste of steam which does not appear in the ordinary theoretical formulas, due to the absorption of heat by the walls of the cylinder during the influx of steam into the cylinder, and the cooling of the walls of the cylinder and the waste of heat during the efflux or exhaust.

This cause of loss or diminution of efficiency was first brought to the attention of engineers, I believe, by Professor Rankine, in a paper read before the Institute of Engineers of Scotland, in 1852. In another paper, published in 1868, he discussed the confirmation of his previous deductions by Mr. Isherwood in the experiments made by the latter under the auspices of the U. S. Navy Department. A recent paper by Professor W. D. Marks, of the University of Pennsylvania, published in the Journal of the Franklin Institute, discusses the subject fully.

The loss from this cause in any particular case is uncertain in amount. The question, however, in the present case is not as to the actual amount of loss, but whether a loss from this cause, which must take place in the bisulphide engine, is greater or less than

that for the steam-engine. This is the only uncertainty in the comparison of the two engines, and until it is demonstrated by experiments that steam and bisulphide vapor differ in this respect, we cannot assume superiority for either as far as cylinder condensation is concerned.

It is thus clearly demonstrable, not only by the application of the general laws of physics as determined by the science of Thermo-dynamics, but by special formulas applicable to heat-engines in which the results of experiments on the physical properties of vapors are employed, that for the same initial and final temperatures there is no theoretical advantage in the use of bisulphide of carbon.

A similar discussion would show the same to be true of the vapor of ammonia.

In regard to the hot-air engine, Mr. Babcock describes its status very clearly. I do not understand him to predict that hot-air engines will ever supplant the steam-engine, but merely to state undoubted facts in regard to the efficiencies of such engines; while he hints that it is possible that some of the disadvantages in the use of this class of engines may in the future be remedied. It does not seem to me, however, that any one can suggest at the present day the means by which the radical obstacles which now stand in the way of the hot-air engine as a universal motor may be removed. These obstacles are chiefly the great bulk of the engines for a given power, the high initial pressure compared with the mean and final pressures, the mechanical difficulties attending the heating and cooling of the air, and the difficulties attending provisions for a reserve of power, etc., etc.

It was shown first by Hirn, and demonstrated also by Zeuner, that the *regenerator* is of no use in air-engines which work under conditions of maximum efficiency. So that neither past experience nor present knowledge seem to me to give much encouragement to the idea that the hot-air engine can ever hold more than a subordinate place among motors.

Mr. Babcock's allusion to the Second Law of Thermo-dynamics, and the illustration which he gives of the analogy which exists between the work performed by a fall of water and a fall of heat, and the similarity of the conditions of efficiency in the two cases, furnish an opportunity which I beg the privilege of making use of to say a few words in regard to the Second Law of Thermo-dynamics.

The unfortunate enunciation of this law by Clausius, in one of

his first memoirs, has given rise to no end of confusion of ideas in regard to it. It is difficult to find two writers on the subject of heat who agree in their exact definitions. Clausius' first enunciation was as follows: "*Heat can never pass from a colder to a warmer body without some other change connected therewith occurring at the same time*," a mere negative proposition which means nothing when stated as a law of nature or of physics, and which certainly sounds like the old adage, "water cannot run up-hill."

Clausius, who, it will be remembered, was jointly (though independently) with Rankine a founder of the accepted science of Thermo-dynamics, in a subsequent memoir modified his definition, prefacing this modified definition or enunciation with the remark that "*the second theorem should be expressed with sufficient clearness to show the real nature of the theorem, and its connection with the first theorem*." The second definition was as follows:

"*If two transformations, which without necessitating any other permanent change, be called equivalent, then the generation of the heat of the temperature t, from work Q has the equivalent value* $\frac{Q}{t}$, *and the passage of the heat Q from the temperature t to the temperature* t_2 *has the equivalence value* $Q\left(\frac{1}{T_2}-\frac{1}{T_1}\right)$, *wherein T is a function of the temperature independent of the nature of the process by which the transformation is effected*." It is difficult to imagine any necessary relation between these two definitions, and yet the former has been most frequently quoted in various ways as the *Second Theorem* or Law of Thermo-dynamics. Rankine gives several definitions also. His first is as follows:

"*The Second Law of Thermo-dynamics*." "*If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided in any number of equal parts, the effects of those parts in causing work to be performed are equal*." And again: "*The second Law of Thermo-dynamics, expressed with reference to absolute temperature*." "*If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal*."

It will be seen that the last enunciations of both Clausius and Rankine agree, substantially, and both refer to the *absolute temperature*.

Both may be included under one general enunciation; and the first and second laws may be stated simply as follows:

1st Law.—Heat is the living force, or vis viva, due to certain molecular motions of the molecules of bodies, and this living force may be stated or measured in *units of heat*, or in *foot-pounds*, a unit of heat in British measures being equivalent to 772 foot pounds.

2d Law.—The living force or vis viva of a body (called heat) is always proportional to the absolute temperature of the body.

From this enunciation it follows that any variation of the heat, or living force, of a body in performing work, is to the whole heat as the variation of the absolute temperature is to the whole absolute temperature; or the disappearance of heat and the disappearance of absolute temperature, when work is performed by a body, are concordant phenomena. The absolute temperature is thus a sort of measure of the sensible heat of the body. And the symbolical expression of the Second Law by both Clausius and Rankine will follow directly from the enunciation,

$\frac{Q_1 - Q_2}{Q_1} = \frac{T_1 - T_2}{T_1}$. This is a well-known expression, which may be called the symbolical or algebraic enunciation of the Second Law—the law which limits the efficiency of heat-engines and which does not depend on the nature of the working medium employed.

Mr. Geo. H. Schuhmann.—It may be of interest to some of the members if I add to Mr. Babcock's valuable paper a short description of a motor, which was also intended to supersede the steam-engine, but which also landed in the scrap-heap before it had accomplished this result. I have reference to the "Bradley Promethor," which was built in a Philadelphia machine-shop during the summer of 1877. The working fluid was to be a "gas," which was generated by injecting water, in fine sprays, into a red-hot boiler, or "generator," as the inventor called it; and the gas, after doing its work in a motor, was to be burned up underneath the generator. This generator consisted of a main pipe about 4″ diam. × 4 ft. long, with 8 branch pipes on each side; small, bottle-shaped steel castings (of about $1\frac{1}{2}''$ largest inside diameter), were screwed into the lower side of the branch pipes (about 80 bottles in all). Inside of the generator was a feed-pipe, with a branch for each branch of the generator, and into these branches small pipes were screwed in, so as to lead the water into the steel bottles. The ends of these small pipes were plugged shut with small caps, which had each a small hole in the center, probably not more than $\frac{1}{64}''$ in

diameter, so that when water was pumped into the feed-pipe, it was forced out of these little holes, in fine streams, into the bottles. Above the generator were quite a number of superheating "cells," similar to a Harrison boiler, but much smaller; the whole apparatus was tested to 4,000 lbs. hydraulic pressure, and was then put into a little furnace lined with fire-brick. The generating bottles were first heated *red hot*, then the feed-water was injected by a hand-pump, causing the pressure-gauge to go up in jumps of 50 to 100 lbs. each stroke of the pump, until the working pressure of 1,000 to 1,200 lbs. was reached. Then the motor was started and the intention was to regulate the pressure by regulating the feed-pump. The inventor claimed that the generator contained a combustible gas, and when a small test-cock was opened, the escaping gas would make a terrible noise, but no moisture could be seen. When the exhaust from the motor was turned into the fire, the combustion was very much accelerated, and the inventor's principal claim was that burning up the exhaust underneath the generator would result in a large saving of coal, and owing to the high pressure carried, a small motor could do a tremendous amount of work.

It is probable that when first started, a part of the injected water was dissociated into its elements, the oxygen combining with the red-hot steel setting the hydrogen free, but after the steel was saturated with oxygen, no further dissociation could take place and the gas could then be nothing else but highly superheated steam. One day, during the absence of the inventor, a temporary condenser was constructed by connecting a line of piping to the generator and the gas turned on slowly; the escape at the other end of the line was steam and hot water. The "motor" was quite a curiosity. The principal part was called "the reciprocating block," and its interior consisted of three cylinders, one below and two above. Each cylinder had a stuffing-box, to admit a 3″ plunger. The outlines of the block represented a heart. It had a wrist-pin on each side, and two connecting-rods connected it to a double crank-shaft. The plungers were stationary, and the gas was first let into the lower plunger, driving the block upwards, then it was let out of the lower plunger into the two top plungers, driving the block down again; the final exhaust was into the fire-box. As all three plungers were of the same diameter, the ratio of expansion was only two. At the first trial the motor did not *mote* very long, as the brass plungers expanded and stuck fast in the cylinders. Mr. Babcock mentioned an engine that congealed, but this one did quite the reverse; in

fact, it became so hot that the bright work had all the colors of a rainbow; to lubricate any internal part was entirely out of the question. Several more trials were made, but the machine got "balky" every time, so that no economy test could be made. One trial was made in the presence of some experts, who were to report about the feasibility of using the "promethor" on a street-car; they watched the pressure-gauge rather anxiously, and said the machine was too hot to go close to it. After a long delay, the "company" had another motor built somewhere in Maryland; the working pressure was greatly reduced, and the motor was very similar to a little yacht engine. They put it on a boat and made a trial trip down Chesapeake Bay, and I was told that when they reached salt water, the little holes in the generator bottles got stopped up and they had to hail a tug-boat to tow them home again.

Mr. W. F. Durfee.—The idea of using steam injected into a red-hot boiler is quite an old one. I once saw an attempt to use steam that was so hot that it melted a lead washer off the cylinder-cock, burned out all the packing around the piston-rod and valve-stem, and cut the cylinder and valve faces full of grooves. The interior of the cylinder was also pretty well used up. The result was a dismal failure.

Mr. Wm. Kent.—I think Mr. Babcock has done great service to engineers, for there is scarcely any of us who has not been approached for an opinion (without a fee) as to some vapor-engine. I think that all we have to do now is to send such persons to the secretary for the paper of Mr. Babcock, simply saying to him, "Those are my sentiments."

Mr. Babcock says, "Science already points to the possible conversion of heat directly into electricity." My present impression is that the indications of science are exactly the opposite. I do not see in any way that science is now tending toward the use of electricity in place of steam, but on the contrary, steam-power is used to furnish electricity.

Mr. John Walker.—Of the various substitutes for steam mentioned by Mr. Babcock, I would advise our members to let bisulphide of carbon alone.

I happened to be with Messrs. Poole & Hunt, of Baltimore, at the time the experiments referred to were made. After the explosion, I personally aided in assisting Mr. Poole, and attending to him and the other parties burnt and otherwise injured.

One of the sufferers was overcome with heat and gas. I had the

presence of mind (knowing something about persons being gassed), to take damp sand and put his head into it, until the gas was exhaled from his system.

The experiments were a success until the explosion; the results of the experiments were carefully compiled by a young man called John D. Isaacs, and I believe published in pamphlet form by Messrs. Poole & Hunt. The data were carefully taken, and are very interesting, and my recollections are that they showed excellent results. However, there are at least two reasons why we should let bisulphide of carbon alone: First, everything about it smells like a bone-yard: Second, the awful possibility of being blown to Kingdom-come.

Mr. F. W. Taylor.—Might I ask Mr. Walker to explain a little more thoroughly his method of resuscitating men who have been overcome by the effects of gas?

Mr. Walker.—I learned of the treatment for persons when gassed in the North of England, where it is used by the employés about the blast-furnaces.

A bed of sharp sand (sea or lake sand, coarse quality preferred, or very fine gravel will answer), say eighteen inches square and nine inches deep, is prepared, thoroughly saturated with water. A depression is formed in the center of this by scraping out (not compressing) with the hand about the size of patient's head; place his face in the soft depression so that he can breathe through the moist sand, and the patient will usually recover in a short time. When the patient is removed, exhalation of the gas can be plainly seen on the sand in a greenish tint.

In addition to this treatment, stimulants have been administered to the patient with good effect.

Mr. Kent.—What can be the theory of that method of cure?

Mr. Walker.—I am not a doctor, and profess to know very little of the theory of this question.

It would seem, however, that the humidity of the air the patient breathes through the damp sand causes the gas to exhale sooner than would be possible with the common atmosphere; the damp air at the same time cools the nostrils and parts affected.

Mr. G. M. Bond.—Is it necessary to have artificial respiration?

Mr. Walker.—It is all the better to have it.

Mr. Crane.—I would like to state that the difficulties in reference to the bisulphide vapor are directly dependent upon how the work with it is done. I was around the present motor two days,

day and night, and could not tell that there was any bisulphide used. It was kept in the generator and engine right along. It would have been as difficult to set fire to it as it would have been to set fire to a pile of ice. Every receptacle in which the bisulphide was contained was surrounded with a steam-jacket. A steam-jacket is a very good protector. There was no place in the working of the engine where the gas was discharged. The vapor was retained. We cannot admit as engineers that the world does not move on. Steam itself made a great many enemies, because it cast some smoke over the green fields of England at first, and frightened the cows. Electricity has paralyzed a great many men who did not know how to go about their work, and so it has been with the substitutes for steam. Men have experimented and taught their successors how to manage that motor.

Mr. Walker.—Some trouble was found, at the time to which I refer, with the bisulphide of carbon itself. The bottom of the condenser would occasionally be filled with small crystals, mixed with the lubricant which went to the condenser from the engine. These crystals seemed to have formed in the condensing process. I might add also, that all the working parts of stop-valves, pump, and engine, that came in contact with the bisulphide of carbon in liquid or gaseous condition, had to be of iron, and the stuffing-boxes were made double—such as are used in ammonia-pumps, with a by-pipe in the chamber to carry off any escaping vapor or liquid.

Mr. Crane.—There is a little exhaust-chamber in the stuffing-box of the present motor—an ordinary stuffing box was used. Any vapor or liquid which attempted to follow the rod out was sucked right away to the condenser; of course, nothing was lost.

Mr. Geo. H. Barrus.—This subject would be incomplete without further reference to an air-engine which is being developed in Boston. I refer to the engine designed by Messrs. Woodbury, Merrill & Patton, and built by Mr. Joshua Merrill of Boston. Several of these engines have been built and used, but they have not yet been put on the market. The projectors have spent five years or more in experimenting and perfecting the machine, and the desire to realize to some extent the excellent results which the engine promises, has delayed its appearance before the public. It is claimed that this form of air-engine is adapted for very large powers. One engine has been running a factory in the town of Winchendon, Mass., for three years, developing about ten net horse-power. The last machine built, one which is now being used for experiment, de-

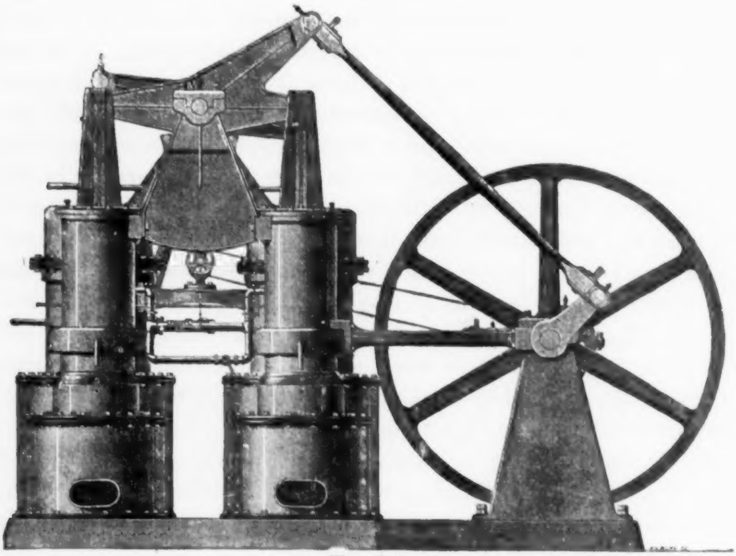


Fig. 272.

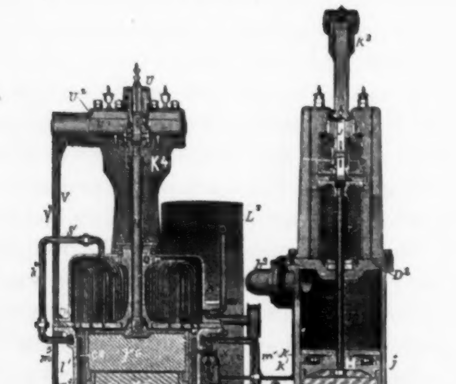


Fig. 273.

velops twenty net horse-power. This engine gives promise of a consumption of $2\frac{1}{2}$ pounds of coal or less per net horse-power per hour. The engine works on the same principle as the Stirling engine, but in perfection of detail is far ahead of it. It has two working cylinders, each of which is double-acting, and two reverser plungers, which effect the transfer of air from the heater

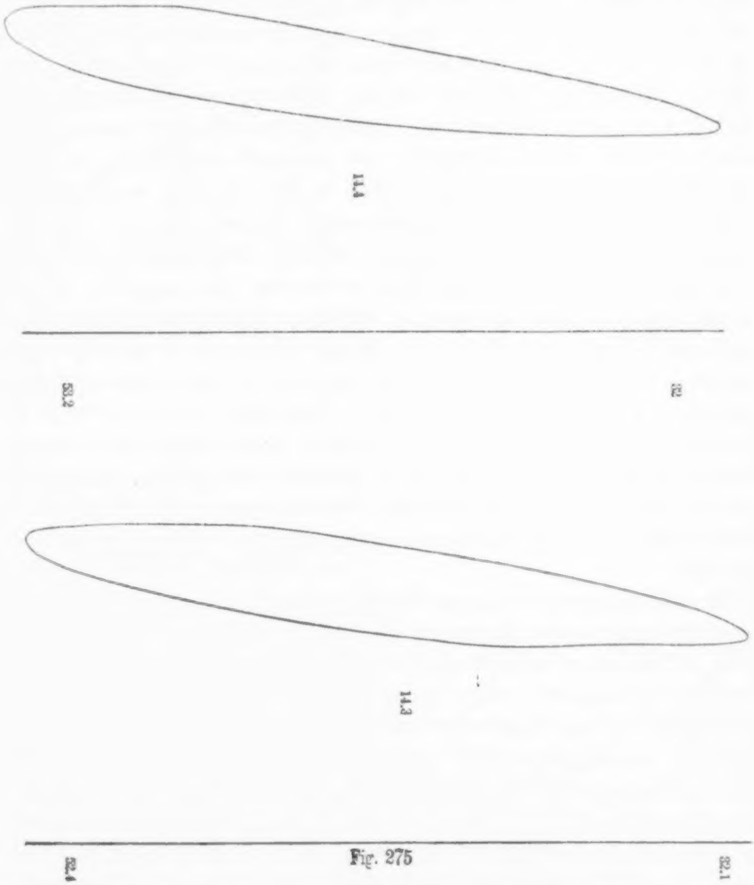


Fig. 275

to the cooler. The pressure used is several atmospheres. A general idea of the engine may be gained from the accompanying sectional cuts, Figs. 272 and 273, appended to which are copies of indicator diagrams, taken by the writer from one of the engines two years ago, Fig. 275. The scale of the spring used is 30 pounds per inch.

Mr. Babcock.—Mr. Crane suggests * that the action of the heat passing through the walls of the cylinder, instead of being put into the fluid in the boiler, may alter the condition of the problem and the formula. It will, but its only effect will be to reduce the possible economy. The condition of what is known as the Carnot formula is that all the heat shall be imparted to the fluid at the higher temperature at the beginning of the cycle; that what is discharged shall be abstracted at the lower temperature at the end of the cycle, and any variation from that reduces the possible economy. The amount of heat which could be carried through the walls of the cylinder with only 100 degrees difference between the internal and the external fluid, with the small conducting power of the fluid itself, is very slight, and would produce no important effect. As is well known, the object of a steam-jacket upon a steam-engine cylinder is only to prevent internal condensation. It does not increase the efficiency, but it reduces the quantity of the steam used. There is a portion of the steam which gets through without doing any work at all. Being condensed at the beginning of the stroke, and evaporating at the end of the stroke, it is not shown at all on the indicator cards. The steam-jacket tends to reduce that waste. It is probable that a steam-jacket on a vapor-engine might have a similar effect, possibly in a greater proportion, for the reason that the condensation of the vapor with its low latent heat would be as great, or greater, notwithstanding the lower temperature.

Mr. Crane seems to suppose that superheating would materially alter the expansion line upon the indicator card. It would not. The expansion of superheated steam varies from that of saturated steam only slightly. Any vapor which is superheated sufficiently to furnish all the heat converted into work during its expansion without condensation will expand, according to what is called Mariotte's law, the curve of the hyperbola, while the expansion line of saturated steam, as shown by Rankine, varies from that only as the tenth root of the ninth power, which is very slightly. The formulæ given in the paper are not intended to be applicable to actual practice. They show, not actual economy, but the greatest possible economy theoretically to be obtained. The remarks which Mr. Crane

* Mr. Crane having withdrawn from the record the remarks to which this reply refers, it might have been advisable to withdraw the reply also, but as it covers some points not elsewhere treated in this connection, it has been thought best to let it stand as spoken.

attributes to Prof. Thurston and others refer to the fact that the formulæ we have are only for theoretical conditions, not for practical conditions. In practice they would need to be very greatly reduced, not increased. Gantt and Maury found in their investigations that bisulphide vapor produced a small percentage of economy over steam, not twenty per cent., as stated, nor even four per cent., as I remember it; and that only when the bisulphide vapor was used at 250 pounds to the inch, as compared to steam at a pressure of 120 pounds. This was unfair for steam. There is no reason why we cannot use 250 pounds of steam as well as 250 pounds of bisulphide vapor. I do not find any such admission as Mr. Crane refers to, that under proper circumstances bisulphide would have the advantage. In regard to the experiments reported from the present motor, I have no reason at present to dispute figures. I know nothing more about them. I do know that the parties have refused to have competent investigations made of the engine. In that respect they stand on a par with the Keely and the Paine motors. The parties who called my attention to this bisulphide motor, and for whom I went to see it, told me that Prof. Richards, of Yale, and Prof. Trowbridge, of Columbia, had investigated it and made a very favorable report, of which they promised to send me a copy. This they neglected to do. What the character of Prof. Trowbridge's report would have been is seen in his contribution to this discussion. It certainly cannot be considered favorable to the claims of the motor.

Mr. Kent asks in regard to electricity. Science certainly does point in that direction. It has been known for many years that through what is known as the thermal battery we can convert heat into electrical activity. Within a few years past a Frenchman, I do not now recall his name, produced what he called briquetts, composed of carbon and nitrate of potash wrapped in asbestos cloth, which when thrown into the fire, so long as they continued to burn, produced a continual stream of electricity, but, of course, at too great a cost for practical use. Mr. Edison is at the present time investigating in that line. I have great hopes that some time in the future, it may not be very far, because we know that science travels very fast nowadays, we shall be able to burn coal and convert it directly into electricity, instead of, as we do now, indirectly through the steam-engine.

In reply to Mr. Walker, I would say, I have never seen any results from the binary engine—which in itself is far ahead of the

simple bisulphide engine—which compare favorably with the best results obtained by the compound steam-engine. There is no doubt but that a wasteful steam-engine, and all our steam-engines are wasteful, may be converted into a more economical engine by adding to it another cylinder, in which bisulphide of carbon or other vapor may be used. It is also well known that equal if not better results can be obtained by using the steam in the additional cylinder before condensing it; and now they tell us that by using three cylinders, or triple expansion, they are securing wonderful results in the way of economy by steam alone, better than anything that has ever been talked of, even for bisulphide, so that we do not need to go outside of theory yet to account for accomplished results.

As to the means of preventing the escape of objectionable vapor from the engine, there has nothing been invented recently. The means described by Mr. Crane are precisely the same as were used by Mr. Ellis, and they were precisely the same as those used by Laboreau, many years previously. While this engine at Lowell was in the hands of experts in its use, it was quite possible, perhaps, to keep the vapor within bounds, but immediately that such an engine was put into the hands of an ordinary workman it would be found to be an exceedingly dangerous thing.

Prof. Trowbridge, working in a different way, has arrived at the same result, and has clearly demonstrated that no difference in economy is to be attained by substituting other vapors for steam. He, however, suggests that there may possibly be slight practical gains if it can be shown that there is less condensation in the cylinder—the greatest known source of loss in the steam-engine. I fear, however, that little hope can be indulged in this direction, for the great density of the vapor will, probably, more than compensate for its lower temperature. The density of the CS_2 vapor is 4.22 times that of steam at same pressure, while at the temperature of the condenser, at which the cylinder remains at least one-half the time, it is 50 times as great. It is reasonable to suppose that because of this comparative density its action in cooling the cylinder is much greater than that of steam, necessitating a greater condensation to restore the heat at the beginning of the next stroke. The cards given above sustain this expectation, as the expansion curve is farther removed from the theoretical than in a steam-jacketed steam-cylinder, even though the difference in the temperature in jacket and cylinder is much greater than in the case of steam. The vapors of ether and chloroform are much denser than

that of carbon-bisulphide, that of alcohol less so, but still far in excess of steam.

Prof. Trowbridge very correctly concludes that it is not my intention to predict the future supplanting of steam for power by the air-engine, but the probability looks clearer to me than to him. The difficulties which he enumerates are already in part overcome. The gas-engines of to-day develop as much power with a given size of cylinder as do the steam-engines. The great difference between initial and final pressure is an element of economy, and as has been shown by Mr. C. T. Porter, with a properly proportioned engine, employing an impulse at each reciprocation, such difference is needed to produce uniformity of rotative effect. The difficulty of heating the air is not found in a gas-engine, but no sufficient means of cooling it has yet been devised.

It is true, as stated by Hirn and others, that the regenerator is of no use in an air-engine working under conditions of maximum efficiency, because in that case the air would be discharged at the same temperature at which it was received. But that condition is difficult, if not impossible, to attain. The regenerator affords a means of securing an approximation thereto.

*Mr. Geo. H. Babcock.**—Prof. Hutton has handed to me since the meeting the following table, giving performances of the Rider engine mentioned in the paper contributed by the Delamater Iron Works, by the courtesy of Thos. J. Rider:

SIZES AND PERFORMANCE OF RIDER ENGINES AS MADE BY THE DELAMATER IRON WORKS.

1	2	3	4	5	6	7	8	9
Nominal Size.	Power Piston.		Revolutions per minute.	Work Performed in Gallons. Water raised 50 feet per hour.	Coal consumed per hour.	Work done in Horse-power.	Coal per Horse-power per hour.	Weight of Engine.
	Dia.	Stroke.						
5"	5"	7"	100—160	350 gallons.	3 lbs.	0.07	40.3 lbs.	1050 lbs.
6"	$6\frac{3}{4}$"	$9\frac{1}{2}$"	80—120	1000 "	5 "	0.21	23.8 "	1800 "
8"	$8\frac{1}{2}$"	12"	80—120	2000 "	7 "	0.42	16.6 "	2700 "
10"	$10\frac{1}{4}$"	$14\frac{1}{2}$"	80—110	3000 "	9 "	0.64	14.2 "	3600 "

I have calculated and added columns 7 and 8, showing the horse-

* Contributed since the adjournment of the meeting.

power exerted and the cost of the horse-power in pounds of coal per hour. The horse-power thus calculated is the efficient work done. The indicated power must necessarily be much larger in engines doing so small an amount of work. The pounds of coal per horse-power are therefore not comparable with the performance of larger engines per indicated horse-power. Considering the small power exerted, the economy is all that could be expected.

Since the above paper was prepared and read, Chief Engineer Isherwood, of the United States Navy, has published in the *Franklin Institute Journal*, June, 1886, the results of a series of interesting experiments made at the Navy Yard by Chief Engineers Theodore Zeller and George P. Hunt, with an engine in which air was pumped into the steam-chest under the same pressure as the steam, and expanded in connection therewith in the cylinder. The results show that for the same amount of total power developed, the amount of feed-water required was precisely the same when air was used as when it was not. Mr. Isherwood thinks there should have been a difference in favor of the air, owing to the well-known action of air when mixed with steam to retard condensation, and accounts for the want of any economy in the experiment on the supposition that there was no intimate mixture within the cylinder. He argues that, in order to secure any of the advantages which might with propriety be expected in a non-condensing engine, it is necessary to produce the most intimate mixture between the air and steam, and that for that purpose some apparatus, more or less complicated, will be required. The article and the experiments referred to fully sustain the position taken in the paper in speaking of the Cloud engine.

After the close of the discussion, the annexed indicator cards (Fig. 250) were handed to me, without the dotted lines, as illustrating tests referred to by Mr. Crane of what is known as the "Triple Thermic Motor," with a request that I give my opinion in regard to the same. A pamphlet was also circulated among the members giving a full report of certain tests. This pamphlet includes the same diagrams and three other sets quite similar. At the very outset, in this report, we come upon an extraordinary phenomenon. It is stated that the pressure in the vapor generator was 80 lbs., and that the cards show, in all cases, 85 lbs. pressure in the engine cylinder, which curious condition is specially referred to and attributed to the superheating of the vapor within the *open* conduit on its way from the generator to the cylinder. We have, then, to assume, if

the cards and data are correct, that the vapor flowed continuously from a vessel at 80 lbs. pressure into another at 85 lbs.—something quite new in mechanics. It is not probable that the steam-gauge upon the generator was wrong. The pressure of the steam in the enveloping jacket is given at 20 lbs. and the corresponding difference in temperature between the two sides of the evaporating surfaces is stated to be 5°. Certainly, then, 80 lbs. is the highest pressure of vapor which could be maintained when the engine was running at full power. Indeed, it is questionable if a difference of only 5° in temperature between the two sides of the generator would be sufficient to evaporate the quantity of fluid required.

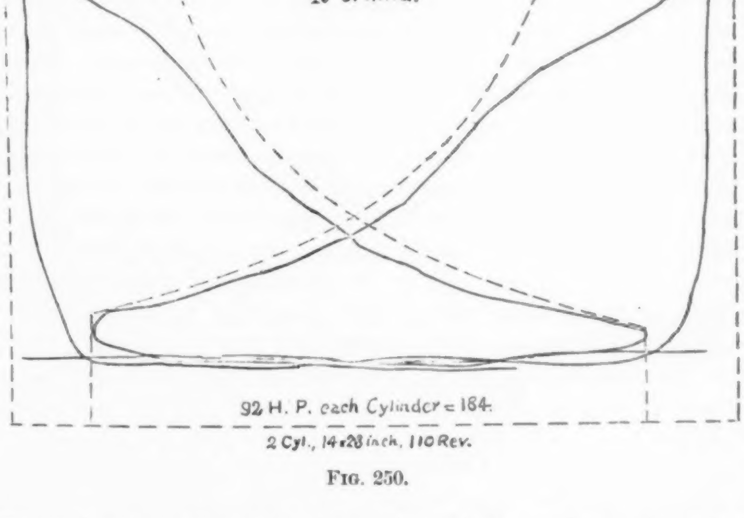


Fig. 250.

These facts, together with inspection of the cards—each one of which shows a sharp limit at exactly 85 lbs. pressure—indicate, with almost the clearness of mathematical demonstration, that something was the matter with the instrument used; that the spring of the indicator was very much lighter than it purported to be, and was provided with a stop which limited its movement to $2\frac{1}{8}$ inches from its normal position. This consideration utterly vitiates the test and all the conclusions drawn from it, as the horse-power developed must have been much less than estimated, and might have been any amount below that stated. This is also demonstrated by the fact that the load upon the engines was 138

Thompson & Houston arc lights. Now according to *Thompson's Dynamo-Electric Machinery*, p. 200, a "2,000 candle-power" Thompson & Houston arc lamp requires from 45 to 46 *volts*, E. M. F., which at 9.6 *amperes* (the current stated) gives 437 *watts* or .585 H. P. per lamp, or 80.8 horse-power for the 138 lamps used. Add 20 H. P. for engines and shafting and we have, say, 100 H. P. as the probable load.

The report assumes to prove that the triple thermic motor "actually converted into power 17.8 per cent. of the total heat which passed through the engine." This statement is repeated three times, and seems to be viewed as settling the question of the great value of the CS_2 vapor. Now this is within less than one-half of one per cent. of the highest possible results attainable in a perfect engine with no losses whatever, working at that range of temperature. The expansion curves which I have placed upon the cards show conclusively that no such approximation could possibly occur. These have been calculated for the CS_2 vapor, and on the scale of 40 as given for the cards. As this is, doubtless, too large a scale, the proper curve would show even greater losses in the development of the power. These curves also show that the assumption that one-third or any other large proportion of the heat passed through the sides of the cylinder from the steam-jacket to the vapor, or that this, or any other cause, increased the average pressure within the cylinder above that due to the ordinary action of expansion, is wholly without foundation.

In his discussion, Mr. Crane referred to the investigations of Messrs. Gantt and Maury as showing a great gain for bisulphide-of-carbon vapor, and the report referred to makes the statement that these investigations "demonstrated that, under conditions suitable to the CS_2 vapor, the latter might generate 60 per cent. more power than steam with the use of a cylinder having only $\frac{55}{100}$ the capacity of the assumed steam-cylinder. The same experimenters admitted that the only considerations counteracting this immense gain in the use of CS_2 vapor were those arising from the possible danger from the inflammable character of the fluid, and its odor and corrosive action upon iron when exposed alternately to the vapor and air." It needs only a reference to the discussion in question (*Van Nostrand's Magazine*, vol. 31, pp. 431, 432), to see the disingenuousness of this statement and to refute it completely. The "conditions suitable" were an initial pressure of the CS_2 vapor of 254.9 lbs. as against 120.2 for steam, and the facts were

that with this enormous advantage in pressure the CS_2 vapor developed 60 per cent. more power *per cubic meter of vapor* at initial pressure, with nearly nine times as much weight as of the steam, or the *same power* in a cylinder of 55 per cent. capacity. It will be noticed that these data have nothing to do with the economy. Under these circumstances Gantt and Maury say: "The vapor of bisulphide of carbon gives a *gain in efficiency of* 3.71 *per cent.* They also say (italics as given): "*if we limit the maximum pressure to that employed in the steam-engine, steam is the most efficient fluid we can use.* The relative size of cylinder necessary to produce the same power is smaller for steam than it is for non-aqueous vapors when all have the same initial pressure."

Apropos of the reference in the paper to the production of electricity directly from heat, a step in that direction seems to have been made by Mr. Willard E. Case, of Auburn, N. Y., to whom a patent has been issued since the meeting for a thermal battery, consisting of metallic tin on a plate of carbon for one electrode, and a plate of carbon surrounded by porous terra-cotta for the other, the battery being filled with a mixture of alcohol and hydrochloric and chromic acids. When heat is applied to this battery it gives off a current of electricity until all the tin is converted into protochloride. On being allowed to cool, the chemical action is reversed and the battery restored to its original condition. This alternate action is said to be capable of being carried on indefinitely; but no facts are yet available as to the proportion of the heat expended which is converted into electricity by the apparatus.

CCXXI.

THE TRAINING OF A DYNAMIC ENGINEER IN WASHINGTON UNIVERSITY, ST. LOUIS.

BY CALVIN M. WOODWARD, ST. LOUIS, MO.

THE complete course of training supplied by this university consists of three distinct stages, which will be briefly presented in succession.

It is proper that an account of these stages should be prefaced with the statement that as yet no students have passed through the third stage. It is but recently organized, and next year will be the first in which all will be in simultaneous operation. We logically began at the bottom and have now reached the topmost story. The first and second stages have been tested and their value confirmed by successful experience. Of the value of the final stage, we have no question.

The primary course of instruction is given in the Manual Training School, occupying three years, from the age of 15 to 18 on the average. The second stage covers four years of undergraduate instruction in the Polytechnic School, from 18 to 22, at the end of which the successful student receives the non-professional degree of "Bachelor of Engineering." The final stage covers one year of what may be called graduate study and investigation, the completion of which entitles the student to the degree of "Dynamic Engineer."

The Manual Training School was established as a distinct and separate preparatory department in 1879.

Our Polytechnic School was organized sixteen years ago. At first the departments of Civil and Mechanical Engineering were combined in one. Three years ago they were separated and the broader and more appropriate name of Dynamic Engineering was adopted. The name is respectfully commended to the consideration of this society. In England and largely in this country, a mechanical engineer is *first* of all a machinist; *secondly*, a draughtsman; and *thirdly*, he is more or less (generally rather *less*) familiar

with mathematics and theoretical mechanics. A dynamic engineer should be thoroughly grounded in all—in the theory as well as in the practice of the great prime movers which serve to develop the *forces* of nature.

I. THE MANUAL TRAINING SCHOOL.

This school gives systematic instruction in mathematics, science, language and literature, drawing and shop-practice. All of these five subjects enter into the programme of every day for every boy, two hours of school time being devoted to the shop, and one hour to each of the other four subjects. The school year consists of about one hundred and ninety days net; the home study varies from two to three hours daily, being the greatest with the highest class. The course in mathematics is not unlike that in the high-schools and academies which prepare boys for college. Arithmetic, algebra, geometry, mensuration, and some plane trigonometry. In science or applied mathematics, comes a science primer, physical geography, botany, elementary physics (including the construction and use of simple apparatus and the determination of laws inductively), chemistry (with very little laboratory practice, though we hope gradually to introduce more), physiology and book-keeping.

The literary work consists of history, rhetoric, Latin, French, and English classics. Those boys who do not propose to take a higher course in a college or polytechnic school, may omit Latin and French, giving the time to more history, English composition, and political economy.

DRAWING.

Penmanship and lettering come under the head of drawing, which is practical rather than artistic in its aim, in view of the principle that the artistic should always follow the practical. The former is intricate, requiring maturity and familiarity with elementary principles; the latter is simple, plain and intelligible to every boy of fifteen years, and so far as known, it has never been shown before what boys of fourteen and fifteen are capable of in the art of drawing. We begin with orthographic projections of simple objects, generally modifications of geometric forms, requiring three views consistently drawn. This is all free-hand work on blackboard, or pencil work on paper. This training enables the boys to make and read their shop-drawings from the very first. Their first instrumental work is on stretched paper with ink, and consists in learning to draw straight and curved lines in ink, clear,

firm and true. Free-hand sketches of simple articles which have details of simple shape, such as speed lathes, center rests, face plates, etc., are next followed by instrumental drawings of the same to exact scale, accompanied by a sheet of "figured" details and sections.

The drawing of the second year is all on stretched paper, and largely instrumental or brush work. Representations of blocks, plain, truncated or intersecting, drawn in strict orthographics; flat tinting, isometrics, lettering, borders; a machine (from the object) with details; architectural details and ornament.

The third year drawing begins with two sheets of geometrical exercises, a sheet of line shading with shades and shadows; a sheet of brush shading of cones, cylinders, spheres, toruses, etc. The final exercise is the drawing from the object by actual measurement of a large engine, machine, or structure. This is first sketched and measured, then drawn and shaded with a brush. This drawing is as finished as the boy can make it, and shows the result of the course. A tracing on cloth is taken of the outline work. Nowhere in this course do we teach linear perspective or descriptive geometry. The very simple exercises in intersections, screws, developments, and shadows are regarded only as geometrical exercises.

Throughout the drawing course, the character of the shop work going on at the same time, and its accompanying working drawing, is kept steadily in view.

SHOP WORK.

The shop practice extends over a very wide field, but like the drawing which runs parallel with it, it is all required of every boy in the school, no matter what his plans for the future may be. It occupies two hours a day for five days each week.

The 380 hours of the first year are devoted to wood, at the bench and at the lathe. Joinery, with wood-carving, gluing, inside and outside turning, forms of beauty and forms of strength, constitute the series. The year ends with the construction of an article, original or copied, which shall embody as many of the steps already learned as possible. Incidentally the pupils keep up the stock of handles, mallets, clamps, trestles, and shelving in the establishment, though the great majority of exercises are of a purely abstract character.

The size of a shop division is limited to twenty-four boys, under

the charge of a single teacher, and the daily lesson is uniform for the division. A working drawing of the piece or model required is first made and explained by the teacher of the division. Every boy copies the drawing in his special book, and henceforth works from the drawing. The piece is then executed by the teacher in the presence of the class. Attention is called to the order in which the steps are taken, what tools are used, and how new processes are combined with old ones. The boys then execute the task, each for himself, with or without special direction or help from the teacher. Boys who work rapidly and well, put their spare time after finishing their exercises into "extras," which generally combine the steps already learned, in some article of use or beauty. The slowest boy generally hands in an unfinished piece. The results are criticised, compared, and graded on an absolute scale where 100 per cent. means reasonable perfection.

The aim is to master the range of every tool, and to cultivate the habit of analyzing complicated processes into simple elements. A high degree of skill is not aimed at, the chief immediate object being an intelligent mastery of every step and every tool.

By a similar method forging is learned during the middle year. The elementary processes of the forge are learned one at a time, with just enough practice to fix them indelibly on the mind and to secure a moderate degree of skill. We have found it extremely useful in giving exact knowledge of forms, and in teaching how to strike and how to hold pieces under the hammer, to use bars of cold lead in a preliminary exercise. The time apparently lost on a lead exercise is more than made good by the material and time saved in the subsequent forgings of iron and steel. The necessity of keeping up the supply of forging tools, and of the construction of a set of lathe tools, cold chisel and steel dog, gives all the variety necessary for a course of mere instruction. The size of a working division during the second year is reduced to twenty-two; hence the shop contains but twenty-two forges, anvils, and sets of tools. A total of only 285 hours is given to the forging shop. The remaining 95 hours of the second year are given to pattern-making, moulding, castings (with plaster or lead), brazing and soldering. In connection with soldering comes practice in cutting sheet metal for special shapes, and spinning. This work is done in strict connection with their drawing of intersections and the developments of surfaces.

The shop practice of the third year is in the Machine and Fitting

Shop. The maximum size of a working division is here reduced to twenty, and yet it has been found impossible to adhere strictly to uniform lessons, for the reason that it is practically out of the question to furnish twenty complete sets of machine tools. We have found our wants fairly met by twelve engine lathes, four speed lathes, two drills, two planers, and twelve vises. As one man is always detailed to keep the tool shop, nineteen are to be kept at work at once. Nevertheless, a large degree of uniformity is secured by means of systematic class instruction on the different tools, and then systematic rotation in the exercises. In the use of the planers and drills, a boy is first learner and then teacher. The series of exercises which we use are the results of large experience in devising such work as shall prove most instructive, and best serve to develop the full capacity of every hand and machine tool. The exercises occupy fully four-fifths of the year, and include the use of every tool in the shop.

The last few weeks are devoted to construction. In some cases new patterns are constructed, in others old patterns, made during the second year, are used, and from the castings (made elsewhere) articles of some complexity and real utility are constructed. During the present year the senior class is engaged in the construction of three upright engines, several jack screws, an emery grinder, and several pieces of brass work. The abstract exercises, however, covered the shop work from the first of September to the middle of April. These engines and other articles are not made with any view to an income. Our purpose in their construction is to give the students themselves an opportunity to see how fully their exercises have prepared them for such constructive work; and on the other hand, to teach them that, no matter how comprehensive their experience may be, a new article may involve new problems which can be solved only by thoughtful study and the exercise of good judgment.

As to our policy of not carrying on a commercial establishment; of taking no contracts; and of not setting out to manufacture for any market—reference will be made later on. It may be now said that we have found our present system of uniform exercises: 1. More fruitful in general skill; 2. Better adapted for teaching method and precision; 3. More economical as admitting of a larger number of students simultaneously under one instructor.

Such, then, are the chief features of the Manual Training School. It was not established, nor is it conducted, as a school for the pri-

mary training of mechanical engineers alone. It is a school for general training. It is assumed that pupils entering its junior class are too young and undeveloped to decide the all-important question: —what occupation or career in life shall he select. By the end of its three-years' course, however, the bent or natural aptitude of a boy is generally found, if he has one. If he combines a love for practical work with strong mathematical power, then he has the prerequisite of an engineer. Thus far those who have entered the Polytechnic School as students in the course of dynamic engineering have had good reasons for their selection.

II. THE UNDERGRADUATE COURSE IN THE POLYTECHNIC SCHOOL.

During the freshman and sophomore years, the course is in common with that of the civil and mining engineers. Higher algebra, and geometry, trigonometry, and analytical geometry in pure mathematics, descriptive geometry, with its applications to spherical projections, shades, shadows, perspective and masonry; physics, theoretical and practical; chemistry, theoretical and practical; surveying, modern languages, modern literature; constitutional government; mineralogy; elocution; free hand and higher mechanical drawing. As compared with other technical schools, our course is marked as particularly full and thorough in descriptive geometry and its applications.

It has long appeared to the writer that educators have failed to appreciate either the disciplinary or the practical value of descriptive geometry. In his judgment, it has no equal as an aid to the geometrical imagination, a quality invaluable to an engineer. Then, again, the great value of its clear and direct methods is shown when they are used to illustrate the parallel and analytical propositions. When a student realizes that every problem in the geometry of space has two solutions, one analytic, the other graphic; and when he is sufficiently familiar with both to use one as a check upon the other, then and then only has he the proper command of the mathematical tools of engineering.

At the beginning of the third year of the Polytechnic, the students enter upon their professional studies in separate divisions. The dynamic engineers are by themselves in the study of electricity, machinery, and mill-work, in which they regularly have five exercises per week. In common with the other students, they take up the calculus and applied mechanics. Four exercises per

week for the entire year are not too much for a working knowledge of the differential and integral calculus.

Two exercises a week for the junior year, and four per week for the senior year, are given to graphical statics and Rankine's applied mechanics, in connection with a large collection of illustrative examples. During the senior year Rankine's Steam Engine and other Prime Movers is thoroughly read and illustrated.

In connection with electricity and mill-work and the prime movers come both drawing and laboratory work.

Throughout the junior and senior years there is systematic work in the laboratory of dynamic engineering. By actual experiments the students become familiar with the strength and elasticity of engineering materials, and the power of motors, as determined by a variety of dynamometers. A great deal of attention is paid to the question of the efficiency of machines.

In common with the other engineers, the dynamic students take political economy and the study of English style during the senior year.

Throughout the course, in all subjects which admit of such treatment, graphical and analytical methods are employed side by side.

We have found the facility with which students can construct apparatus at the shop a great help in all their experimental work. In designing and planning new pieces, a knowledge of moulding, forging, and machine tool work is simply invaluable.

So far as possible, we rely upon knowledge at first hand, upon primitive judgment, from contact with materials and forms, instead of getting ideas from books and verbal descriptions. The solution of ideal problems and the planning of works, under purely ideal conditions, is comparatively an easy matter. The difficulties lie in expressing *real* conditions in mathematical language, and in giving to each element of the *real* problem its just weight of influence. The writers of books have too generally confined themselves to ideal problems; and, though they are careful to say that their results are only approximately true for real problems, they give no hint as to the degree of approximation. It is not till a student has tried his hand at putting in all the conditions, with their proper weight, that he *realizes* the full meaning of the word approximation. No student can need this training more than the dynamic engineer. In *static* engineering the conditions are far more simple. It is only when we deal with *unbalanced* forces and *moving*

bodies that we encounter the greatest difficulties. It is for these reasons that we attach great value to our laboratory work in physics and applied mechanics.

In the previou course the students are made familiar with the manipulation of the engine. In this course the discussions upon the making and using of steam are illustrated by the examination and frequent tests of furnaces and engines in and near St. Louis.

If a student did not receive his preliminary training at a Manual Training School, he is required to spend four hours per week, for the whole four years, in the shops. If the shop training came in the preparatory course, the laboratory work of the freshman and sophomore years is devoted to drawing, physics and chemistry.

The shop exercises of undergraduate students differ in no important respect from those of the Manual boys. They are older and of course make more progress in the same number of hours.

III. THE GRADUATE YEAR IN THE POLYTECHNIC SCHOOL.

The required work of the additional year, which is intended to render one worthy of public confidence as a dynamic engineer, is partly theoretical in the realms of heat and electricity, and partly practical in connection with further use of apparatus, and in critical examinations and analytical reports of existing mechanical plants. Such reports take the form of professional papers, and will be subject to the severest criticism on the part of the professors. Here, most appropriately comes in examination of prices, and estimates of cost of material, workmanship, and labor.

The laboratory work will take the form of research, instead of being chiefly illustrative, as during the undergraduate stage of training. The character and equipment of the "Laboratory of Applied Mechanics and Dynamic Engineering," which it is proposed to construct during the present year, is outlined in the following sketch from a report recently adopted by the Faculty:

3. The Department of Dynamics can never be what it ought to be without a suitable Laboratory of Dynamic Engineering. Such a laboratory should contain a large variety of apparatus for testing and illustrating the principles of the mechanics of engineering. For the first time in the history of the Polytechnic School, we are in a condition to appreciate and use such a laboratory. More than three-fourths of the students in dynamic engineering next year will be graduates

of the Manual Training School, and, therefore, familiar with the use of tools and machinery. The time which in the past students have given to shop-work, should in their case be given to work in the proposed laboratory.

The following is a partial list of the apparatus needed in the proposed laboratory:

(*a*.) Machines for testing the strength, elasticity, and durability of the materials of engineering, such as wood, bricks, stones, cements, iron, steel, and alloys.

(*b*.) Dynamometers, such as springs, brakes, gears, indicators, etc., for determining the power of a motor, or the energy transmitted, or that used by a particular machine or process.

(*c*.) Water-wheels and turbines, in connection with tanks and dynamometers.

(*d*.) Pumps, cylindrical and centrifugal, for liquids and for air, in connection with tanks and dynamometers.

(*e*.) A high-speed steam engine with a steam jacket and a condenser.

(*f*.) Special apparatus for testing lubricants and the coefficients of friction.

(*g*.) Calorimeters of various kinds for determining the specific heats of solids, liquids, and gases; also for determining the vaporizing heats of liquids in common use.

(*h*.) Electric dynamos and motors, and electrical plant in general.

Much of this apparatus we already have, in one shape and another, scattered about in different buildings.

In common with the students in civil and in mining engineering, the dynamic students will make a thorough study of thermodynamics, and engage in such practical applications of the theory as can be conducted in the laboratory or found at accessible points outside.

A certain amount of elective work will be offered during the fifth year, which it is hoped will prove very valuable. There will be opportunity for an extended study of the metallurgy of iron and steel, with a view of determining their engineering value from chemical tests and a knowledge of constituents and the methods of manufacture. Exercises in designing machines to perform specified work, and in planning various mechanical establishments under given conditions, extend through the year.

One of the chief features of the graduate year will consist in the preparation of the professional thesis. In the past, the students have been compelled to attempt the preparation of a thesis before the principles, which ought to be employed, have been sufficiently mastered to render their intelligent use reasonable. Not till the time when the thesis was to be finished did the student feel prepared to begin it. As a consequence of this change, of requiring only a literary or scientific essay on the reception of the degree of "Bachelor," and of the postponement of the thesis till the following year, we expect a much higher grade of work, and occasionally

some really valuable discussions. By thus reducing much of our work to the plane of professional life, we hope to shorten the probationary period of an engineer.

Having thus briefly outlined the three stages of our methods of training dynamic engineers, no defence of its character will be entered into. The Manual Training School stands in need of the Polytechnic to supplement its work, as truly as the Polytechnic stands in need of the Manual to properly prepare students for its ministrations. Purely manual work is elementary in character; it is only the close reasoning about such work that requires maturity, and only those who have tried our plan, can know how helpful it is to the polytechnic student to be familiar with the manipulations of practical mechanics.

All that was said a year ago by Prof. Alden, in regard to the wholesome intellectual effect of combining theory and practice, is cordially endorsed by the writer. Even on the intellectual side there is no waste of time, in either the earlier or the later stages of our training. The reasonableness of every step in the course is evident to every one who enters upon it. On two points only need more be said.

THE POLICY OF THE SHOP.

One point is the old one of the policy of the shops in which the students receive their manual or tool instruction and practice. It must be borne in mind that our shop practice comes as a rule during the preparatory stage, while the student is in the Manual Training School. We may, therefore, still speak of the students as boys.

Prof. Alden believes in a commercial shop where real business is done, and where commercial standards are used. He admits that "such a plan would not have been developed as the outgrowth of a school," and says it was made a necessary condition of the acceptance of the donation for the establishment of the shop. Nevertheless, he appears to regard it as the best means for securing the end sought, viz., the education and training of the students in practical mechanics. According to Prof. Alden, the question is, "whether shop shall, *first*, be a place where business is done, in order that there may be something practical for the students to learn, or whether it shall be a place fitted with tools, where only their use and the processes of shop practice are taught." He decides for the former; here we have decided for the latter.

The *first* thing to do in the shops of a school is to teach the use of tools and the processes of the arts; the question of what shall be done with the incidental products is a *secondary* matter. Our exercises are so designed that their execution shall be as instructive as possible, and not at all with a view to sale. We cannot afford to fill orders; the moment a boy is fit to fill an order involving only old exercises, he must turn his attention to new ones. We aim to put but one article upon the market, viz., *boys*.

Not that we hold, as Prof. Alden appears to think, that the sale of an article produced as an exercise "would in some way render the practice unfit to be associated with a school." We make no attempt to sell their drawings, their surveys, their English essays, their physical apparatus, or their chemical analyses; so we do not aim to sell their shop work. I think the policy of deliberately manufacturing for the market is unwise or mischievous in three ways:

1. The orders which the superintendent can get, and the sequence in which he gets them, are greatly inferior in the opportunities they offer for logical treatment and fullness of instruction to the *orders which he is capable of designing.*

2. The pecuniary risk involved in the execution of a delicate operation on a large or complicated article, is liable to lead the skilled instructor to do with his own hands in every case, what each student should have a chance to practice upon for himself.

3. In spite of all efforts to the contrary, the filling of actual orders is sure to involve not only a dearth of the most instructive processes, but an excess of the simpler steps, continued practice in which ceases to be of any subjective value, and which therefore results in a waste of time and loss of interest. [It must be borne steadily in mind that I am not speaking of learning a trade; I am speaking of the training of an engineer.]

All of the shop teachers were trained in business shops, one of them at the Worcester Institute, and yet after several years of experience in which they have combined exercises of their own design, with the execution of projects more or less complicated and quite analogous to outside orders, they are more and more in favor of their own exercises for the purpose of instruction.

As regards the interest which students take in their work, we have found no lack of it in judicious exercises. I have heard of a lack of interest upon a chipping and filing exercise which lasted

several weeks, and which was generally left unfinished. At the same time we have no objection to putting to actual use such of our exercises as will admit of it. During the past year, every student of the graduating class of the Manual Training School has been required to make, as a lathe exercise, three small and three large bolts with their nuts. Now the first bolt finished was likely to be poor; the last in each set was likely to be good. Such being the case, we had no wish for the class to make more. Our object was secured. We could not stop to make more, even to fill an order. With a full knowledge of all the facts, a St. Louis firm, of whom we bought iron and steel, offered to let us have all the material we needed for this exercise if we would let them have the finished bolts when we were through with them. This offer we accepted purely in the interest of economy.

THE STUDY OF MONEY VALUES.

Secondly, I wish to consider an argument offered by the superintendent of the "Miller Manual Labor School" in Virginia. He says in his catalogue of 1885: "We consider it part of the instruction of the shop to teach boys the value of labor, the increased value of skilled labor, and the still greater value of an educated mind guiding a trained hand." And again: "We feel sure that no course of shop instruction will be complete that does not take cognizance of the value of material and the value of labor." It is probable that by "value" is meant money value.

This sounds well, and the object aimed at is worthy, but I doubt their success in this direction. A learner can get no correct idea of the money value of his time or of the education he is getting. His time is well spent in learning, even if he spends six hours in doing what an expert would do in less than one. Take mechanical drawing, for instance: A boy at school makes but one good drawing of a kind. He knows how long it took him to do it, but he does not know how long it would take him to duplicate it; much less does he know what an experienced draughtsman can do. Speed comes with long practice, which a school ought not to try to give. It is the same with shop work.

As to his making a just comparison between skilled and unskilled labor, and between an ignorant and an educated workman, it is clearly out of the question. Only long experience in employing and directing workmen of all grades of intelligence and skill, gives

opportunity for reliable judgment on these points. Of course our boys *feel* the difference between knowing and not knowing, between thoughtfulness and thoughtlessness; but the money value of that difference is beyond their horizon. So in their study of political economy they get ideas about wages, and the value of skill, both mental and manual, but such ideas cannot be called knowledge until confirmed by personal experience in the real work of life.

Neither do I think much is to be gained in discussing the cost of materials. Economy may be taught even if the material costs nothing. We can teach intrinsic values without meddling with market values. The former are permanent, the latter fluctuating.*

It is only during our third stage of training, in the fifth or graduate year, when the would-be engineer is preparing directly for the responsibilities of professional life, that a systematic consideration of market values finds appropriate place.

In conclusion, let it be said that there are many things which cannot be taught or learned at school. A West Point cadet cannot be drilled in the presence of flying bullets and bursting shells; though exercise under such conditions is the "real business," the "something practical," which the real soldier must some time learn. The law student argues before a "moot" court; it is only the lawyer who engages in real business before a real court. So the medical student amputates and dissects dead men, leaving living people to those who, worthily or unworthily, have received their diplomas.

In like manner, while an engineering school can successfully teach and train students in the details of shop work, as a matter of applied mechanics and practical mechanism, we shall not wisely undertake to train them in the actual transaction of business. Such training lies outside the walls of even an engineering school; and any attempt to bring it in is sure, in my opinion, to result in deep-seated errors, in false estimates, and in the diminished regard for those intrinsic values, those immutable laws, and those permanent factors which are of universal use, which most reward careful study, and which form the only secure basis for good engineering.

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* Learning market values is like committing to memory the population of towns in geography. I can remember that as a boy I learned the useful fact that St. Louis had 25,000 inhabitants!

APPENDIX VII.

MANUAL TRAINING SCHOOL.

JUNIOR CLASS.

Arithmetic, completed. *Algebra*, to Equations.
English Language, its Structure and Use. Study of Selected Pieces. *History* of the United States.
Latin, Grammar and Reader may be taken in place of *English.*
Huxley's Introduction to Science. Physical Geography. Botany.
Drawing, Mechanical and Free-hand. *Penmanship.*
Carpentry and Joinery. Wood-carving. Wood-turning. Pattern-making.

MIDDLE CLASS.

Algebra, through Quadratics. *Geometry* begun.
Natural Philosophy. Experimental work in the Physical Laboratory. Principles of Mechanics.
English Composition and Literature. Rhetoric. English History.
Latin [Cæsar] may be taken in place of *English and History.*
Drawing, Line-shading and Tinting, Machines. *Development of Surfaces, Free-hand Detail Drawing. Isometric Projections.*
Forging—Drawing, Upsetting, Bending, Punching, Welding, Tempering, Moulding, Casting, Soldering and Brazing.

SENIOR CLASS.

Geometry, continued. *Plane Trigonometry. Mensuration.*
English Composition and Literature. History. Ethics and Political Economy.
French may be taken in place of *English and History*, or in place of the Science of Study.
Physiology. Elements of Chemistry. Students who have taken Latin, and who intend to enter the Polytechnic School after completing the course in this school, will take History in the place of Physiology and Chemistry.
Book-Keeping.
Drawing, Machine and Architectural.
Work in the Machine Shop. Bench Work and Fitting, Turning, Drilling, Planing, Screw-cutting, etc. *Study of the Steam Engine.*
Execution of Mechanical Project.

POLYTECHNIC SCHOOL.

COURSE IN DYNAMIC ENGINEERING.

FRESHMAN CLASS.

FIRST TERM.

Mathematics—Chauvenet's Geometry complete.
Physics—Pneumatics, Acoustics, Heat.
French or German—Elementary or advanced.
History—United States Constitution.

Free-hand Drawing.

Mechanical Drawing—From Flats and Models. Use of Scales, Tracing, and Construction of Plates, etc.

English—Elocution and Composition; Rhetoric.

Descriptive Geometry—Orthographic Projections, Problems of Points, Lines and Planes, Curves.

Shop Work—Use of Carpenter's Tools.

Ethics—Lectures.

SECOND TERM.

Mathematics—Plane and Spherical Trigonometry.

Physics—Heat continued, Optics.

French or German—Grammar and Reader continued.

Free-hand Drawing—Continued as First Term.

Descriptive Geometry—Tangency and Intersections.

History—England.

English—Elocution and Composition; Shakespeare.

Shop Work—As First Term.

SOPHOMORE CLASS.

FIRST TERM.

Mathematics—Higher Algebra.

Physics—Electricity, Magnetism and Meteorology.

Descriptive Geometry—Spherical and Isometric Projections; Shades and Shadows; Perspective.

Theoretical Chemistry—Roscoe's, with Lectures.

Free-hand Drawing—Machinery, Casts, etc.

Mechanical Drawing—Line and Brush Shading; Lettering.

French or German—Reading prose writers.

Shop Work—Turning of Wood.

SECOND TERM.

Mathematics—Analytical Geometry.

Surveying—Land, Topographical, Hydrographical and Geodetic Surveying. The use of instruments: Compass, Transit, Level, and Stadia in the Field.

Practical Chemistry—Qualitative Analysis.

Mineralogy and Geology—Lectures.

Free-hand and Mechanical Drawing—Machinery, Architecture, and Topography.

Physics—Theoretical and Practical Work.

English—Modern Literature (Lectures).

Shop Work—Pattern-making.

VACATION WORK.

Surveying and the preparation for a full Report upon an *Engineering Method.*

JUNIOR YEAR.

FIRST TERM.

Mathematics—Differential Calculus and Applications.

Stereotomy—Applications to Masonry, Carpentry, and Machinery.

Mechanical Engineering—Rankine's Applied Mechanics, Part II.; Principles of Mechanism; Rankine's Machinery and Mill Work; Lectures on Steam Engineering.

Mechanics—Graphical and Analytical Statics.

Electricity and Magnetism—Theoretical.

Practical Chemistry—Qualitative Analysis.

Shop Work—Testing Strength of Wood and Iron; Working the Blacksmith Shop: Practical Stone-cutting; Construction of Arches, etc.

Drawing—Machinery and Models.

SECOND TERM.

Mathematics—Integral Calculus; Analytical Mechanics.

Mechanical Engineering—Rankine's Applied Mechanics; Designing Valve-gearing; Steam Engines and Boilers; Workshop Appliances; Visits to Engineering Works, with Notes and Sketches; Rankine's Machinery and Mill Work.

Mechanics—Strength and Stiffness. Kinematics; The Principles of Mechanism.

Electricity and Magnetism—Dynamo-Electrical Machinery.

Drawing—Machine Drawing, continued.

Shop Work—Welding and Tempering; Testing Strength of Materials.

VACATION WORK.

A Descriptive Report of a Prime Mover and its Work.

SENIOR YEAR.

FIRST TERM.

Mechanical Engineering—Analytical Study of Different Machines; Steam Engines; Water Wheels; Water Engines and Pumping Machinery, Efficiency of Machinery, Steam Boilers and Furnaces; The Theory of Combustion; Theoretical Electricity.

Mechanics—Rankine's Applied Mechanics continued; Stress Kinematics.

Drawing—Of Working Machinery.

Astronomy—Newcomb and Holden's Astronomy.

Shop Work—Chipping and Filing; Machine Work; Study of Engine and Boilers.

English—Study of Style.

SECOND TERM.

Mechanical Engineering—Designing and Computing the Parts of Machines required to perform Particular Work; Construction and Management of Engines; Rankine's Steam Engine.

Drawing—Of Machinery of Original Design.

Laboratory Work—Experiments and Researches in the Dynamic Laboratory.

Mechanics—Theoretic Study of the Prime Movers; Water Wheels and Engines; Theory of Mechanism; Dynamics.

Political Economy—Bowen, Mills, Rogers, Cary. (Recitations and Lectures.)

English—Study of Style.

Shop Work—Machine and Hand Fitting.

Thesis—For the Degree of "Bachelor of Engineering."

THE FIFTH YEAR COURSE.

A full professional degree will be conferred only after an additional year's successful study in the line of the work already done during the four years of the Polytechnic course. The requirements of the fifth year are peculiarly liable to change, but for the year 1886–7, they will be substantially as follows :

Thermodynamics continued.
Electric Plants.
Elasticity, Resistance, Fatigue of Engineering Materials.
Various methods of transmitting power ; wire ropes, steam, water, compressed air, and electricity ; their relative economy and advantages.
Engine and Boiler Tests ; Experimental Methods.
Specifications and Contracts.
The preparation of Designs, Abstracts and Reports upon certain classes of machines, obtaining the *data* from professional papers or by observation. The reports will include estimates of cost of construction and operation.
The Metallurgy of Iron and Steel.
The Theory of Least Squares.
The Preparation of a Professional Thesis for the Degree of Dynamic Engineer.

ELECTIVE STUDIES FOR ALL THE COURSES.

Courses of lectures will be given on the following subjects, provided a sufficient number of applications are made by members of the class :

Quaternions.
Higher Geodesy.
Theoretical and Practical Astronomy.
The Analytic Geometry of Space.
River Hydraulics.

The following subjects are included in the work of the first four years. They are open to those whose course of study has not already included them :
Chemistry.
Pumps and Pumping Machinery.
Tunneling.
Heating and Ventilation.

DISCUSSION.

Prof. R. H. Thurston.—I have been reading Dr. Woodward's very interesting paper with attention, and am very much pleased with the systematic way in which the work is evidently done at the Washington University. I am pleased to see that a considerable amount of practice in free-hand drawing is there insisted on, and that it includes some final work in machine sketching. This training gives the boys the best possible exercise in the movements of hand and arm, correlated with the action of the eye. The use of the sketches so made, in the succeeding work with drawing instruments, illustrates a principle which a carefully planned course may illus-

trate in a great many ways—that of combining the practice of the moment with the preparation of work for a later period in the course. It will be often found thus practicable to "kill two birds with one stone," and I know of no more important factor in the successful operation of any extended system than this. The order and succession of exercises are admirable.

I am very similarly impressed with the systematic plan and working of the shop instruction. I particularly like the system of introducing each exercise with a lecture—if so unpretentious a talk as is necessary in this case may be so denominated—in which the nature of the exercise is explained, and the method of its accomplishment; the tools to be used are indicated; their form, method of use, special characteristics stated; the piece to be made being exhibited, the best way of using the tool is explained, and the execution of the work is illustrated by the instructor. I do not think it possible to attain the desired result promptly and satisfactorily with classes and to secure rapid progress, in any other way nearly so well as by this method of reaching every student at once, and thus leaving a minimum of time to be expended by the instructor on individuals who happen to be slow or inattentive. With boys who have the real spirit and knack of the mechanic in them—and no others should ever be allowed to attempt to enter the profession of engineering, in view of their inevitable failure—it is marvelous to see how rapidly they acquire the power of skillfully using tools. I find many a youngster who had never used a tool before in his life, other than his jack-knife, after a few weeks doing such work as his instructor, not to say the average journeyman, may well admire. His muscles and his nervous system are in a stage of growth when they can be made to accept this systematic training of every fiber of both, and when they are by nature best prepared to acquire the habits and to gain the sleight that is characteristic of the naturally good mechanic. If a boy does not show that he has the essential proclivities in childhood, it may be usually assumed with safety that he is not of the elect, and he is not likely to prove, in later years, a good mechanic or a great engineer. I do not mean to say, however, that I regard the working into its best possible shape of such material as this latter as useless or objectionable. I believe that nineteen boys out of twenty do possess more or less of the mechanic's tastes and powers, and that the other one out of the twenty will be so benefited, and his usefulness to himself and the world so increased, by shop instruction, that he will do well to secure it. But,

in the work of life a man must do that for which he is best fitted, and he cannot hope to succeed in competition with the world if he attempts to make a livelihood and to carry on a business for which he is not fitted. The turtle may be an admirable diver, but he cannot hope to succeed in the race with the hare—if the hare attends to his business.

It is claimed for the system of general exercises, such as has been described, that it secures fruitful application of talent in the acquirement of general skill; that it is especially well adapted for conferring upon the student habits of method and precision; and that it is peculiarly well adapted for instructing classes, in which the work must be done by the least possible number of instructors. I believe these claims to be all perfectly correct. Once that knowledge and skill are acquired, the student is ready to turn his attention to their application in the arts and trades of whatever department he may choose to enter. He will succeed in any trade, or will progress toward success in any department of engineering, *provided* he have, in addition to the skill of the mechanic, the intellectual and moral character and force essential to advancement in any and every walk in life. Without the latter, all the training that all the trade schools and schools of engineering in the world could give him would be useless.

The arrangement of the work in the lecture room courses, during the period of attendance in the polytechnic school, seems to be excellent. I am a little puzzled by their variety and apparent extent. I have never been able to get such heavy work as is usually demanded of the advanced student in engineering into the four years of a technical course, and at the same time find time for good work in eight subjects in one term. I think it is usually found that some very desirable, and even what are generally thought very important branches, must be sacrificed, in arranging such a course, in order to secure the essentials of professional work. The technical course should be made a superior course, to be pursued, as in law, in medicine, or in the divinity school, after an earlier general, more or less liberal, and largely "gymnastic," education of the ordinary type. Could all students secure a liberal education before entering upon the technical and professional studies of the polytechnic school, it would be of enormous advantage to them, and to the world about them. Since this cannot be done in the majority of cases, it seems to be the next best thing, at least, to adjust the course in the latter school to the most imperative neces-

sities of the student, making so much sacrifice, or rather so little sacrifice, as may be found to meet the case best. I should be glad to learn that literary studies can be carried satisfactorily through a course in which good work is done in pure and applied science, and especially in the advanced work of a professional course in engineering. If it should prove thoroughly practicable, I shall certainly become decidedly less conservative than at present, both in views and in practice.

The question whether the shop shall be a business establishment as well as a school, or simply the latter, is a very interesting one, and one which can never be determined by debate or individual opinion, but must be settled by experience. For my own part, I have watched the progress of the two systems very carefully, from the beginning, in this country, and can see the advantages claimed for each system very clearly; but I see no reason why the advantages of both plans should not be combined. I propose, at least, as opportunity offers, to endeavor to combine them. There can be no question that to teach the use of tools *first*, is the first and indisputable duty of the instructor, and that, the use of the tool being once well taught, its application in the arts is his second and no less imperative duty. I should therefore, whether attempting the administration of the school-shop, or of the shop-school, first attempt systematically and thoroughly to teach the entering pupil the use of the tools of his art. This can undoubtedly be best done by a series of graded exercises, beginning with the easiest and simplest, and with the simplest tools, and proceeding by a carefully studied course to the more intricate operations. The progress which can be made by the skillful application of this system of exercises is simply astonishing to one who has not seen it in operation; and it is probably well within bounds to assert that, by means of this method, the young aspirant can be carried farther along in his trade in a year than by the old methods and lack of system in apprenticeship, he could go in the whole period of the seven years for which it was once customary to write the indentures. As a means of attaining skill in the use of tools, simply, it is beyond question the only commendable system.

A good knowledge of the nature, special characteristics, and uses of the tools of any art being thus taught, the next step, it would seem evident, must be the acquirement of some useful knowledge of the proper and best methods of application of this skill. This can be obtained by an extension of the same general system, to

a very considerable and useful extent, and, so far as this is possible, there can hardly be a question that this is the best course to take. But this division of the work of instruction demands vastly more than the familiarizing of the student with certain standard and stereotyped methods of forming parts and assembling machines. It demands such variety of work as will give the young man some idea of the thousand difficulties and problems arising in actual practice, and in the course of regular business. Such practice and instruction cannot well be given, ordinarily, by deliberately planned exercises, beyond a certain and somewhat limited extent, and the experiences of a manufacturing establishment do, I think, more generally confer upon the novice that kind of knowledge, and readiness in adapting himself to circumstances. In fact, it is the habit of promptly meeting emergencies, and of inventing and of perceiving on the instant just what is needed to meet unexpected and exceptional demands upon the intelligence and skill of the learner that actual shop life and practice gives which constitutes its special value in the development of the young mechanic. I also think that the life of the young man in the shop, in the atmosphere of real life which he finds there, must in all cases prove a very valuable incentive to industry, and to a good spirit of work and thought. Nothing so greatly stimulates a boy as to be able to feel that he is taking part in a man's work, and doing something that will have a real and immediate practical outcome. It has always seemed to me that if these two methods can be combined, either by synchronous or by successive practice, the result must have maximum value. This cannot be done, as a rule, to any great extent in the school-shop: it can be done to a useful extent in a shop-school, *provided* that, in the latter, the instructors are compelled to adhere to the system of instruction by means of graded exercises until it has fully completed its work, and that the business exigencies of the establishment are never permitted to break into that system to an appreciably injurious extent. I can see, as I think, a possibility of the two being, to a certain extent, made to work together, and the one to aid the other, the exercises being so planned as not only to teach what is desired to be taught by their means, but also to furnish pieces, more or less finished, for the commercial purposes of the shop. But if the attempt is made to effect this combination, it is evident that the primary object must be the instruction, and not the making of money. I presume that, in those schools in which the business side is presented, this rule is generally adhered to, and

being so adhered to, I should think the presence of the shop an advantage. The needed capital is not, however, always available, and where it is, the usual risks of business must be accepted; and they are not so small as to make their acceptance a matter of small importance to the average trustee. In the school-shop, such as that at St. Louis, and such as that at Cornell, for example, in which this business capital has not been invested, it would seem possible to secure some of the special advantages of the system, by giving to the work of instruction, by means of graded exercises, so much of the time as may be absolutely essential, and then, should it prove possible to find more time, turning the now somewhat skilled students over to the direction of the foremen, who could put them upon work of construction, as illustrated in the building of a steam engine or of a lathe. I should, in every case, however, insist upon the preliminary training by systematic performance of carefully planned exercises, to such an extent that the student should have acquired a good knowledge of the use of tools of all the familiar kinds in regular use in the principal trades which underlie the work of the engineer.

At Cornell University, we are trying to do some such work in Sibley College. Of this work, I will endeavor to give a full account at some later time. Just now, our schemes are too youthful to permit a judgment to be affirmed. The general plan is to have a carefully adjusted undergraduate course, in which the student coming from the high schools, or possessing a good high school education, may enter upon a scheme of study and shop-work which shall accomplish the objects so well described by Dr. Woodward, and by substantially the same means. This course occupies four years and leads to the degree of "Mechanical Engineer"—I prefer the old and established nomenclature—but this may be followed by a year of graduate study in either of several courses of advanced instruction, each of which leads into some one of several special lines of engineering work, as Marine Engineering, Steam Engineering, the Mechanical Engineering of Railways, etc., etc., and to Electrical Engineering; or the student can take up advanced lines of Civil Engineering, also arranged as supplemental to a regular undergraduate course. The undergraduate course includes a considerable amount of shop-work, conducted in much the same way as at St. Louis, the latter part of the course being spent upon work of advanced character, and having commercial value if possible. Investigation and experiment in the laboratory forms a very im-

portant part of our work. The college is divided into three principal departments: the Department of Mechanical Engineering, that of Industrial Drawing and Art, and that of the Mechanic Arts, or shop-work. Much thought is given to making these several departments help each other, by each working in such manner that the products of its work may be useful to the others, as, for example, the exercises in the drawing rooms are made to answer the demands of the instructors in the shop, who desire their pupils to work from drawings made by themselves. The appended schedule gives an idea of the arrangement of the graduate courses. In this schedule, one period in the workshop, or drawing rooms, or laboratory, is the equivalent of an hour in the lecture room. The total number of periods allowed is not expected to be less than fifteen per week, nor more than eighteen ordinarily.

SIBLEY COLLEGE POST-GRADUATE COURSES.

SCHOOL OF MARINE ENGINEERING.

ONE YEAR.

FALL TERM.—Structure and efficiencies of Marine Engines and Machinery, 3; experimental work in mechanical laboratory, 3; contracts and specifications, 3; chemistry or physics, laboratory work, 3; optional, 3 to 6.

Optional: Mathematics, 5; history, 3; languages, 2; natural history, 6; history of philosophy, 3; literature, 3; civil engineering, 2; astronomy, 5; architecture, 3; special work in science, 5.

WINTER TERM.—Naval Architecture; resistance and speed of vessels, as affected by size, form, material of surfaces, and power, 3; mechanical laboratory investigations, 3; chemical *or* physical laboratory work, 3; contracts and specifications, 3; optional, 6 to 9.

Optional: Mathematics, 5; history, 3; languages 2; literature, 3; military science, 2; astronomy, 3; moral philosophy, 2; political economy, 2; architecture, 3; civil engineering, 5; rivers and harbors, 3; special scientific work, 5.

SPRING TERM.—Designs of Marine Machinery, etc., 3; investigation in mechanical laboratory, 3; chemical *or* physical laboratory work, 3; preparation of reports or thesis, 3; optional, 6 to 9.

Optional: Mathematics, 5; literature, 3; American law, 5; Constitution of the United States, 12 lectures; architecture, 5; civil engineering, 3; natural history, 3; physiology, 3; political economy, 5; special scientific work, 5.

SCHOOL OF STEAM ENGINEERING.

ONE YEAR.

FALL TERM.—Structure and efficiency of steam boilers, 3; experimental work, 3; contracts and specifications, 3; chemistry *or* physics, laboratory work, 3; optional, 6 to 9.

Optional, as in marine engineering.

WINTER TERM.—Structure and efficiency of steam engines, 3 ; investigation in the mechanical laboratory, 3 ; chemical *or* physical laboratory work, 3 ; contracts and specifications, 3 ; optional, 6 to 9.

Optional, as in marine engineering.

SPRING TERM.—Designing steam engines and boilers, 3 ; experimental investigation, 3 ; chemical *or* physical laboratory work, 3 ; preparation of reports or thesis, 3 ; optional, 5 to 9.

Optional, as in marine engineering.

SCHOOL OF RAILWAY MACHINERY.

ONE YEAR.

FALL TERM.—Structure and efficiency of locomotive engines and railway machinery, 3 ; civil engineering, 3 ; experimental work, 3 ; contracts and specifications, 3 ; chemistry *or* physics, laboratory work, 3 ; optional, 3 to 6.

Optional, as in marine engineering.

WINTER TERM.—Study of special types of locomotive engines and railway machinery, their structure and proportions, 3 ; civil engineering, 3 ; laboratory investigation, 3 ; chemical *or* physical laboratory work, 3 ; contracts and specifications, 3 ; optional, 3 to 6.

Optional, as in marine engineering.

SPRING TERM.—Designihg railway machinery and apparatus, 3 ; civil engineering, 3 ; experimental investigation, 3 ; chemical *or* physical laboratory work, 3 ; optional, 6 to 9.

Optional, as in marine engineering.

Choice of optional studies, as well as of the special schools of engineering, is subject to the approval of the Director.

Students in the post-graduate courses pay no fees and are entitled to all the privileges of resident graduates.

I have asserted a very decided preference for the old nomenclature of our profession, and have expressed a conviction that the often proposed change of title is neither right, wise nor politic, even if it were possible (as I am very strongly inclined to think it is not), to secure its general adoption, or even favorable consideration, by either the profession itself or the public; while the latter are certainly very much puzzled by the new cognomen, and are not at all certain what the bearer of the new title proposes to do in the world.

I have stated that the term proposed is incorrect, as well as undesired, and entirely undesirable. It is well known that the science of "Mechanics" is now coming to be called, and properly so, "*Dynamics*," and that it includes, as its primary subdivisions, "*Statics*" and "*Kinetics*." Should a change of name be desired by the profession, it should be called "Kinetic Engineering." The term "dynamics" does not properly apply to any idea specially involving that of motion. The term has been generally so used, but

it is coming to be recognized as inexact by derivation, and "kinetics" is now substituted for it. This change has become fully accepted by the English mathematicians, and is well illustrated in Professor Clifford's little work on "*Dynamics.*"

In the next place, there is no valid reason for an attempt to change the title. The existing title is well settled, and it is always a serious matter to attempt such a change without imperative necessity; and to endeavor to effect the change in the face of the prejudices and predilections of the profession, and without either a good reason or other excuse than a desire to introduce a novel designation, seems to me very inadvisable.

The old designation is a better one than the new. Were it defective in any manner, the case would stand quite differently. But the term "mechanical," relating as it does obviously and unmistakably to mechanism, the department of construction with which the profession are specially concerned, is precisely that which suits our purposes. To the vast majority of men it conveys just the idea desired, and is by all well understood; while the newly proposed term is to many men quite without meaning, and is to all less expressive of the meaning to be conveyed than the accepted designation. "*Mechanical Engineering*" is understood wherever the English language is spoken. We could not have, need not have, and, so far as I am able to judge, do not want, a better name.

The profession—and the profession must ultimately determine the question—does not desire a change. The English society is designated the "Institution of *Mechanical* Engineers." Imagine its changing its name to the new form! The title is well understood throughout Great Britain, as it is in this country. Its occasional abuse is no more reason for its abandonment than is the infraction of the decalogue a reason for giving up the ten commandments. Far greater reason, on this score, exists for surrendering the name "engineer," which in this country is usually assumed by the average citizen to be synonymous with engine driver, and in Great Britain with the engine-building machinists also. When our own society was founded, this question was very generally talked over and fully discussed, with the result of settling the title of the society, and, therefore, probably of the profession, for all time, so far as we can now see. The profession does not recognize and does not want the new name. The old is right, is good, is well understood, is acceptable, and is established. The newly introduced title is wrong in derivation and in application, is not a good term,

is not generally understood, even among the members of the profession itself, is not acceptable, and is not now, nor does it seem likely to become, established. We have no right to endeavor to force upon the profession a title which it has not chosen and does not approve. It is for the profession to take the initiative, in any case. When it has done so, and has effected the change, the schools may follow; not till then.

The objections to the introduction of a new title as a degree to be conferred by the technical schools, seem to me even more numerous, and not less serious. All that has been said applies to this phase of the affair as well. Furthermore, it is, as it seems to me, a serious wrong to young men to give them a course of instruction for the profession of mechanical engineering, and then to send them out with a title on their diplomas that is not recognized, and is even unknown by the great majority, not only of the people who it is hoped may become their clients, but even by their professional colleagues, and one which, in some sense, reads them out of their own profession. It is putting them at a very serious disadvantage in their business, as well as making them special difficulties in every direction in which the title of the practitioner comes into view. The title to be conferred at graduation from the school must probably be always determined, aside from the practice of the profession itself, mainly by the action of the special schools, in either branch of the profession, which have become best known. These schools, like the societies, have adhered, most fortunately as I think, to the old designations. So long as they, and the profession generally, make no change, the other schools must necessarily follow the old nomenclature or submit to a disadvantage which may possibly prove sufficient to determine the success or the non-success of the school and of its average graduate.

The term "Bachelor," given in many schools of engineering, is not generally liked by the profession, and is objected to on the score of being incongruous and unsuitable. At Cornell we have abandoned it, and now follow the practice of the schools of longer standing, and give the degrees of Mechanical or of Civil Engineer at graduation, reserving a higher degree—Master in Mechanical Engineering—for the post-graduate year. The designation Master is good English, and is peculiarly suitable to our profession. The schools must in this matter be guided by the action of the profession as a body, and I think that there is no question that the great majority of its members are satisfied with the old title, dislike the

new, and are not likely soon to change. I have not observed any inclination on the part of the civil engineers to dub themselves "static engineers," and I think it probable that the two will change together, if at all. Of the two, the progressive tendencies of our branch of the profession of engineering may be well indicated by the term "Kinetic Engineering;" but I doubt if our friends of the other branch will accept, as significant of their status, the cognomen "Static Engineering," as indicating the unprogressive tendencies of which they are accused occasionally by the more radical of their own members.

Mr. Wm. Kent.—I have watched the progress of the Manual Training School at St. Louis for many years with considerable pleasure. I think the success of that school will eventually make a revolution in our whole educational system. Our public schools must some day make the change which has been made at St. Louis. Prof. Woodward knows that I took part in the discussion of his presentation of the subject at the meeting of the American Association at Philadelphia, in 1884, and that I approve all that he has said to-day, especially in regard to the school-shop. I then said that to judge of the value of the work of a student by its salability, was utterly wrong.

I have taken a little trouble to see if I could satisfy myself as to my real reasons for objecting to the term "dynamical engineer," and I have here a few notes. Prof. Woodward says it is a broader and more appropriate term than mechanical engineer. I hold that it is far narrower and far less appropriate. Mechanical engineering includes not only dynamics, or the science of force in motion, but also statics or the science of forces in equilibrium, kinematics, or geometry of motion, also mechanical drawing, also workshop practice, the ways and means of accomplishing results laid out in the engineer's designs, and also the selection and the application of the best existing practice. The best engineer of the present day is the one who uses the best judgment in the selection of designs, whether they are original or not. It includes also a knowledge of the properties of fuels, the methods of generating steam, the adaptation of furnaces to these fuels, and many other things which are not "dynamical." Turning to some of the great authorities, Smith says: "*Mechanics* is the science that treats of laws of equilibrium and motion." Rankine says: "The engineer is he who by art and science makes the *mechanical* properties of matter serve the ends of man. The title of engineer is more properly restricted to those

who make the useful application of *mechanical* science their peculiar study and profession." General Barnard referring to this definition of Rankine says: "Engineering is the art and science by which the *mechanical* properties of matter are made to serve the ends of man, or it is the useful application of *mechanical* science of those ends." None of the old authorities use the title "dynamical engineer," and there is no reason for the existence of this new title. I agree with Prof. Thurston that the name "mechanical engineer" is thoroughly understood, while the name "dynamical engineer" is thoroughly confusing and misleading. I hope the society will strongly disapprove of it.

Mr. J. T. Hawkins.—I think I can offer some little suggestion with reference to the manual training schools, that may be of value. I might premise by saying that I had the honor in 1865 to organize the practical exercises in the manual training school at Annapolis, and was in charge of it for four years afterward. The part of the present system adopted in such schools in the shops, which in my opinion can be improved, is that they do not give enough lectures in the shops. I think that shop lectures may be so extended and systematized as to save a vast amount of time, and give information in a way that cannot be given in the method adopted by the Professor, that is where one instructor is put in charge of a large number of boys whom he has to instruct verbally. During my first year at Annapolis I adopted that plan and found that it occupied the time of three or four, while in a great many instances lectures and tabulated statements put upon a black-board in the shops saved a large amount of time. Take for instance the question of tempering. A lecture would be given on that subject, and the results given in tabulated form and put up in the shop, enabling the boys to see at a glance what they needed to do to produce a certain result on certain kinds of metals for certain purposes. The same applied to cutting tools and their uses, and many other shop methods. I merely say now that whether there be any value in them or not, I have copies of lectures given at the above institution with diagrammatic matter which I should be glad to furnish to the society as a part of this discussion, if it is thought desirable.

Mr. Oberlin Smith.—I have long thought that there was a wise middle ground in a polytechnic or mechanical school of any kind between the two extremes of throwing away all the work that has been done, or, on the other hand, of producing work for the market.

I am glad to see that Prof. Woodward is working somewhat in that line—that he is not throwing away all those bolts he makes, but is allowing a manufacturing concern that wants them to take them. The evils are of course great if we try to run a school as a commercial institution; such a course must be subversive of proper learning. On the other hand, a disposing of bolts that are needed in the market, and at the same time giving to the boy the exact kind of work he needs to develop him, is a good thing, provided nobody tries to get orders for them, which orders must be filled on time or in excessive quantity. If, for instance, in the case of the bolts, the first two of the three bolts can be thrown away, and the third one, which comes up to the standard necessary for that boy's work, and which also comes up to the commercial standard demanded by the customers, can be sent to them and the money got for it, it is an excellent thing in more than one way. It shows the boy what is an actual practical standard, instead of an imaginary one gotten up in the school, and one which may not be quite practical enough. Another advantage is that the boy does not see things which he has made wasted, that is, none but the imperfect first attempts. With some persons this would have an evil influence throughout their lives that would tend away from economy. It would not affect all minds that way, but it would some. The fact that the part of his work was wasted which was not good enough to come up to the standard, would always incite him onward towards the standard degree of perfection. A third advantage to the school is, of course, the actual money received from sales.

I think that eventually the tendency of all these schools will be in this direction. Part of the work will be thrown away and part kept and used in actual commercial life, but not in such a way as to interfere with the proper training of the student.

With regard to the nomenclature of engineering I agree with the other gentlemen who have spoken entirely. I see no reason why we should use that word "dynamical engineer," especially as it is bad English, and does not cover the ground at all. As far as I know, the word "mechanical engineer" is a good enough name for us at present. There are various others, such as mining, electrical and military engineers, that are very properly named. The only ones that are badly named are the civil engineers. I presume we are *all* civil. I hope in time the civil engineers will get a better name. It is certainly a bad thing that every fellow who, perhaps, cannot read or write, but who can grease a little agricultural engine,

should in this country be called an "*engineer*." I don't know how we can remedy it, except that some time I hope this society will act upon the matter of *mechanical nomenclature*, and that there will be some commission appointed who will get up a good mechanical dictionary, so that these words may be properly defined. I think societies like this can influence the great railroads of the country to adopt some new name for the men who run their locomotives. If all the railroad companies would adopt some name for their engine drivers and the larger manufacturing concerns would do likewise, this new name would be printed in the newspapers and the public would get used to it, and we might gradually be able to get rid of this word, which in the sense so often used, does not belong in our mechanical literature.

Mr. F. W. Taylor.—I desire to take exception to one statement made by Prof. Thurston in his paper. His opinion is that one year of practice in a school shop will supplant seven years of practice in an actual machine shop. I think it would be more nearly the contrary. I think one year of actual service in a machine shop would in certain respects supplant twenty years of practice in a school shop. Probably the great majority of those who go through a practical course of that sort intend to become masters; that is, they would not intend to remain workmen, and it would seem to me that in the course in the school shop the boy misses, perhaps, the one thing which will be afterward of the greatest use to him in his experience with men; that is the knowledge of the character of the men with whom he is dealing. He learns thoroughly the feeling of one student toward another and of a student toward a professor, but he fails to appreciate properly the feeling of apprentices toward their teachers, of workmen toward their foreman, and of foremen to their employees, which will enable him afterwards to manage men successfully. I think that no training whatever in a manual school can give a man this experience, which is more valuable than any manual dexterity which he can attain, and which I think he never can get if he starts at the other end as foreman and attempts to work down. He can only have it by passing through the mill himself; getting there at seven in the morning and leaving at six, and being knocked about to a certain extent as an apprentice in the shops.

Mr. Angus Sinclair.—No one belonging to this society can have a greater interest in the development of the manual school system of this country than I have; but at the same time I think that senti-

ment which has been showing itself through the society, of depreciating the apprentice system and giving preference to the manual school system is not calculated to be of benefit to the mechanical interests of America. It has been repeatedly said here that the apprentice system is dead, and consequently we must look to the manual school system for something to take its place. There is no member, I presume, more around the shops than I am. I am continually over the country from the one ocean to the other. I spend half of my time continually traveling, and I am always watching the men who are doing the mechanical work of the country. I find that there is a growing class of mechanical men—boys who have not the name of apprentices, but nevertheless they are learning the trade just the same as apprentices used to do. They are not held down for seven years under the close rules of the apprentice system, but they have opportunities of learning the trade that perhaps the old apprentice system did not supply. Now, I think that the duty of the Mechanical Engineers' Society, in regard to that class, is to give them the opportunities of learning the higher branches of mechanical engineering, supplying them with facilities for night schools, where they can learn the principles which they are so often deficient in. That is a system which is becoming very wide-spread in Europe. It has been receiving a very great deal of attention in Great Britain lately. There is not a city of any size in that island where machine shops are running that apprentices cannot go nightly to school, where they have the very best opportunities for getting the higher parts of the mechanical training; and I consider that in that respect this country is falling very badly behind Great Britain, and it is to a great extent because this society and similar societies hold that manual schools and the technical school should do what eventually will have to be done by the apprentice system. The great mass of those who are learning the business are in shops, under some name or other, and there is where the great mass of experience is obtained that enables men to carry on manual work. A boy may work in a technical school or in a manual school and attain the skill which enables him to do a piece of very fine work, but there he will never collect that great mass of experience that enables him to control men doing similar work to the best advantage in a great shop or even in a small shop. If I mistake not, the leaders of this society, the men who have made their mark on the mechanical work of this country, have risen through the shop. They have gained their technical knowledge

through burning the midnight lamp under the very greatest disadvantages. If they would help those who are coming in their footsteps, to obtain the information which they acquired under such great disadvantages, more easily, they will be doing a great work for themselves, to the country and for the mechanical interests generally.

Mr. J. M. Dodge.—I came here by railroad from New York. I would have known more of the country if I had walked; but it is a question whether that would have been any decided advantage. I served my time in a shop. My foreman hit me with a hammer one day because I asked him how to temper a chisel. Afterwards, I got a little engineering chart. I followed its directions at home, took my chisel back to the shop and found out I knew how to do it. I believe if I had gone to the manual training-school and had that lecture about tempering given to me I would have known more about it than my foreman did. I do not believe in this age of the world's history it is worth while to cling to an old institution, simply because it is old. It is not fair to say that the apprentice system made foremen, because the apprentice system, out of a thousand boys only made one foreman. The fact of the matter is, that the personality of the boy has got a great deal to do with it. I went through the shop experience very thoroughly. I went on a strike and got hit with a brick. I followed the men right through, worked with them, fought with them, and did everything, and I was thoroughly put to blush by a young man from the Stevens Institute, after I had erected an engine on a large ship, by finding out that he knew a great deal more about it than I did. I had built the engine as I thought, and the proudest thing I did was to find a mistake in the design, which I afterwards discovered was because I didn't measure it right. The fact is that I had some training at Cornell University, and afterwards I went through the shop thoroughly, and I must say I wish I could go through a manual training school now. So far as managing men is concerned, my experience is, that a man who knows what he wants will get it done. A man said to me: "What will you do if there is a strike?" I said: "I don't know. What will you do if there is a strike?" He prided himself on being able to manage men. He had a strike afterwards, and he didn't manage the men any better than anybody else. He fell right in and did the best that he could when the time came. I am largely of opinion that with good material, a manual training school will instruct thoroughly

and perfectly as far as it goes, and that a man of the right mind will gather up the deficiencies without any trouble.

Prof. S. W. Robinson.—I think that this course which has been detailed at some length is an admirable course for students in mechanical engineering. I might state a few points from my own experience which may be of some interest in regard to this question. In the first place, with respect to the apprentice system, in my experience of four years of apprenticeship in a machine shop, I never got so much shop philosophy as this—that in forming a piece of iron for a machine there are two operations—first, rough dressing it out; and second, finishing the piece; and further, that in rough dressing the piece out, the way to do it well is to do it quickly, as the main point; and in finishing the piece the way to do that well is not to take the biggest chip you can possibly take, but to take such chips as will give the best results as to form and surface. This is the kind of philosophy that should be given in our school shops. If a man can go through a whole appenticeship and not get as much philosophy as that, I think that five minutes under a teacher in learning this is worth more than four years' apprenticeship for this point. When you apply it to all the points, you will see that there is need of both applications. I think the school shop is a necessary element for the highest success, and that the machine shop is also a necessary element for the highest success in life. Let a young man who comes out of school full of philosophy go out and learn the practical. In this day of competition it is necessary, as has been expressed by one of our members, not to allow the workman to tinker along with little "tickling chips;" he must take the heavy chips when he can.

This age, I think, might be called a mechanical age. Who is responsible for the great improvements and the wonderful things which we enjoy every day all over this land? Who is more responsible for these than the mechanical engineer? I think it is eminently proper that the mechanical engineer should be so styled, instead of being called a dynamical engineer, or some other name which leaves out most of the meaning connected with the profession.

With regard to the manufacturing of articles in school shops, I tried both ways and can speak from experience. I started out in one school where a mechanical laboratory was set up with the central idea that we must make something to sell, because, "if a student made something to sell" his enthusiasm would be unbounded, as compared with working on a piece which, when done,

would go to the scrap heap. Well, I would urge from my own experience, in that as well as the other plan—that is, where there is no manufacturing and no remuneration given—I would urge a fourth objection to the three which have been already named, and that is this: that in this practice of manufacturing something, the student commences to work and things will go on very nicely until you come to find out that the student has accomplished very little and the work is dragging. The man who wanted the work comes around in a hurry and says he will lose his order unless the work is ready next week. Well, we cannot begin to finish the job by next week without an extraordinary effort for us. The man finally brings such a pressure on us that we take all the boys out of the classes in the school and concentrate them on this job work. That introduces irregularity into the whole thing, spoiling class standing, running shop unusual hours, etc. This fourth objection I count to be a serious detriment to the school manufacturing scheme.

I think it is well that we follow Mr. Kent's suggestion to "sit on" some of these titles, by appointing a committee who shall take under consideration the proper titles to apply to the mechanical engineer, the engine runner, etc., and make a proper distinction all through.

Mr. Hosea Webster.—The fact that the majority of the prominent members of this society are men who have started from the bottom, and, as expressed in homely phrase, "pegged up;" the fact that a great many of them have a good deal of distrust of "college men," and the fact that many college men after being out of college a few years, would find it a hard matter to pass their first calculus examination, lead to a good deal of perplexity in the mind of one who may happen to be called upon to advise a young man desiring to become a mechanical engineer, what course to pursue.

It is unfortunate that there is this distrust of the college man among our leading manufacturers, but may not one cause of it be found in the fact that, having spent four years in college, a young man is granted his degree if he has attained an average of say seventy-five per cent. in his examination, and so gets into a seventy-five per cent. habit, while the profession of engineering is, above all others, a hundred per cent. profession?

A seventy-five per cent. tracing won't make a head draughtsman.

The discussions of the matter of technical education, which are now going on, are a good sign, and indicate that the time is not far

distant when manufacturers will realize that technical education is the best foundation for the profession of mechanical engineering.

The English system of paid apprenticeships is meeting with good success. A young man, upon payment of a small sum annually, is admitted into the shops, put to work with the men, and has special attention paid and careful practical instruction given him in return for the money invested. This system is said to be turning out some excellent young engineers.

The technical graduate has reached the point where he has learned how to learn, and draw logical and practical conclusions.

The proper combination of the theoretical with the practical, must result to the advantage of the producer, and it is hoped that the discussions in this society will soon bring about the desired result.

Mr. W. F. Durfee.—I am aware that the time is getting very short, and I will promise to be as brief as is consistent with a clear presentation of certain points which I regard as deserving of especial consideration in connection with any scheme of study and practice intended to serve as the foundation of the education of the mechanical engineers of the future. When an engineer of experience undertakes to erect a structure or mechanism of any kind, the first thing he considers is the character of the soil upon which he is to lay those foundations, upon whose stability the integrity and usefulness of all his future work depends. In such a fundamental matter there must be no mistake; a proper selection *must* be made or disaster is absolutely certain.

I am a firm believer in the great possibilities and far-reaching value of the work of the leading technical schools of our land in laying the foundations upon which the practice and the fame of the engineering of the future is to rest; provided, however, that their efforts are conscientiously expended upon a wise selection of mental material.

Notwithstanding the fact that the leading schools of engineering have met with some measure of success, and that there are many members of that profession to whom they point with pride as evidences thereof, I am, as the result of a somewhat prolonged experience, firmly of the opinion that these schools will not conserve the best interests of the future, until they adopt some thoroughly effective system of selection, which shall at an early stage of their studies, cull out from among those students who aspire ulti-

mately to become engineers, such as have not that intuitive practical sense of the eternal fitness of things, and of the adaptation of means to ends, *born in them, as an endowment of nature*, which is an essential qualification of every competent engineer.

Relative to such a vital matter as this the trumpets of our technical schools should give no uncertain sound, but should proclaim with a resonance that cannot be misunderstood, that it is not sufficent evidence of fitness for the engineering profession that a student is successful in passing through some mere routine of study extending over a certain fixed period of time;—that there is more required than the mere facts that a young man has studied mathematics until his head is a veritable "ant-hill of units and tens;" that he has attempted to learn the mere rudiments of mechanical drawing, and next to nothing of the art of design;—written a thesis upon some question affording opportunity for an exuberant display of the opulence of his knowledge of the higher calculus, which is used with such success as a foil to his abject poverty in all things practical, as to win the plaudits of his friends and the envy of his enemies:

"And still they gazed, and still the wonder grew,
That one small head could carry all he knew."

More even, than the culminating fact of academical experience, which much too often follows such a successful exhibition of the smooth working of the young man's memory;—that of being straightway turned loose into the work-a-day world, clad in sheepskin, "*cum maxima laude*," and decorated with some such titular abcmination as "dynamic engineer."

Yes, Mr. President, something more is required than all this, to fully qualify a young man for the initial experiences of an engineer, and that something is:—mental adaptation *to*, and an abiding love *for* the profession;—an inborn and irresistible consciousness that in no other walk of life can he so employ the ten talents intrusted to him as to gain other ten talents, and what is of vastly more consequence, the final commendatory judgment upon his stewardship, "well done, thou good and faithful servant."

For the purpose of exemplifying my meaning, a few illustrations from my own observation and experience may not be out of place.

Some years since I had as an assistant a young man who was a graduate of a famous European institution of learning; on his

diploma were the names of some of the world's most renowned professors, who could have had no other justification for their signatures than the fact that the student, as the result of a persistent application of diligent study to a retentive memory, could "with parrot tongue repeat the scholar's part."

This young gentleman, whose American antecedents had been reinforced by European study, and crowned with its honors, not only deliberately proposed to me, as the best possible means of determining the quality of any ore of iron and of the metal that could be produced from it, the erection of a miniature blast-furnace, ten feet in height, having all the apparatus and appurtenances peculiar to furnaces of ordinary dimensions—but he was both earnest and eloquent in support of his proposal. It is not necessary for me to say to metallurgists that there was not one grain of practical common-sense in that suggestion.

I once knew a gentleman, of excellent scholarship and admirable social qualities, who had conscientiously endeavored through all his active life to acquire business and reputation as an engineer. At the age of fifty he built a structure, which, after standing for a time, actually tumbled down from its own inherent weakness; and on investigation it was found, so imperfectly had its details been supervised, that some of its supporting columns could have been tested to absolute destruction by a tack-hammer. This gentleman had a constitutional inaptitude for the practical, although his theoretical qualifications were beyond question. I knew another man who, after seven years' apprenticeship at the brass-worker's trade, acknowledged that he could not mould a spike.

Now, it would have been of the greatest benefit not only to these individuals, but to society at large, had some one in authority said to each of them in the outset of his career: "You are not adapted to the line of life you propose to yourself to follow." The attempt (in which far too many of our technical schools are engaged), to make engineers of anybody and everybody who, in the ardor of youthful inexperience, thinks he "wants to be an engineer," should, in the interest of society as a whole and of the future of American engineering, be stopped at once and for all time. As well think of making every man a poet who can write "doggerel," or every person a singer who has a loud voice, as to endeavor to make engineers of men whose only qualifications are perseverance and a good memory.

Before closing I desire to say a few words relative to what I will

call the ethics of engineering, a subject almost if not totally ignored in our technical schools.*

I have not time to discuss this important question at as great length as I desire, but will divide it into four heads and briefly speak of each.

1st. The duty which a young assistant engineer owes to his chief.

2d. The relations and duties of engineers to each other.

3d. The duty which an engineer owes to his employer.

4th. The duty which the profession owes to society at large.

As regards the first division of the subject, I will say that I have often heard it remarked by men in responsible positions, that they would not have a young man from a technical school about them, as graduates from such schools had no idea of subordination or discipline, and were too much disposed to spell assistant with a big A and chief with a small c; and I have also heard of instances in which young men from some of our schools of engineering had hardly got warm in their positions as assistants to some worthy chief before they began to use all the arts of the "sapper and miner" to undermine the man whom they should have been loyally assisting, with a view of securing through the influence of relatives and friends the position he occupied.

All this is wrong, and in a great measure such conduct on the part of young engineers is the result of a thoughtless selfishness, against which they have heard no warning voice raised in the school from which they came. A young engineer accepting the position of assistant, should be as loyal to his chief as the chief of staff of an army is to its general;—should do everything in his power to win his confidence and to deserve it; should keep him advised of the progress of all work under his direction, and protect his reputation as carefully as his own—in short to be an assistant, confidant and friend to his chief who appointed him.

Relative to the relations and duties of engineers to each other, much more can be said than passing time permits. Mutual helpfulness and generosity should always characterize these relations, and a charitable consideration one for another always prevail. No honorable engineer can ever do willful injustice to his fellow, or

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* At the Pittsburgh Meeting of the Am. Inst. Mining Engineers in 1886, *Trans.*, Vol. XIV., page 609, the President, J. C. Bayles, Esq., of New York, delivered a very able address upon "Professional Ethics" which every young engineer could "read, mark, and inwardly digest" with great profit.—W. F. D.

pass adverse criticism upon another's work without the fullest knowledge upon which to base it, and especially will he never comment adversely in a public manner upon the work of another upon mere *ex parte* testimony. "Do as you would be done by," is the rule by which every honest engineer gauges his relations with his fellows.

I must be still more brief in my comments on the third head of this subject—the duty of an engineer to his employer—and will simply say that that duty consists in the strictest honesty of word and service, and in its discharge he is to keep always in mind, to dare to "do all that may become a man."

I now come to the fourth head—the duty which the profession owes to society. One of the first and most important duties which the profession owes to society is so to govern and conduct itself as to secure and retain the confidence of the public, to the end that when important engineering works are proposed, society will have entire faith in its judgment and unhesitatingly call for its advice and assistance. Another important duty of the profession is the maintenance of organizations for the dissemination of professional information, which tend greatly to protect society from the schemes of ignorant pretenders and unprincipled knaves. Other duties of a public character might be named if these remarks were prolonged, but I will close by earnestly urging the managers of our technical schools to bestow more attention in the future than they have in the past to the matters of discipline, and the determination of the natural qualifications of the students they graduate, and by commending the ethics of engineering to the careful and conscientious consideration of all interested in schools of engineering, and more especially to some of the more recent and enthusiastic advocates of manual training.

Prof. C. I. King.—It seems to me that when an apprentice has served his time, and when a young man from college, having been through the technical school, has served his time, we will say, and both come out, we will imagine on a level, and begin their work in the shop—that the apprentice has practically arrived where he is ready to begin to learn something. He is where he feels a responsibility that he has never had before, and if you are going to make a foreman out of him he must acquire all that implies after his apprenticeship has been served. It is equally so with a college man, if he is going to make a superintendent or foreman. The only difference it seems to me is, that your college man has a basis to build

upon that is as broad a foundation as can be laid. The difference in the broadness of these foundations depends on the men altogether. It seems to me that in that respect alone the college man is a good way ahead, and I hold that with this system of manual training, every college man, if he has any natural ability, when he steps into the shop he is capable of earning a living there. He is able to earn as much and in a great many cases more than the apprentices. In regard to the different systems of work in the colleges, I hold that it is almost impracticable, so far as good results are concerned in the way of instruction, to mix instruction and business, and especially so if there is a time limit for completing the work. The result in every case will be found, as Prof. Robinson says, that you are sacrificing instruction to the completion of work. That has been my experience for eight or nine years. Mr. Smith touched upon one matter which I wish to mention, and that is in regard to a vocabulary or dictionary of mechanical terms. It seems to me that in a great many of our processes we have need for a term which when spoken makes the object referred to stand out before one so that one knows what is meant. But in a great many pieces of our machinery we refer to a part as "a boss" or "lug," or something else of that kind, and it does not mean anything. We should try to teach our boys terms that will bring out some kind of a meaning, or picture something to the mind. I think there is a great deal of work before the Society and before the technical schools in that direction.

Prof. Woodward.—I have little to add in closing the debate. I desire to thank Mr. Hawkins for his excellent suggestion, while at the same time I ought to say that we make continual use of the lecture method. In fact, the class, or "Russian," method of tool instruction necessarily involves lectures, black-boards, and general explanation. The economy of our method arises from giving an explanation, a diagram, directions, etc., to twenty-four boys at once. Then, again, when they proceed to the execution of their task, the teacher knows so well what each has to do, that a glance suffices to tell whether the student is doing as he ought or not. Meanwhile it must be remembered that our students are not men; they are boys from fourteen to eighteen years of age, and that they know nothing whatever of engineering.

Again, I think the gentlemen who have spoken have dwelt too much upon a single phase in the training of an engineer—that of being a shop foreman or manager. Some of you seem to regard

what I call the "Training of a Dynamic Engineer," as only a new kind of apprenticeship, the object of which is to make a man a good machinist or a manager of machinists. To be an engineer means vastly more than to be a skillful workman or a fine executive officer, or both combined. I grant, if you wish, that it does mean thus much, but it means a knowledge of theory too; a knowledge and ready command of both analytical and graphical methods of investigation; a knowledge of the best practice; of what has been done, and how done, in the engineering world. An engineer is a man whose familiarity with different methods and theories is wide enough to entitle him to speak of his "*judgment*." A man who knows but one way has no *judgment* about ways. A man who has never "seen any use" for graphical statics, or the calculus, or thermodynamics, simply admits that he has no command of such things, and that countless opportunities for their use pass him without his being in the least aware of the fact. A born frontiersman is apt to "have no use" for a thousand of our great conveniences about which he knows nothing. These remarks are in part suggested by some side-discussions I have heard at this convention. Let me say that no subject is put down in the course of study given as an appendix to my paper, which I do not consider essential to the training of a finished engineer.

I am glad to find that you all appear to agree with me in regard to the policy of our shop. As regards what we cannot and do not try to teach in our shop, I will quote a word from that very keen observer and successful man of business, William Mather, Esq., manufacturer, Manchester, England, late Royal Commissioner of Education to America, now Member of Parliament.

"There is no possibility of teaching in a school that sort of knowledge which practical work, carried out on commercial principles within restrictions as to time of execution, etc., can alone make any one familiar with."—*Technical Education in Russia*, p. xii.

Bear me witness that the manual training school does not claim to teach a single trade, nor to give business experience.

I did not expect my suggestion of the term "Dynamic Engineer" would be received with much favor. All your traditions are against it. It suggests distinctions not generally made. When I first heard the phrase "dynamic engineering" from the Sheffield Scientific School, I was not pleased with it. It struck me then as it does Mr. Kent to-day. But further reflection effected a change

of view. Mr. Smith thinks "dynamic" is not good English. It is certainly good Greek, and in that respect it is on a par with "mechanical" which is also from the Greek, "mēchanē," (μηχανή) *a machine.* Now if our engineer is to be only a machinist, I will withdraw my suggestion (and this is not intended to reflect in the least upon that honorable calling); but if he is to grapple with, and control, and direct in the best manner the great FORCES ("dynameis," δυνάμεις) of nature; steam, electricity, wind, water, and all-embracing heat, then I claim for him a higher title than a maker of machines.

Mr. Kent appears to make the term "mechanical engineering" embrace all engineering, civil and mining. Rankine, whose fine definition of an engineer Mr. Kent quotes, was a civil engineer, or rather he was, as I should say, a civil and a dynamic engineer. But there must be a distinction between the two. They have much in common, perhaps most things in common, but the names by which they are to be distinguished must come from their differences. Now nothing better represents the peculiar field of our society than "dynamics," which from time immemorial has embraced *forces* upon *bodies* in *motion.* Here then we have our peculiar province; the *bodies* (machines) with their details; the *motions* of their parts (kinematics); and the *forces* by which they are moved, which the several parts transmit, and which they apply to useful ends. I still think that no name can so appropriately express our high calling as "Dynamic Engineering."

CCXXII.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

No. 222.—18.

Are there any grave objections to cam motions for moving the valves of high-speed engines? What is a limiting speed for cams?

DISCUSSION.

Mr. F. F. Heminway.—It is difficult to say in these times what a high-speed engine is, and while I have had some experience in operating valves on steam engines by cam motions, I should hardly be prepared to call them high-speed engines. I have used it on a steam engine making as high as two hundred and forty revolutions a minute, without any difficulty whatever. As to what the limiting speed would be, I could not say. That is as high as I had any occasion to use a cam motion for that purpose. But at that speed I found no difficulty. Of course it must not be a heavy unbalanced valve. It must be a balanced valve, and a valve so arranged that motion is moderately limited.

I think the largest engine on which I ever attempted such speed was 10 inches in diameter. I have used the cam motion on cylinders as large as 20 inches diameter, but in that case the stroke was so long that there was no occasion for a high rotatory speed.

The valve was a multi-ported valve, and the extent of lift of the valve was three-quarters of an inch. That was the most that the cam ever was required to lift the valve, so that the lift, as I say, was very moderate. With a short lift I never found any difficulty. I ran the small engine at 240 revolutions continuously, that is through ordinary working hours, for a year, without any attention whatever, and at the end of that time found the cam was scarcely worn. It is fair to say, however, that there was almost absolutely no pressure to be overcome in lifting the valve. The entire travel of the valve was an inch and a half, or three-quarters of an inch each way from its central position.

Mr. Babcock.—I cannot say much about valves on high-speed engines, though I have made a great many engines with valve motions driven by cams. I cannot now recall any running over 75 revolutions, which, as you know, is very slow speed in these days. But I have had them running for ten years or more with very little wear and very little rattle. Those cams were made to move the valve about an inch and an eighth—a multi-ported valve which had the pressure of steam upon it a part of the time. But it was a four-valve engine, so that the pressure was relieved from the valve largely at the time when it was moved. I imagine that the main difficulty to be found in running cam motions at any speed is simply in making a well-fitting cam. There is no reason why what is known as the French cam working between two parallel bars, when it is well fitted, without lost motion, should not run as smoothly and at as high a speed as the eccentric. The difficulty is to keep it in that condition. When I saw this question in the list, I imagined that our friend Porter, whom some of us consider the apostle of high speed, would be here to say something on that subject. He told me the other day that he had the best valve motion in the world. He said that he had a single valve which would cut off at any point of the stroke, gives a constant lead, a constant release, and a constant compression at any point of cut-off, and only used one valve to do it, and asked me how I supposed that was accomplished. Said I, "You use a cam to move it." Said he, "I don't use an eccentric." So I judge he is in that line—to make a cam motion and run at high speed.

No. 222.—19.

"What is the maximum safe load for steel tires on steel rails? Can this be expressed in terms of the crushing strength of the tire and rail, and per inch of width for different diameters of tire?"

DISCUSSION.

Mr. Henry R. Towne.—To start the discussion, I may mention the fact that Mr. Chanute some years ago, while in charge of the Erie Railway, made some very interesting experiments on this point. I think a partial description of them was published in the Proceedings of the Civil Engineers, but unaccompanied, I believe,

by any illustrations. Some two or three years ago, having occasion to investigate this subject myself, I wrote to Mr. Chanute, asking if I could get the information he had acquired in his experiments, and he very kindly sent me tracings which he had compiled, showing the actual tests which he has made, and which were very interesting indeed. His method of investigation was to take the different standard locomotives in use on the Erie Railway, jack up one of the driving wheels and place under the wheel, between it and the head of the rail, a piece of very thin tracing paper with a piece of thin carbon paper under it; then let the wheel down on the paper, leaving it a few moments, then raising the wheel again and taking the paper out. The result was to make a record showing the area and form of the contact between the wheel and rail. Those outlines he then had inked on the tracing paper, and from those tracings he made blue prints, of which he sent me a full set. I think there must have been forty or fifty of them. The forms of the surfaces of contact were very curious. I could not produce them on the board, for none of you would be able to see them; but the outline of the surface of contact was irregular, angular, and quite sharp oftentimes in outline, the total area being exceedingly small. The maximum measurement from one extremity to the other of any one of the tracings, as I remember it, would not exceed fifteen one-hundredths of an inch. Mr. Chanute had calculated from each of these, very carefully, the actual area of contact, and had collated that with the weight on the driving wheel, and from that had ascertained, *first*, what was the actual pressure per unit of area—per square inch if you please—of the contact of the rail and wheel with the engines that they were using; and, *second*, from that had made a deduction as to what, in his opinion, constituted a safe limit. Mr. Chanute's deduction was that 12,000 lbs. should be the limit of load on any one driver. Perhaps some present may be more familiar than I with the experiments to which I refer, or with others in the same direction.

Mr. Wilfred Lewis.—The only expression that I know of as to the relation between the diameter of the wheel and the load to be put on it, is the one suggested by Mr. C. Shaler Smith for the rolls for a pivot bridge, and the expression he used may be reduced to the formula—

$$\text{Load} = 1760 \times \text{face} \times \sqrt{\text{diam.}}, \text{ all in lbs. and inches.}$$

It would seem more natural that it should vary directly as the di-

ameter. I do not know what the philosophy is in putting it in that form.

The Chairman.—This is a matter of great consequence in railway practice, and of great importance by reason of the growing necessity for increasing the size of engines and the load on the drivers.

Mr. Angus Sinclair.—I think it would be very desirable if the members of this Society who are familiar with very heavy loads would give the results of their experience of what is likely to be a safe load for the locomotive tire. Most railroad men who have studied railroad mechanical subjects are familiar with those experiments made by Mr. Chanute, and I don't think that it is generally believed now that the deductions made from those experiments have been favorable either to the wear of rails or to the wear of tires in practice. The exact experiments which Mr. Chanute made show very distinctly the amount which was resting on the rail, but it is very probable that his deductions of how much weight the rail would safely stand have had injurious rather than beneficial effects. The small area resting on the rails, with the tires with which he experimented seems to show that the tire had a considerable cone. A cylindrical tire would have a greater resting surface than one which was considerably coned, and it would depend considerably on the form of the rail head and the form of the tire how much the bearing surface was. Mr. Chanute's view that 12,000 pounds was the safe limit to put upon a tire, has had a very great influence on locomotive designers, and I shall not hesitate to say that that influence has been very injurious to the steel rail. A locomotive which has a light load on it is much more injurious to rails than a locomotive which has a heavy load. In English practice they are using eight and ten tons safely on locomotive tires, that is with single drivers, and there does not seem to be any trouble from the wearing out of rails. The tendency among locomotive designers in this country now is continually to increase the weight on drivers, and they are by no means confining themselves to 12,000 pounds, and those who go beyond that limit are having the best success in making the tires and the rails wear. The locomotives on the Erie have Mr. Chanute's influence very strongly on them still, and I do not believe that there is a railroad in the country whose rails have suffered more from the wear of the slipping drivers than the Erie locomotives, and the tendency now is to ignore the area of bearing and put a weight on the drivers which experience shows is conducive to long wear of tires and of rails. Probably there is a limit

which is not determined yet, where the rail will crush under the driver. It seems to me that the engineers here who have had great experience with crushing weights in other departments will be able to shed light which would be beneficial to railroad men on this subject.

The Chairman.—The statement is certainly interesting, and it suggests that one fact which ought to be included in any equation representing this matter is the elastic strength of the material composing the rail and tire. Obviously with a theoretically perfect rail and tire there is contact only either at a point or at a line. If you conceive of a perfect cylinder resting on a perfect plane, of course the contact is a line. Now, if that were really the case with a locomotive, obviously the wheel would have to bed itself into the rail until the actual area of contact was so extended from a mathematical line as to give an area which, divided by the load, would reduce the pressure per unit of surface to some figure within the elastic strength of the material. If you went beyond that you would have crushing, and therefore it would seem to me that any correct equation representing this point would have to bring in that as one of its factors, and that the results obtained would vary materially with different steels, hard and soft.

Mr. Oberlin Smith.—I have not much experience in locomotives and cars, but I have run a good many cams on rollers under heavy pressures and, substituting steel castings for cast iron, have put from 10,000 to 20,000 pounds upon rollers which were only about four inches in diameter and three inches face. In the case I speak of the cast iron soon crushed and wore out, but the steel castings stood excellently well.

No. 222.—20.

"What is the present status in Chicago of the question of smoke-preventing furnaces under steam boilers?"

Mr. G. E. Palmer.—That the City Council has passed laws abating the smoke nuisance in Chicago is nearly the whole extent to which the matter has progressed.

I have given this matter some attention for quite a number of years, but I have been thinking during the past few moments that, with all the experience I have had, my mind is still almost entirely

a blank on the subject of smoke prevention, or smoke-preventing furnaces. You can imagine from this that we have accomplished very little; in fact, I do not know of any substantial, general progress that has been made in the city of Chicago in the direction of abating the smoke nuisance or in the general introduction of any kind of smoke-preventing furnaces under steam boilers. There have been introduced a hundred or perhaps a thousand (I don't know how many) inventions, claiming to prevent the making of smoke, nearly all of them mechanical, and nearly all of them failures or worse than failures, for in almost every instance when anything of the kind has been introduced in the city of Chicago, even if it has succeeded to any extent in abating the smoke that was produced, it has also been found to result directly against economy.

People who have been forced, by reason of the aforesaid city ordinance for the prevention of smoke, to put in some design or some machine or some claimed invention for the prevention of smoke, have generally found that the use of it has cost them considerably more money to run their works, and in most instances they have concluded to pay a fine once in about three months, rather than pay the extra coal bill required to operate the same.

The only direction in which there has been any success in the introduction of smoke-preventing furnaces under steam boilers in the city of Chicago, has been in the construction of brick-arch furnaces, burning the fuel under these brick arches, and in some cases these have been quite successful both in economy and in preventing smoke, particularly when plants of boilers have been put up of sufficient capacity in order that they may be run and not crowded beyond a reasonable limit. But whenever these plants have been put up, even with the brick arches or ovens, and have been over-pushed to any considerable extent, there would be about as much smoke as before. In fact, Chicago, with all its legislation and all its inventive genius, is almost entirely in the dark as to proper furnaces to be put under steam boilers for the prevention of smoke, and we are certainly in hopes that some light may be thrown upon this subject by our visitors at this time. We are seeking for light in this direction.

Mr. G. W. Cole.—Recently I was passing by the West Side Water Works in this city, and my attention was called to the combustion at the rear of the furnace. It appears to be very perfect indeed. I was told that that furnace was the invention of Mr. Mason, who was the chief engineer of the city water works. It may be that some resident here will correct me by saying that the smoke-stack gives

off a good deal of smoke. Still the combustion was very perfect that I saw when I was there.

Mr. Palmer.—The fact of the smoke-stack not giving off any smoke is not an evidence in my opinion of perfect combustion or great economy.

Mr. Cole.—I would say I did not observe the smoke-stack at all. I looked in a hole in the rear of the fire, between the point where the products of combustion left the grate and where they went into the return tubes of the boiler. The smoke-stacks I did not look at at all.

Mr. G. H. Babcock.—I would say that there is a great deal of the personal equation in this matter. The fact is, that with a good furnace and a good fireman, smoke can be prevented to a large extent without any mechanical appliances, but a great deal depends on the kind of coal. A furnace which will burn one kind of coal without smoke will not burn another kind of coal without smoke. I am not speaking of anthracite, because we all know that we can burn that without smoke. But you may take two different kinds of bituminous coal, put one in a furnace and it will burn without smoke, and another in a similar furnace will not burn without a good deal of smoke. The coals may be practically of the same smoke-producing quality. For instance, there is a furnace running in Chicago which has been running for four or five years on Indiana block coal, with absolutely no smoke, and giving good economy. The same furnace put up in other places, and using other kinds of bituminous coal, did not answer. I remember noticing many times, a number of years ago, at the Baltimore Sugar Refinery, the utter absence of smoke from the chimney. They were always burning a semi-bituminous coal known as the Cumberland coal, while other chimneys in the neighborhood were all producing immense quantities of smoke from the same coal. The difference there was wholly in the firemen. The furnaces were not different. It was entirely in knowing how to handle the coal. In Glasgow, considerable is being done in the way of preventing smoke, by automatic stokers. At the works of the Singer Manufacturing Company they have a stoker (I cannot give you the name of it), but it is one of a common kind, having a revolving chain grate. The furnaces are provided with fire-bricks in such a way as to produce a sort of reverberatory action. They get absolutely no smoke under ordinary circumstances, with very good economy. The same arrangement is used in other parts of Scotland and is working very well. Whether it would work equally

well with American coals, is entirely conjectural. Men like Mr. Palmer and myself, who have had a good deal of experience in attempts of this kind, begin to distrust our own knowledge on the subject. We feel that all our experience with one coal does not qualify us to judge of the requirements for another.

Mr. W. F. Durfee.—Probably the simplest expedient for the prevention of smoke that has yet been devised, (if we except the fireman who knows how to use coal), is the "dead plate" invented by James Watt. I have used that with bituminous coal with very great success, no smoke passing out of the chimney. It consists simply of a plate of metal in front of the grate bars, or a surface of brick two and a half feet long in the case in my own experience, and the width of the fire-door in front, and expanding to the width of the grate at its inner end. The coal should not be shoveled directly upon the fire, but be spread in a thin layer on this dead plate, when its gases pass off slowly and are consumed without smoke, while the solid parts of the fuel are partially coked. When the fire needs replenishing the stoker pushes this partially coked coal upon it, and fills the dead plate again with fresh fuel. If this simple and inexpensive method of preventing smoke is managed properly, very little or no smoke at all will escape from the chimney.

Prof. R. C. Carpenter.—I made a trial of the brick arch method, and found the result just the same as Mr. Palmer mentions. Where our consumption was twelve pounds or less of bituminous coal to the square foot of grate surface, we got very good results indeed. But when we had to push our boilers and make a great deal of steam, we got very poor results. I think that necessarily follows from using the brick arch. The arch tends to store up a good deal of the heat of the coal. We got, with a very small consumption of coal, very good results indeed. I have understood incidentally that the water works in this city were using anthracite coal. That may not be so, however. I have seen a furnace which, although using slack coal, gave little or no smoke. It had an automatic stoker, and the boiler was set a great distance above the grate bars. The result with that furnace was very good, both as to evaporation and as to smoke prevention.

Mr. Cole.—The suggestion has been made that they are using anthracite coal partially in the city. The North Side Works are using anthracite coal, but the furnaces of which I spoke are at the West Side, where they are using soft coal, a portion of it being Hocking Valley coal occasionally.

Mr. Palmer.—I understood Mr. Carpenter to say that he obtained

very good results by burning about twelve pounds of bituminous coal per square foot of grate. Do I understand by that economy, or the prevention of smoke only?

Mr. Carpenter.—We made some tests with and without the arch. The result was several per cent. in favor of the boiler with the arch, in economy, and our fireman thought that we saved a great deal. Our trial showed a saving of from five to ten per cent.

Mr. Palmer.—The experiments which I made proved to me that it is useless to burn so small a quantity of coal as 12 pounds, and expect high economy. I have obtained the highest economy I ever expected to obtain in burning 28 to 30 pounds to the square foot. When I wish to obtain the highest economy, I burn all the coal I can to the square foot of grate. In other words, I raise the temperature of the furnace as high as I can. I believe that the ordinary results of burning bituminous coal in the West, so far as my knowledge goes, do not show evaporation of more than 5½ pounds per pound of coal. When I have been making experiments, burning as low as 12 pounds, and then taking the same boiler with the same setting, the same furnace, and burning 25 pounds per square foot of grate, I have obtained as high as 40 per cent. better economy in burning 25 pounds per square foot of grate, than in burning 12 pounds. I do not know whether this would hold true of hard coal passing out at a very high temperature. My experience is, that the higher the temperature of the furnace, and the more coal per square foot of grate surface burned, the higher the economy.

Mr. Carpenter.—I would like to inquire how about draft, especially if you have bituminous nut or slack? My experience has been exactly the reverse—I never tried the blower.

Mr. Palmer.—The matter of draft is a very essential consideration. If you wish to burn 25 pounds per square foot of grate, you must have a draft sufficient to do it. If you only have a draft sufficient to burn 12 pounds, you could not burn 25. I am speaking of where the draft is unlimited, and where we have all we want.

Mr. J. T. Hawkins.—I imagine that the question of economy has very small bearing on this case, as it has been pretty well settled, I think, that the greatest economy that can be gotten from burning bituminous coal with the most perfect combustion, over imperfect smoke-producing methods, is about seven tenths of one per cent., so that the question of economy, I think, is hardly worth consider-

ing, compared with the nuisance of the smoke in a city; but I was going to state that I witnessed an experiment some eleven or twelve years ago, made by Mr. J. B. Hoyt, of the firm of J. B. Hoyt & Son, belting manufacturers, who has now retired from business. He built a furnace for the express purpose of trying to avoid smoke from bituminous coal, and he used coal containing the greatest quantity of volatile constituents which he could obtain. His aim at that time was also to effect a considerable economy. In this, of course, he did not succeed. He did, however, succeed in burning the bituminous coal without a particle of smoke. A young engineer named Skeel made some very exhaustive experiments with that furnace. It constituted an elaboration of the dead plate, which, as mentioned by Mr. Durfee, is about the best device for producing that result. The furnace was built in this way: the air was admitted under the grate, but the coal was put in at about five or six different points on each side. These holes were arched and kept stopped up with the coal, which was gradually pushed in from time to time. Then there was heated air let into the back of the grate, so that practically the coal was distilled at these arched openings. Occasionally they would push the coked coal when the volatile constituents had been taken out of it on to the grate bars. The arch of the furnace was low and the temperature of the arch was carried to a very high point, but in the combustion chamber at the back of the grate the flame was perfect and there never was a particle of smoke came out of the chimney.

Mr. F. H. Underwood.—I had charge of Mr. Hoyt's establishment at the time a portion of these experiments were made. We used Cumberland coal, and we succeeded in doing away with all of the smoke. The furnace was constructed as has been described by the gentleman.

Prof. C. I. King.—I discovered a boiler some time ago which was set with grate bars and fire-doors at both ends, the grate bars at the back end of the boiler being slightly above those in front. The gentleman who was using it told me he always started his fire at what we call the back end of the boiler, and then started it afterward at the front. He claimed excellent results. I did not see it in operation, as the establishment was using water power at that time. I thought that some of the other members of the Society might give us a little information with regard to this style of setting of boilers, and what the results may be. This gentleman, as I said, claimed most excellent results in economy and a decided ab-

sence of smoke from his chimney. I do not suppose this to be new, as I have received information that it has been in use to a slight extent in England.

Mr. Harding.—I had occasion to doctor up a battery of boilers in my charge. I found that the distance between grate bar and shell was fourteen inches. It was down on the river, down where they were cursed by the old steamboat practice. They reset the boilers with their false front, and gave them twenty-six inches between shell and fire. No patterns were changed in the fire walls. We found a gain in economy between twenty and twenty-two per per cent. We were doing very badly before. It shows what an odd ten inches will do between the shell and the fire.

Mr. H. P. Minot.—I think Mr. Babcock has taken the right ground. We want firemen who know their business. No doubt, in many cases where parties are building a chimney or stack, they do not build it high enough and large enough to get draft enough. I think if we investigate the matter at the West Side Water Works here, we will find that the engineer is pretty well up in the way to fire his boiler, and keeps his firemen where they should be. I think we ought to have firemen go through a school of instruction somewhere, and understand how to handle their fires properly.

Mr. John Walker.—Would some of our members posted on boilers tell us, why a gas producer placed immediately in front of a boiler would not be an economical arrangement for getting up steam and abating the smoke nuisance?

The Chairman.—I think Mr. Walker has touched the key-note of this thing, and as there is no punishment at the present day for prophesying, I will venture to make a prediction. It seems to me we have the clue given us in two recent usages, one, to which we were directed first by that great English engineer, Siemens, and the other by nature; and I may add also that the New York Steam Heating Company is helping on my argument, which is, that steam under certain circumstances can be carried in mains, just as gas and water are, economically. We see in Pittsburgh that gas can be distributed in the same way for industrial purposes. Siemens told us how to take coal and make gas of it and get rid of smoke. Why not convert our fuel into gas at convenient points outside of large cities, and distribute it by mains, just as we do our illuminating gas, and apply it under furnaces of proper construction where the gas would be entirely consumed and no smoke result from it? Perhaps the next time this Society holds its meeting in Chicago you

will find that process in operation here and the smoke nuisance gone.

Mr. Babcock.—The matter has been experimented on extensively. We have ourselves made a great many experiments in that line, without any great satisfaction, owing to the difficulties of making a producer in connection with the boiler. If, however, the gas be made in a separate producer and cooled so as to be carried in pipes to the point of combustion, a large share of the value of the coal would be lost before it reached the boilers. Water gas is the great cure-all or do-all in these days, but water gas cooled contains just about one-half of the heat units of the coal from which it was made. Therefore, if we use the gas after it has been carried in a cold-blooded manner through the streets we shall have to content ourselves with having expended one-half of the value of the coal in making the gas, leaving the other half only for use under our boilers. An experiment of that kind was tried here in the city of Chicago at a large brewery, and it was found that the economy was so bad that they had to return to firing beneath the boilers in the ordinary way. Gas-fired boilers are very successful in Pittsburgh and other places where they have natural gas, and also in connection with blast furnaces where they have gas to throw away or use as they prefer. But there is no hope whatever, for mechanical purposes like steam making, in using gas made at gas works and carried through pipes, for the reason that so large a proportion of the heat units have to be used up in the production of the gas.

Mr. Durfee.—I did not suppose that the question would permit of the consideration of the use of gas. I thought that it would be confined entirely to the prevention of smoke arising from the use of coal burned directly under the boiler on grates. Had I supposed the discussion would take the range it has, I would have stated when up before that in the years 1868, 1869, and 1870, I used gas made in the Siemens producers, under flue boilers. In the works under my charge at that time, there was a battery of eight producers, which made the gas for the whole works, and a sufficient quantity of that gas was taken for consumption under the boilers. There was no difficulty at all in making the necessary amount of steam without any appearance of smoke from the top of the stack. About the economy, it is impossible for me to state, because the consumption was from the general amount of gas produced. We did make a saving in this way: we had no firemen especially employed to take charge of the boilers; the watchman

at the works lighted the fire in the morning, and when the engineer came the steam was ready, and he looked after the gas during the rest of the day, and on leaving at night he shut the gas valves; and of course there were no ashes to pull out or clinkers to remove, or grates to clean, or any work of that kind.

Mr. Walker.—My suggestion was to have the gas producer immediately in front of the boiler and to take the place of the present furnace, flames and gases only passing under the boiler. It occurs to me that this plan would be compact and economical (as gas producers certainly are economical), and there would be less loss by radiation than if the gas producers were some distance away.

*Mr. Geo. H. Barrus.**—The discussion on this subject at the meeting gave me a strong impression that there were no smoke preventing furnaces in use in Chicago. I was afterward much surprised during my visit to some of the factories in the city to find that this impression was unwarranted. I do not know how extensively such furnaces are in use there, but I can positively affirm that there is at least one furnace in the city that burns Illinois nut coal with absolutely no smoke. This furnace is at the Norton Mills. A description of what I observed and learned there may be of interest.

There are two horizontal return tubular boilers, with shells 65 inches diameter, 16 feet in length between heads, each containing 56 4″ tubes. The grate surface in each furnace has an area of 24 square feet. The furnaces are fitted with automatic coal feeders. The coal is shoveled into chambers built in the walls of the setting, and extending the length of the furnace. The chambers are somewhat higher than the grates, and they have inclined bases, and each has a side opening at the bottom leading to the furnace. The grate is inclined downward from these openings on each side to the center of the furnace, and the automatic device takes the coal from the openings and gradually crowds it over upon the inclined grates.

Each furnace is roofed over with a fire-brick arch, above which is a passage through which air is admitted above the fuel. This air enters the furnace at each side in a long stream just above the entering coal, and the amount which is admitted may be regulated by a damper in front. The rate of feeding of the coal can be varied by appropriate mechanism.

The principle of the furnace is a gradual and uniform supply of coal, the gradual coking of the coal as it enters, and the proper sup-

* Contributed since the meeting.

ply of air above it for the combustion of the gases. No coal is thrown directly on the fire by hand. The appearance of the furnace space immediately on opening the fire door is similar to that of a Siemens furnace, though not so hot. The outside of the boiler shell is free from deposit and the flame space at the rear end is as transparent as it would be with anthracite coal in the furnace.

To show the effect of firing coal directly on the grate by hand, observation was made of the discharge from the top of the chimney when working in the normal manner. There was no smoke under these conditions of ordinary use. In thirty seconds' time, after three shovelfuls of coal had been fired directly upon each grate, smoke appeared at the chimney and gradually increased in quantity and density, till it filled the chimney outlet with a cloud of the blackest color. This cloud rolled away from the chimney several hundred feet before it dispersed. In the course of two minutes' time the quantity and density began to diminish, and the smoke gradually passed away until it had absolutely disappeared as before.

The boilers supply steam to a Harris-Corliss condensing engine, having one cylinder $42\frac{1}{2}$ in. diameter, $36\frac{5}{8}$ in. stroke, making 67 revolutions per minute. It drives the machinery of a flour mill on the roller process, which makes 1,445 barrels in 24 hours.

The engine-man reported that on a run of 20 hours with the engine developing 393 I.H.P., 31,800 lbs. of Streator nut coal was burned (containing 3,225 lbs. of ash) and 217,812 lbs. of water fed at 35° was evaporated. At this rate, the boilers evaporated 9.26 lbs. of water from and at 212° per lb. of combustible, and the engine used 19.1 lbs. of feed water and 2.79 lbs. of coal per I.H.P. per hour.

No. 222-21.

"How do you make successful foundations for structures upon yielding earth?"

DISCUSSION.

The Chairman.—We moved this question forward on the docket, in order that it might come up to-night, for the reason that we have present with us a gentleman of Chicago, Mr. Jenney, one of the leading architects here, who has had special experience in this matter, and who, I hope, will respond to an invitation to give us

some light on the subject. Mr. Jenney has erected many buildings in this city, notably the new Union League Club Building, and the Home Insurance Building, in speaking of which he surprised me yesterday by telling me that in the starting of the works an allowance of two and a half inches was made for the settlement of the building during its completion. In other words, the whole building goes down two and a half inches while it is being erected. I hope Mr. Jenney will favor us with some information on this point.

Mr. W. L. B. Jenney.—Formerly Chicago was very low and flat. We do not claim to have any very high hills here yet; but there has been a great deal of filling, notably after the fire, when material was abundant. Now, underlying our streets about 11 to 12 feet below the sidewalk in this business district where we are now, and upon which all these very heavy buildings are erected, is a bed of clay many feet deep. That clay is very soft and wet, about the consistence of soap grease on a mild day. The upper two or three feet is partially dried, and by courtesy is termed hard-pan, and the top of this so-called hard-pan is the best stratum on which we have to build here. (Fig. 267.) That is not very hard. We dare not load it with more than 2 tons to the square foot.

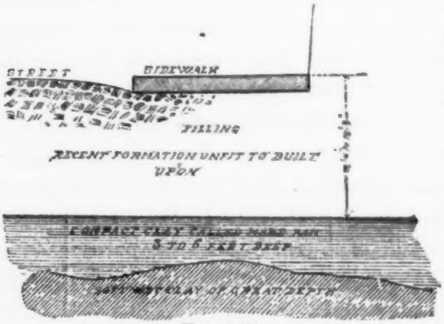


FIG. 267.

Now, any building upon these foundations must naturally settle, and, as Mr. Towne has stated, we calculate that a heavy building will go down say two and a half inches, and therefore the grade at the outset is placed that much above the definitive grade. Now, as this building must settle, the great point is that it should be made to settle uniformly. Therefore we must calculate our weights and their disposition so that they should be distributed uniformly over the foundation. The method we employ here with most success is what is known as independent piers—that is, that each pier that goes through the basement, or each wall, or each column, must have its own independent foundation; and where these foundations, as most of them do, come close together, the concrete base is separated by vertical 2 inch planks, that is, so that each one shall

settle separately from the other. If we should put two together, as one side of the construction may go down more rapidly than the other, they might be torn apart on lines not desirable. Now, there are several difficulties; one is to know exactly what the weights are. We know what the permanent weight of a building is. We calculate the building for the foundations entirely independent of the general construction. The foundations are calculated first for the empty building, and then we add what we deem, to the best of our judgment, is going to be the permanent weight. In an office building we put it very light, say 12 pounds per square foot for all we call the live load, that is, the furniture and people in the building; for if we overload or underload it is equally bad. If we should calculate a room filled like this to-night for every floor, why that weight would be there very seldom, indeed, and the columns

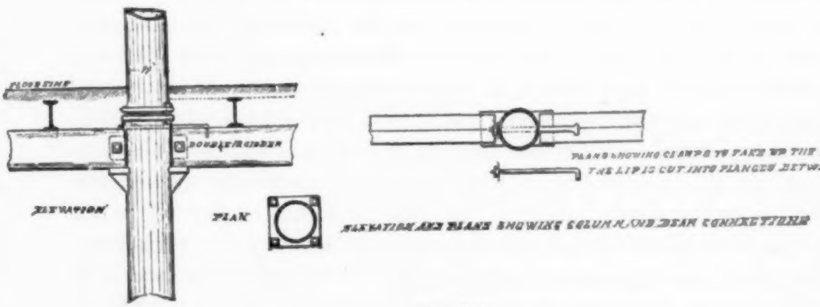
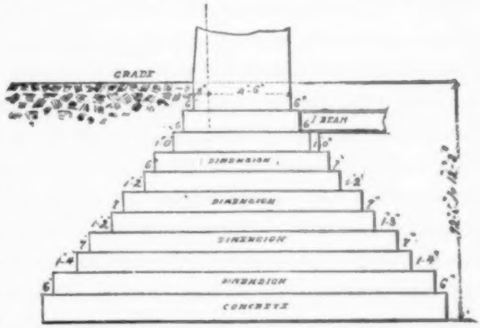


FIG. 268.

in the center of the building would stay up while the walls would go down, and the floors would soon be very unsightly. Another point: it is very difficult to make our buildings, while in the process of erection, receive a weight which is uniform over all the foundations at the same time. The building should be designed so as to do that as far as possible. Wherever any settling is likely to take place, the walls must not be united, they must be separated; and if they have to be tied together, or anchored together, the anchors must be made to run into slots so that they may go down independent of each other and without injuring one or the other. This necessitates also a great deal of anchoring. A building of eight or ten stories high will settle two and a half inches in the first year, and it must be made to do that settling so as not to injure itself; and as it is impossible to carry your construction along so that every part shall be uniformly disposed in regard to its

weight, the anchoring must be very complete. We build in here quite generally, wherever there is the slightest indication of a weak point, over the archways, at all the corners, etc., very long strips of hoop iron, three inches wide and three-sixteenths of an inch thick, turned up strongly at the ends, and the brick or stone pounded in with close joints at the end, so that all this hoop iron shall hold that building together. (Fig. 268.) Now, the Home Building was a decidedly heavy building. It was fire-proof in every particular except the top floors and the doors and a few little trimmings, and it was made decidedly heavy, calculated to be very substantial. The piers are square iron columns. Every one of those columns carried the end of a girder, so that we knew exactly where the weights were going. The illustration shows a section of the iron column in the piers; the brick was then built around it. The pressed brick on the outside is only four inches thick, just sufficient to protect the columns from fire. On the inside, brackets or corbels carried the ends of the girders. Then between these columns, over to the next pier, resting on the brackets on the square columns, and above the windows, were iron lintels, these all bolted together, so that you have a complete system all over the building on each floor, by which it is thoroughly tied in all directions. Wherever an I-beam crosses a girder, or an I-beam comes in contact with an I-beam, they are bolted together, so that we make each floor a complete net-work, and if one part goes down more than another, it is held by the balance of the whole building. In ordinary systems it is necessary that we should find our center of gravity and make that the center of our footing. With the front piers, for example, there is not room enough to make them square, therefore we put in our plank to limit the concrete footings. They are much longer out toward the sidewalk than they are to the right and left. Where walls and columns come near together and there are vaults, etc., so that they cannot be separated in their footings, then arrangements must be made to group them, and arrange those groups so as to get our weights distributed as uniformly as possible, for if we get a weight greater per square foot in one place than another, in this soil, we have trouble. In the Home Building is first a bed of concrete, say 3 feet thick, separated wherever it comes in contact with another bed by vertical 2-inch plank, so that they may slide independently of each other. Upon that we put dimension stone. (Fig. 269.) This city is very well provided with dimension stone. We put in a dimension stone directly on the concrete

with no dressing. The surface as it comes from the quarry is substantially even, but still not even enough to put another dimension stone on top of it. The bed would require to be dressed. As that is an expense, we "step in" here with large rubble. The rubble can be placed directly upon the dimension stone without dressing. On top of this course of rubble stone we now put another dimension stone, because we can set that with a coating of cement between. Here we can make an offset of about say four inches in the dimension on the rubble; so we get first concrete, then dimension, then rubble, then dimension, making an offset of rubble on dimension of 12 or 13 inches, and of dimension on rubble of about 4 inches. Where the columns come we give a very large and very heavy bed plate of iron.



SECTION OF PIER
FIG. 269.

In answer to Questions.—This hard-pan that we build upon is our best surface, and we do not like to wound that at all. It lies under this building about 11 or 12 feet below the grade of the sidewalk. That is just about where we would want to go unless we have very broad footings. If we should remove that in any way we would diminish the quality of our foundation, and then we hardly know what weight we dare to put upon it. Another point, and that is there are some light walls—we cannot help ourselves—places where we carried them down without any footings at all, and yet these would have too much footing for the very little weight upon them. Whenever this occurs we have to cut them off from the other portions of the structure. They will not go down, where the other portions of the building will. Last November, at the meeting of the Institute of Architects, I was there invited to describe with drawings this new Home Building. The description was published in the *Sanitary Engineer*, December 10, from which the illustrations are taken, in which any one who cares to may find a more detailed description.

Mr. G. E. Palmer.—I would like to ask Mr. Jenney what expe-

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rience he has had in regard to placing iron on this bed of concrete above the hard-pan.

Mr. Jenney.—Personally I have not used that much, because when I could use dimension stone and rubble, I would prefer them. There are times when we have to use the basement for such purposes that this dimension stone and rubble put in as usual would incommode us, and we use railroad iron, which allows an increase of the width of the offsets. This can be seen within what is called the Rookery Building in this block. I would say in regard to the success of these independent pier foundations, that in the Home Building the calculations were made over and over again by some three different assistants, and careful leveling shows that the building had gone down at the time of completion two and a quarter inches out of the two and a half allowed, and with inequalities of less than three-quarters of an inch which were so generally distributed about the building that they could only be detected by an engineer's level.

Mr. Oberlin Smith.—I would like to ask Mr. Jenney about how thick this layer of clay is, and how far it is down to bed rock, if there is any?

Mr. Jenney.—In some instances I have known them to sound it sixty feet and then they stopped, because they thought it was no use to go farther. The artesian wells show the sections, but I have not them in mind. It is of great depth; practically it is unlimited, because there is no possibility in our buildings of using anything below the clay. There are occasionally boulders of stone in it, occasionally little pockets of sand. Under our Post Office there was an old slough and there were pit holes. When we find those we must go to the bottom of them. In one instance I was obliged to go down with the concrete at one angle twenty feet below the other portions of the building.

Mr. W. F. Durfee.—The importance of the consideration which has been presented to us by Mr. Jenney so plainly, I have seen very well illustrated in a very expensive building which is now being erected in an Eastern city. In the design of this building there was contemplated and it is now in process of erection, a very lofty and heavy tower. That tower, on the right and left flank of it, as you face the tower from the interior quadrangle of the building, is united to the main walls of the structure. The tower is now one-half of its intended height, and has already settled so much that the blocks of stone in the adjacent wall of the building proper are

almost the whole of them cracked in numerous places. The window caps and sills are very much dislocated, and some of the lighter portions of the building show evidence of being forced up by the upward movement of the clay soil on which the tower rests. The tower has forced substance from under it which is actually lifting parts of the adjacent structure.

Mr. J. W. Cole.—I would like to ask Mr. Jenney if he has in mind any unsatisfactory experience in using piles?

Mr. Jenney.—This soft clay of great depth is not well adapted to piling. Besides piling is necessarily more expensive, and we have found that it is not necessary for a fire-proof building of ten or eleven stories high.

Mr. Palmer.—I would like to ask Mr. Jenney if it is not a fact that the county part of the county building of Chicago was put upon piles, and the city part upon such a foundation as this, and if it is a fact that the county part has settled so much that it has nearly ruined the building?

Mr. Jenney.—I cannot speak from any actual information, except hearsay. It is universally reported that one-half of that building is on piles, and the other half not, and that there has been some very bad settling. If we load this soil with more than two tons a square foot, or if we get it uneven, we are certain to have trouble.

Mr. Durfee.—There is a very simple and cheap method of getting foundations which is applicable under many conditions of soil, particularly if it is a clay soil resting upon a solid bed of gravel, but with clay so deep that it would be too expensive to remove the whole mass down to the gravel. By boring vertical holes with a common post augur through the clay at about as frequent intervals as you would naturally drive piles, and filling those holes with dry sand, poured in and compacted by a pointed stick, you get a very satisfactory substitute for piles, and I have used that construction in two or three instances under the conditions which I have named with entire satisfaction.

The Chairman.—We are all very greatly indebted to Mr. Jenney for the very interesting account which he has given us of this mode of construction, which I am sure must be new to many of us, and his remarks show how modern mechanical operations overlap in the different so-called groups. Mr. Jenney might just as well claim membership in our Society of Engineers as in the Institute of Architects. His work is as much of an engineering as an architectural character, as is shown by what he has told us to-night.

No. 222—22.

"Would the use of an annular jet in an ejector induce a greater current of the fluid to be moved than if a solid jet was used of the same area?"

DISCUSSION.

Mr. John Walker.—I am of the opinion that the annular opening would have more friction than a plain, round opening of same area, and hence the latter would have the least resistance.

Mr. Jesse M. Smith.—I think that since the circumference of the outer circle is greater for the annular jet than for the solid jet, and since the air which is forced out is a question of entrainment by the steam of the air, that the larger the surface of the steam jet the larger would be the quantity of air delivered. The annular jet would be thereby the more effective. If it were a case of an injector for raising water for a very large jet I should think that the condensation of the steam by the water would be more effective in the case of an annular jet, because the stream of steam is thinner and the water would come in contact with it more readily and condense it more rapidly. For small sized jets I do not suppose there would be much difference. But for an air pump or an air exhauster, I should think that the annular jet was preferable.

Mr. W. F. Durfee.—In 1869 I constructed a filter pump with an annular jet, and the reasons that influenced my decision in the matter of its design were precisely those stated by the last speaker. The filter pump was intended to work four filters, and it performed that office perfectly well. The thickness of the jet of water was one-sixteenth of an inch; its internal diameter one-half of an inch. I am not able to say whether a solid jet would have worked any better or any worse, but my impression is very strong that an annular jet for such a purpose is the better form.

Mr. Walker.—I would like to call Mr. Smith's attention to the fact that this is an ejector, not an injector.

Mr. Smith.—I recognize the fact that it is an ejector, and I think that the surface of the jet, whether it be of steam or of air, would be better if annular. If it were a jet of air forced by pressure through an annular nozzle and it was designed to entrain more air and make a blower so as to deliver air in a larger quantity and at a lower pressure—even then I think that an annular jet would

be better on account of having a much larger surface for entrainment of the surrounding air.

Mr. Durfee.—As an adjunct to the filter pump described, I provided a reservoir into which the air was discharged and from which it would be delivered into a blow pipe, so that the same apparatus could be made use of as a blow pipe without any inconvenience. Any one who is interested in seeing that apparatus will find an illustrated description of it in the thirteenth volume of the papers of the American Institute of Mining Engineers, to which I contributed it.

Mr. G. H. Babcock.—With air I have no doubt that an annular jet would be more effective, as the action is simply one of friction between the two fluids, the energy of the escaping steam being imparted to the air by contact and friction. This is illustrated by the blowers which are put into the smoke stacks of steamboats. It is found that the most effective blowers, using live steam, are made of a ring in which are a large number of small holes, so that there is a large number of jets, forming practically an annular ring of steam. But I imagine that the conditions would be very different if water or any other fluid capable of condensing the steam was to be moved. In that case there is a direct transference of the momentum of the steam to the water upon the condensation of the steam, its energy being imparted directly to the water. I think it would make no difference in that case whether the jet was annular or sub-divided in any way. The question is how much steam at a given velocity is condensed in a given quantity of water.

Mr. J. T. Hawkins.—It seems to me that the annular jet is going to be more efficient because of its greater surface. If we have the air entering up from the center as well as on the outside, we have a great increase of contact, after the manner of the Argand burner. The Argand burner furnishes oxygen for the flame more completely than a solid burner, and I should say it would act in the same way for a jet. I know that on some of the United States steamers during the war they had that same kind of jet to which Mr. Babcock refers, and in that case the air passed up inside of the ring as well as outside. But if it was closed in the center, I should say it would not make so much difference.

Mr. Smith.—I would say that Giffard who first called attention to this form of injector states that an annular jet and a solid jet of the same area are the equivalents one of the other, and that the liquid jet may be either the central jet and the steam the annular jet

or *vice versa*. That, of course, applies to steam raising water where the steam is entirely condensed by the water. But where air or steam is used to entrain air or steam or any other gas or vapor, the question is quite different. It is simply there a question of entrainment and not of condensation at all; so that I think that the annular jet would be more efficient for entraining one gas or vapor by another gas or vapor, while the forcing of a liquid by steam might not have so great a difference.

Mr. Babcock.—I do not know that it is proper to branch off at all upon the question, but there is a point in regard to the action of such jets, particularly for chimneys, which is of interest.

The Chairman.—I think that would be entirely pertinent; I think it is the object of these discussions to bring out minor points of practice or experience that the society would not otherwise obtain.

Mr. Babcock.—Let us suppose a smoke-stack of ordinary proportions. If a steam jet be put into that stack near the base it will have a certain effect in producing a draft. If we should now bring our jet near the top of the stack it will have a very much greater effect. The reason is, that the steam, escaping under a considerable pressure immediately expands and occupies a large volume, and the additional velocity required for carrying the air and the steam together through the pipe, increases the friction so greatly as to modify in a considerable degree the effect of the jet. In fact, using almost any amount of steam at the bottom would produce scarcely any increase in the amount of draft; whereas, if you bring it to the top, so that it has a chance to escape, and just about fill the chimney by the time it reaches the top, it produces the best effect.

No. 223.—23.

"Moulds for printing-press ink-rolls. What are the best known conditions covering the moulding, mixing, melting and casting of iron for the successful production of such tubes?"

DISCUSSION.

Mr. J. T. Hawkins.—As the propounder of that question I might state the facts with reference to it as nearly as I know them. The article in question is simply a tube of cast iron, varying from two inches internal diameter up to as high as five or six, but generally

two and three inches. We make them about half an inch thick, finding that to be the best practice. They run up as high as seventy-two inches in length. The experience in making these tubes so they shall be perfectly free from defects inside, after they are bored and polished as they have to be, seems to be of the most erratic character. That is my experience, and so far as I can find out, it has been the experience of pretty nearly everybody. In the last two or three years we have tried all the principal establishments in the United States, and none of them have hesitated to say that they could make them perfectly, and they all have failed. We succeeded in making them for a time in our own foundry, and have at times made as many as eight or nine hundred without getting a bad one, and without changing the conditions in any way whatever so far as we could tell, we have made eight or nine hundred bad ones. I hope there will be some gentleman here who can throw a little light on that. The object is to produce a tube of cast iron about six feet long which will polish inside; we don't care about having a perfectly high polish, but without any pin holes or blow holes or defects in it, and having it come out with a reasonable degree of certainty that you can do the same thing every time. If we could confine the loss within twenty per cent. we would not complain; but as I say we have made eight hundred or nine hundred good ones out of a thousand, and then we have got eight or nine hundred bad ones out of a thousand. We are making them now in our own foundry and getting about seventy-five per cent. good. At the same time we have another foundry in Brooklyn, where they are supposed to be thoroughly posted on this question, and the proprietor has had long experience in making these very things for a well known printing machinery firm, and claims that he was uniformly successful, and we gave him an opportunity to make some of them. He had an order for a hundred: He has supplied about seventy-five, and of what he has supplied up to this time, about sixty per cent. has been bad. Of course they want to be pretty near to perfection. A pin hole or a blow hole of an eighth of an inch in diameter is sufficient to render one of no use. We, of course, find in those of the castings that have been cast on end in dry sand that the upper part will be defective to some extent, and we cut off two to three inches and then other defects will appear further down. Some we cut off and make shorter ones, but in these cases I speak of they will be full of pin holes all the way down, and we have to throw the whole thing away.

Mr. T. D. West.—I would like to ask the gentleman the manner in which he pours, in his own shop.

Mr. Hawkins.—I am not in a position to state critically just what the operations were, because I have not been able to witness them myself. I understand, however, that they pour the metal in through one opening and have no escape. They get the best result from one single hole into the mould in the top, allowing the gas to escape from the same hole through which they pour. These people in Brooklyn pour into a conical dish in the top which has a number of small holes in it. The gas escapes through these same holes. I resorted to making them in halves and did succeed in making some pretty good ones in halves and devised a means of disguising the joints, but after being used a little while and being expanded and contracted it would make the seam become objectionable and we had to give them up.

Mr. West.—In view of the fact which the gentleman has stated, that so many of our best founders throughout the country have tried to make them and failed, I would not attempt to say here how they should be made successfully. Where I could go in the shop and see the material which was used and how the moulding was done, I could then talk understandingly, and perhaps point out the cause of the trouble. I might just state a few points which might be a guide in avoiding the trouble. There is one conception which many form in reference to dirt or blow holes in castings—that they come from the iron. The iron itself is literally clean when in a liquid state. If there is any dirt or holes in a casting, we may expect that they come from the mould, either by blowing or scabbing, or some dirt getting into the mould through the gates, and if the rolls can be procured at all clean and free of blow holes, the only way to accomplish it, as far as the mode of casting is concerned, would be by casting them vertically, unless they were cast horizontally and plenty of stock left on the under-cast side of core to allow for boring out any dirt which might be caught and held by the under side of the core as the mould filled with metal. One gate, in pouring vertically would be objectionable. The best plan would be to have a small gate, admitting the iron in at the bottom, so that it would rise about a foot in height and by the time that it had obtained that height then to drop from the top through small gates distributed around the circle. I would not advise any outlet gate for gas at all. Dropping from the top would agitate the iron and any dirt would be floated towards the top of the casting.

There is one great trouble with dry sand work or work which has a core in it, and that is the blacking. They will mix blacking of such a character that it will be so close that the iron, when it is in the mould, will not lie kindly to it. It will blubber and boil, and the result is that it kicks the blacking off the mould. Or it will be so weak that the friction of the rising metal will wash it off; these scales mix with the iron and give us our dirt and are what a great many consider due to poor iron. There is, generally speaking, no dirt in iron when it is liquid. It comes from the gates and mould or the surface of the metal when slaggy, and in oxidizing when it is exposed to the open air.

Prof. C. I. King.—I would like to ask the gentleman if he uses dry sand cores?

Mr. Hawkins.—I would say we have and in various ways. We have built them up on a rod, and we have made them up on gas pipe, and we have made them from loam. That did not work quite as well, although we got pretty fair success with a loam core built up on a pipe.

Mr. King.—I know of one difficulty of this kind being overcome on very similar work, except being larger in diameter, by using a core which was made up of very coarse sand. It often had gravel stones in as large as the end of the little finger. This was in the manufacture of printing-press cylinders from 12 to 14 inches in diameter, and where they were losing about fifty per cent. of the cylinders before they used this kind of core. It is now very seldom that they lose one at all.

Mr. Hawkins.—With reference to that, I would like to say to the gentleman that of course in printing-press cylinders these defects would be on the outside. In this case which troubles us they are on the interior, and my impression is that they would be perfectly sound on the outside. In the case of the larger cylinder it would not apply. The great trouble in this case is in getting a sound interior.

Mr. John Walker.—I should judge from the description given of the difficulties in producing these tubes that the iron must have been of different qualities, and that producing the bad castings was highly sulphurous. The facts that a large quantity of the castings are produced good at one time, and as many are defective at another time, that the moulds have all been prepared alike, and the little holes in the castings have not been from dirt, confirm my opinion that the iron has been at fault.

Mr. Hawkins.—We have not been able to tell what was the cause of the holes. We could not tell very well. They go into the boring mill and the operation of boring would tear the dirt out, if dirt, or leave them visible if blow holes.

Mr. Walker.—The illustration (Fig. 261) shows a new method of pouring castings, which I have recently introduced. The casting illustrated is a cylinder 30″ bore and 8 ft. long, cast a few weeks ago, perfectly clean; the top flange was as clean as the bottom flange (when faced) and was cast as shown without a riser head. The mould was prepared in loam in the usual manner. At the bottom of the runner, and in the gates are prepared conical loam or core seats to receive corresponding conical iron plugs or valves. Placed at one side, or on top of mould, as may be desired, are automatic risers, which are cup-shaped cores with horizontal outlets. Placed in the core and across these outlets are pieces of sheet lead $\frac{3}{32}''$ or $\frac{1}{8}''$ thick. The proper level of the under side of this riser is 2″ to 3″ above the bottom of runner, so that when the mould is poured, the metal will flow through the riser, leaving 2″ to 3″ of iron in the runner. In operating these plugs and risers, the ladle is skimmed, but no skimming is necessary when pouring. The pouring is started slowly until the runner is well filled, when the plugs are pulled out vertically, one or two at a time, making the intervals of time to suit the amount of metal in the runner, and the speed at which the casting ought to be poured. It is best to have a couple more gates than the casting would ordinarily need, and open them if occasion should require, such as the casting pouring too slowly or the iron being duller than was intended. It is evident when the plugs are pulled, that the metal will flow from the bottom of the runner, and nothing but the pure liquid enter the mould, the scum and dross remaining

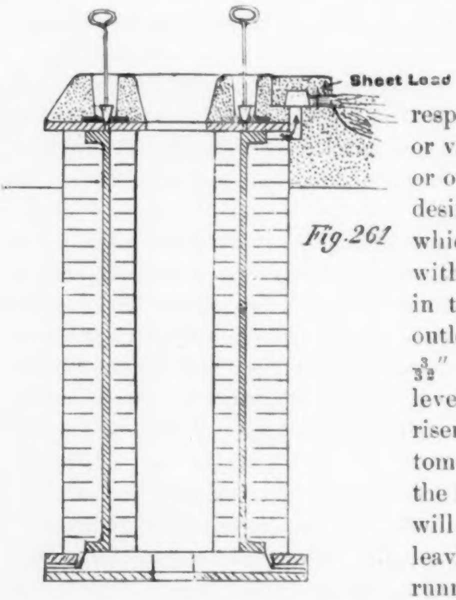


Fig. 261

on top. We are all aware of the troubles caused when risers are opened, the air rushing through the mould tearing off portions of its surface. The automatic riser described here effectually prevents this, it being held down air tight. As soon as the iron flows up into the horizontal outlet of the riser and comes in contact with the lead partition, it melts it and the iron flows through in the usual way, needing no attention. This method of casting has proved so successful that I think it may help Mr. Hawkins in getting perfect castings.

The Chairman.—Does the metal flow to the bottom of the mould?

Mr. Walker.—Yes, sir.

The Chairman.—Do you have any trouble from the coating?

Mr. Walker.—None whatever. The mould being deep was well prepared and thoroughly dry. The iron went directly into the mould, falling from bottom of runner to bottom of mould, a distance of 8′ 6″. The top flange was perfectly clean, showing that when the iron goes into a mould clean and there is no disturbance in the mould, we get a clean casting. I think if the moulds in Mr. Hawkins' case were perfect, and no pieces came from the mould or core, then the trouble was in the metal itself. I think the iron used in the defective castings must have been highly impregnated with sulphur. This class of iron cast in the form of a plate will, when planed, show innumerable small globular or semi-globular cavities on the upper side of the casting.

Mr. West.—These sulphur holes in iron are a good excuse to help the moulder out. In my experience I have found that the point brought forward by the gentleman in reference to the openness of the sand has a great deal to do with such castings as these rolls or pipes, in preventing the sulphur holes, as the moulder would have it. It is the same thing as what I spoke of with regard to blacking. In mixing the blacking they may get it so close and hard that the iron would not lie kindly to the core. Now, the same conditions must be followed through with the sand. The sand must be made of such a porous character or so well vented that it will allow the iron to lie kindly to it, and when you cast anything on end so that you have a good "head pressure," it is going to force the gas out through the mould, providing the same is open enough to allow the metal to lie kindly to it. I cannot agree with Mr. Walker in reference to these being sulphur holes. I think the whole trouble is due to the fact that the iron does not lie

kindly to the core or mould. If you can pour a mould so as to have the iron come up without a blister, you are pretty sure of a good, clean casting, if you don't allow dirt to get in through your gates. That plan of stoppers for gates is a system which I have used a good deal with reference to casting big flat-surfaced dies, in helping to get the face clean. There would be a big basin made and a plug like that would be used to stop up the gate. The plug would be drawn out as soon as the basin was filled and the iron would go in with a rush, and cover the face of the chill all over. That would give a body to the chill sufficient to prevent what we call "cold shot," but when we start with an open gate the iron goes in slowly on the face of the chill and "cold shots" from the fact that there is not body enough there to cover the face of the chill quickly. That principle which Mr. Walker shows there of a stopper would be a very good one to get clean castings. I have made a good many cylinders in my time, but never experienced any trouble with reference to what moulders like to jump on—sulphur holes. There may be such a thing in the iron to cause holes, and some claim that a little manganese mixed with the metal will help that defect; but, as I said before, I cannot fully concur with Mr. Walker in laying the blame of the trouble with Mr. Hawkins' castings on to the sulphur. The trouble, I feel safe to say, lies entirely with the mould, and not the iron.

Mr. Walker.—In answer to Mr. West I would like to remind him that the position I took was this, that the moulds were practically all alike. Mr. Hawkins explained that the moulds of the bad castings were made precisely the same as the moulds for the good castings. Is that not so, Mr. Hawkins?

Mr. Hawkins.—Yes, sir.

Mr. Walker.—The same material being used, the same workmen probably, and the same conditions throughout, I see no reason why (if the same metal is used) they should not have the same success one time as another. It seems so strange that they should have a run of seventy-five per cent. good for a time, and then a run of seventy-five per cent. bad. I know of nothing which could produce those small holes, perfectly clean and globular in shape, but sulphur.

Mr. West.—I would just state, Mr. President, that Mr. Walker may find several moulds alike, but I have had considerable difficulty to find moulds twice alike.

Mr. Babcock.—I imagine a good deal of the difficulty which Mr. Hawkins finds is in the moulders themselves; more likely to be

there than in the iron. In the production of duplicate castings of an intricate nature to a large extent, we find that it is entirely a question of moulders. Most of you are aware that in an article which I am manufacturing there are used very many intricate hollow castings. We have never yet been able to pick up a journeyman moulder who could mould them. We have tried a great many accomplished moulders, and they accomplished nothing. We never yet found the man, however experienced he was in moulding other things, who could get one good casting out of four moulds. Our only means of success has been in taking green hands and teaching them the art. We take a green hand, throw away all his work for two or three weeks, until he learns how to do it. He never did anything else of the kind; he does not know how to mould anything else than just that one piece, but he does know how to mould that, and he will mould four to six a day and get ninety-nine good ones—not out of four, but out of every hundred he moulds.

Mr. W. T. Magruder.—I am employed in the works which require these moulds. About a year ago a lot of seventy or seventy-five moulds were made, and of that number about ninety-five per cent. were good; the rest were bad. The foundryman, seeing his success, immediately went on and made a still larger lot of them; and of this second lot of moulds, made from the same iron and by the same moulder, and as far as possible in the same way, between sixty and seventy per cent. were bad. The iron was obtained from the same place, was of the same brand, and of the same quality throughout, so far as we knew.

Another point Mr. West speaks about is that of blacking. By cutting a mould in halves, crosswise, we find that the blow or dirt holes are in the center next to the core, and not on the outside; so that the remark has been made that, if we could use the outside of the mould instead of the inside, good rollers could be cast every time.

Mr. West.—That would prove to me, Mr. Chairman, that the trouble comes from the core. The core does not vent quick enough, or the sand is not open.

Mr. Walker.—So far as getting uniform iron from the blast furnace is concerned, I would say it is scarcely possible to get two cars of iron alike. Those who have charge of foundries know this. I am supposed to get iron of the same brand alike on every order, but occasionally I send a specimen to an analytical chemist and

find them very different. I believe we got our irons more uniform years ago than we do now.

Mr. Kent.—I would suggest that even if the moulders were all to work uniformly, and the pig iron were uniform, there may be a difference in the cupola work. There can surely be a difference of temperature of the metal when poured, and when it leaves the cupola, and also in the character of the fuel.

Mr. West.—I am still opposed to laying the trouble on the iron. I have seen so many moulders in my time trying to get off by that excuse, that it cannot be fought down too hard. In nine cases out of ten the trouble is with the moulder. In my experience I cannot recall an instance where I have had any trouble with the iron in obtaining sound, clean castings. The trouble at issue now is undoubtedly due to the mould or moulder.

Mr. Hawkins.—In closing the discussion I would like to refer to the illustration used by Mr. Walker. That shows a cylinder eight feet in diameter, which is a very different proportion from what we proposed to make. With reference to the sulphur, I can say that the greatest care has been taken to manage, as near as possible, to get the same kind of iron, but in spite of every precaution these results have been obtained. We used all the precautions that can be taken in that direction. I will say further, that the Mason Machine Works, who make these moulds, have no difficulty whatever in making cylinders of the proportion shown in the illustration. They make perfectly sound locomotive cylinders and similar castings of that kind. I also think that it is hardly possible that there is any question of moulders in this case, as the individual man who has had this work in hand—in fact, several of them—have been as much interested to bring about good results in this case as the proprietors themselves. Moreover, the foreman of the foundry has given these matters his personal attention, as well as in the cupola work. I have noticed that he has been anxious to give his whole time to endeavor to bring out good results. Nor can I see that there was any difference on different days of the week.

No. 222.—24.

"Mechanical circulation and filtration of water in steam boilers."

DISCUSSION.

Mr. Wm. Kent.—I cannot see how circulation in the boiler would prevent scale. Scale is caused generally by deposition due to the evaporation of the water. The scale materials are in the water in solution, and when the water is evaporated the scale will be deposited. Circulation may carry it from one place to another, and deposit it in a place where there is the slowest current, such as a large mud-drum, or something of the kind, but it won't prevent the scale from forming. In connection with the paper on Hyatt's filter, something was said about circulation and filtration outside the boiler. That, no doubt, will prevent the deposition of scale, and is a very good thing.

Mr. W. F. Durfee.—Some years ago in a rolling-mill boiler I introduced an arrangement of feed-water pipe, for the purpose of keeping the boilers from blowing off steam, when all the furnaces were "blowing." Instead of feeding hot water into the boilers

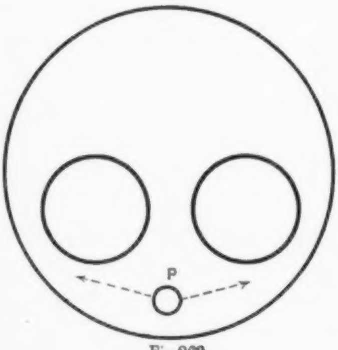


Fig. 263

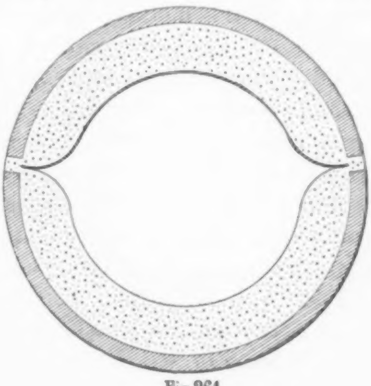


Fig. 264

from the heater at such a time, I arranged to feed cold in this way. I placed lengthwise in the boiler, and in about that position shown at P, Fig. 263, a three-inch pipe, and occasionally there was a slit sawed through the circumference of the pipe, so that the water was distributed in thin sheets in about the direction in the boiler shown by the dotted arrows. That worked well for a short time. It stopped the blowing off of the steam under the circumstances named, and made it safer and more pleasant to work in the mill. One of the "water boys" at last reported to me that he could not get any water in No. 1 boiler, and on examination we found the pipe P in some such condition as that represented in Fig. 264.

There was about half an inch of carbonate of lime deposited on the inside of the tube, as shown in the figure, and the slits were entirely closed up, the deposits taking the general shape shown. The circulation of water through that pipe increased the deposit of scale within it as compared with that on the interior surface of the boiler, and all such feed pipes had to be removed.

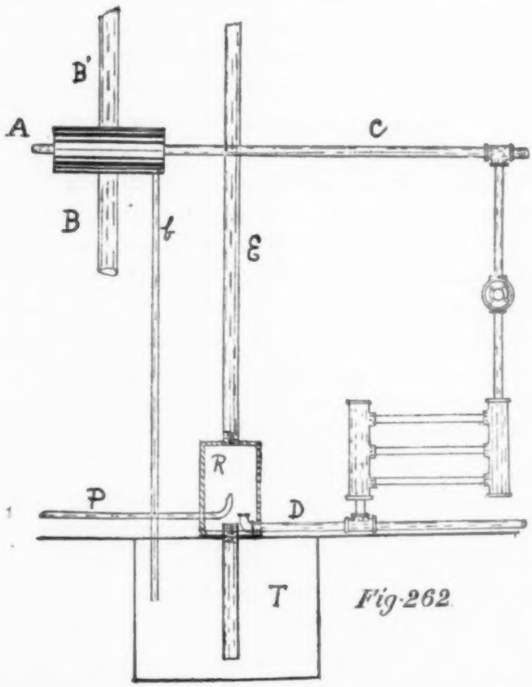
No. 222.—25.

"What is the best device to catch the water of condensation in the exhaust pipe of a high-pressure engine? Would any economy result from entrapping this water and pumping it back into the boiler?"

DISCUSSION.

Prof. C. I. King.—The first part of the problem I will not agree to solve by this method, though most excellent results are obtained by its use. The water which is used at Madison, Wisconsin, where this device was gotten up, is very similar to that known throughout the country as the Waukesha water, and it contains a very large percentage of the carbonate of magnesia, which seems to be given off with a smaller amount of heat than the carbonate and sulphate of lime. It does not stick to the boiler so tight as the carbonate of lime, but it comes in such quantities that it will cover the shell of a boiler in a very short time, and especially the flues. The idea of this device is not only to get rid of that, but to catch all of the exhaust water that can be obtained. It is used in connection, also, with a system of steam heating by the exhaust. We have at A, Fig. 262, a cylinder somewhat larger than the main exhaust pipe (B B′). The main exhaust from the engine passes in at B and passing out at B′ in warm weather. This cylinder is tapped in a number of places and pipes conducted from it throughout the building. The pipe B extends above the bottom of the cylinder about 2″, so that any water carried through it can flow back. There is a drip (*b*) from this chamber directly back to the tank (T). In the system of exhaust heating two pipes (C and D) are run, which we will say are parallel with each other, and all the water flows from pipe C back through pipe D into the tank again. One feature of this system is in using short coils, which are usually only eight feet long, and in no case over twelve. The pipe D, after it passes the

last coil of the system, simply serves as a drainer to the coils. This tank receives all the water that is pumped back to the boiler, both for a further supply and the exhaust water. In the receiver (R), where the small pipe (P) comes in, a fresh supply of water is furnished for whatever is lost during the process of condensation. There is a direct exhaust pipe (E) to the air from the chamber (R). By contracting the pipe P at the end in R, it serves as a jet, and all steam coming out through D comes in contact with that small jet,



Fig·262

and nearly all of the matter that will be deposited at that temperature is left in the cast-iron receiver (R), which is so arranged that it can be cleaned out whenever desired. The pipe C is left open during the summer and a slight circulation of steam takes place through it, but the coils are shut off so that no steam passes through them then, yet a good deal of water is carried through the mains back to the tank. It is stated that only about six barrels of fresh water a day through this system is necessary for an engine that is delivering from fifteen to twenty horse-power. The results obtained from this are exceedingly surprising to me. I

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had not the least idea that the method would heat the water sufficiently, at least in the summer, to get rid of scale, but in that receiver which held something over half a bushel, I saw at least a peck of scale taken out after a two weeks' run. The result in the boiler is, that it is as clean as any one could desire to have a boiler, and a saving is claimed of from thirty to forty per cent. over the system before in use. I would say that no other heater than this is used, and in the winter the temperature of the water in the tank gets as high as a hundred and ninety-six degrees. I have tried the thermometer in it a number of times myself, but I never found it above one hundred and eighty-five. I examined the boiler on one occasion and found it exceedingly clean, and I have also observed the water glass, and it is as clean as when it was first put up. Using this same water before this method was adopted, I found that there seemed to be an incrustation that would work out through the packings of the glass and run down on the outside of it. It is a most troublesome water in this respect. It is nearly equal to bisulphide of carbon to keep in the boiler unless the salts are deposited first.

Mr. F. A. Scheffler.—Does the oil in the water from the cylinder give any trouble in that apparatus?

Mr. King.—I suppose if we were feeding tallow by the quart into the cylinders, as we used to do fifteen to twenty-five years ago, there would be a good deal of trouble; but where the sight-feeding lubricators and mineral oils are used I think there is no difficulty or trouble whatever to be experienced from that source. So far as I know, there has been none. I have used the water of condensation from different methods than those where the sight-feed lubricator has been used, and have never known any trouble with it.

Mr. C. M. Giddings.—In a recent visit to Philadelphia, I was talking to Mr. Simpson of the Kensington Engine Works, and he said he preferred to catch the water of condensation on the other side of the cylinder. This naturally led to an inquiry as to the means he had used, and he said he had made a simple cast-iron cylinder of a capacity equal to about three times that of the cylinder of the engine. This cylinder had solid rounded heads cast in, and was placed in a line with the steam pipe, as close to the engine as possible, at right angles with the steam pipe. The length of the cylinder might be three or four times the diameter. On the opposite sides and in the center lengthwise of this receiver were necks and

flanges to which he would bolt his steam pipe. Then in the middle was a cast partition extending one-quarter to one-third of the length of the cylinder at right angles to the direction of the current of steam. He said that in a recent trial, working the steam through, I think, an eighteen-inch engine, that he had collected in the course of a three or four hours' run, three barrels of water, an amount far beyond his expectation, and one which was so great that it led him to conclude that since such a simple device would afford the means of catching this amount of water that he could not afford to do without it, and had made a standard practice with his customers, in putting up engines of the larger size, of putting these receivers in the steam pipes.

Mr. Kent.—I think that device is quite common. I knew a case in Pittsburgh where a line of steam pipe eight hundred feet long was carried into a very large cylinder about twenty feet high by three or four feet in diameter, and there was a constant stream of water running through an inch pipe out of that steam trap. But to come to the question under discussion—what is the best device to catch the water of condensation in the exhaust pipe of a high-pressure engine? I think there is no better way than just a big enlargement of the exhaust pipe, putting in diaphragms, or bends in the steam pipe itself, so as to change the direction of the steam—anything to change the direction, and make all the exhaust passages large enough so that there is no back pressure, and drain off the water from the bottom.

As to the second question: Would any economy result from entrapping this water and pumping it back into the boiler? Certainly not. If you can catch that water in the big exhaust chamber at say a hundred and ninety degrees temperature and put it back there, it is of course better to use that water than to use cold water; but it is no better economy than using the ordinary feed-water heater, which heats the water up to two hundred.

Another possible advantage, when using this mineral water, in enlarging this exhaust pipe, to enormous dimensions, is to make it an air condenser. If you have not a water supply for a surface condenser handy, then get as near as you can to a system that will convert the exhaust steam back into water, simply by air-cooling and radiation from the walls of this large chamber. In that way you will get a larger amount of pure water than if you let the exhaust all go out of the engine directly into the air.

Mr. Durfee.—Some years since I had occasion to do something of this kind, for the reason that the building where the engine was placed was on solid rock, and there was no readily available source of water supply. The water had to be brought to a cistern. I placed on the roof of the building a tin-plate cylinder, six feet high and about two feet in diameter, and across that cylinder were a number of horizontal tin-plate tubes crossing each other at all angles. The ends of these tubes were open so that the wind could blow through them. The exhaust pipe entered the center of the bottom of this cylinder and projected inside about two inches. As the exhaust steam entered the tin cylinder and expanded, its velocity was diminished, and by contact with the comparatively cold sides of the tin cylinder and the tubes which crossed it, a large portion was condensed, and the resulting water was returned to the cistern through a drain pipe connected to the lower end of the tin cylinder. The quantity of water required to be purchased was of course diminished by just the very considerable amount saved by this apparatus.

Mr. Babcock.—If this meeting is to be continued any longer I have some remarks to make on that subject, which I think would be of interest. If you are going to adjourn I will not inflict them on the meeting.

The Chairman.—We would like to hear from you, I am sure, Mr. Babcock.

Mr. Babcock.—The question of trapping water out of steam pipes is a matter to which I have given a good deal of attention, and in which I have had considerable experience.

A number of years ago in England, a device was used consisting in an enlarged bag-shaped place in the pipe with a baffle plate inserted to compel the steam to take a sudden bend. The impact of the steam on the baffle plate caused it to give up its water to the surface, but this water as it dripped from the plate was caught up again by the current and carried forward. To be of service, such an apparatus must convey the water drawn from the steam to some place where it cannot again enter into the circulation.

For this purpose we have found an apparatus like Fig. 276, to be most efficacious. A shell two or three times the diameter of the steam pipe is provided with heads, the upper one being fitted with a center opening for the exit and a side opening for the inlet of the steam, the two forming an elbow in the pipe. The inlet is not

made radial, but is so formed that the entering steam shall take a spiral motion, while the outlet is provided with an extension pipe leading to within a certain distance of the bottom of the shell. A blow-off valve, or a trap, is fitted to the bottom, and a glass gauge serves to tell the height of the water within. The rapid circular or spiral motion of the steam drives the heavier particles of contained water to the outside, where they come in contact with and run down the shell, while the steam taking a sharp turn rises through the center pipe freed from its entrained water.

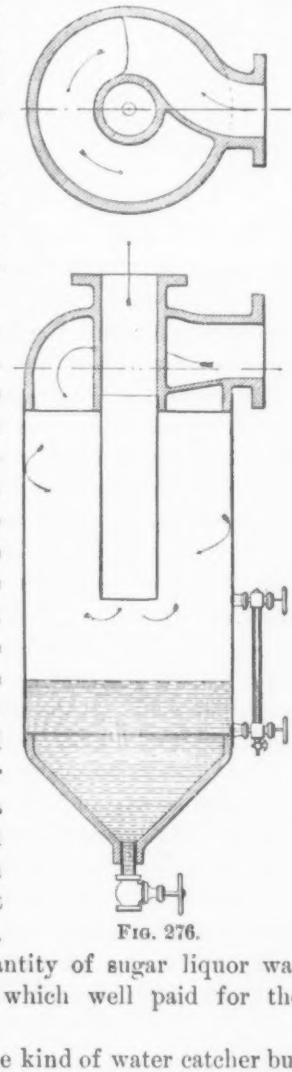

Fig. 276.

The first apparatus on this principle was fitted by Mr. Wilcox to his yacht the *Sophia*. In fact he used two, one between the locomotive boiler and the high-pressure cylinder, and the other between the high and low-pressure cylinders. It was curious to watch the action. Water would rise perceptibly in the glass until it reached a given point, when it would cease to gather. If the accumulated water was drawn off, then it would again gather to the same point, and stop as before. This point was found to be about two diameters of the inner pipe below its opening, and it was concluded that the current of steam at that distance from the end of the pipe swept up the water again as fast as it was separated. A large separator of this kind was placed upon a triple-effect in the Chicago Sugar Refining Co.'s. It was found that a noticeable quantity of sugar was carried away in the vapor from the vacuum pans, and this was attached to the outlet for the purpose of saving that sugar. The result was that a considerable quantity of sugar liquor was retained by it, resulting in a saving which well paid for the trouble.

Mr. G. Schuhmann.—We use the same kind of water catcher but we just reverse the thing. We take the steam in the center pipe

and let out at the side. We do not get any centrifugal action, but the water is all deposited at the bottom.

Mr. Babcock.—That is a very old device. This centrifugal action is very much more efficient.

No. 222.—26.

"Power required to drive a blower under conditions known to the respondent."

DISCUSSION.

Mr. H. I. Snell.—I have made quite a number of experiments on the power required to drive a blower under various conditions. It is a subject which I think is very much misunderstood by many of the profession, and I certainly found in my experiments a great number of facts I did not expect to find.

My experiments have been made mostly with what is known as the Sturtevant blower. I recollect one experiment made some thirteen years ago with a number 6 Sturtevant fan blower of the pattern of that date, and known as the "Red Pressure Blower," the fan wheel of which was 23 inches in diameter, and having a width at its periphery of $6\frac{5}{8}$ inches, and a diameter of inlet on each side of $12\frac{1}{2}$ inches, with 8 blades, each blade having an area of 45.59 square inches. I should say my discharge pipe from fan was a conical tin tube, with sides tapering at an angle of about $3\frac{1}{2}$ degrees with the central axis, and that the actual area of discharge was 7 per cent. greater than given in the experiments, in order that the actual discharge of air might be calculated from the area given. This difference you will understand was made to counteract the effect of the *vena contracta.*

Running a fan of the size just given at a speed of 1,468 revolutions per minute, it took 3.55 H. P., and gave a pressure of blast of 3 ounces to the square inch. The same blower I have used at about the same speed, viz., 1,519 revolutions, and it took 0.8 of one H. P., and gave a pressure of 3.5 ounces per square inch, as shown in the accompanying table.

EXPERIMENTS MADE WITH A NO. 6 RED PRESSURE STURTEVANT FAN, AUGUST 15, 1873:

Revolutions of Fan per minute.	Area of Discharge opening in square inches	Pressure obtained in ounces per square inch.	Horse-Power.
1,519	0.	3.50	0.80
1,480	10.	3.50	1.30
1,471	20.	3.50	1.95
1,485	28.	3.50	2.55
1,485	36.	3.40	3.10
1,465	40.	3.25	3.30
1,451	44.	2.88	3.50
1,468	44.	3.	3.55
1,426	89.5	2.38	4.80

These two results were from the same blower under the same speed conditions as near as possible, and the difference in the power required was due to the different areas of discharge, and consequently the different volumes of air moved. In the first case cited, the area of discharge was 44 square inches, and the volume of air delivered 2,750 cubic feet per minute, and in the last case the opening was closed and the volume of air nothing. Air under a pressure of 3 ounces will be discharged into the atmosphere at the rate of 62.5 cubic feet per minute through an aperture of 1 square inch.

Generally, without making any experiments at all with regard to the power required to drive a blower, by taking the rule I see is in the paper by Prof. Trowbridge before this meeting,* giving the theoretical power required to handle a weight of air, and knowing that a fan proportioned as the Sturtevant fans are, will give a useful effect of 66 to 75 per cent., the actual power required may be determined more accurately than *some* experiments show.

If we know the size and speed of the fan wheel, the area of discharge, the area of inlet into fan, and amount of obstruction in inlet or outlet, either from small diameter of long pipes, short bends or angles, etc., it is easy to calculate what the power is or

* See page 531, Vol. VII.

should be, and if it is not giving the useful effect before stated it is not properly belted up or properly piped.

I made experiments, carrying them through for all sizes of outlet and also of inlet, finding the relations one bears to the other, also with wheels of various shaped blades, etc. These experiments were made about the year 1873. I have very full notes of them and intend to put them in the form of a paper at some future time.

I recollect one experience I have had. I was in charge of Mr. Sturtevant's exhibit at the Centennial, and during that time was running a number 12 fan, with a 3-inch single leather belt, and a gentleman well known in the mechanic arts, and for whom I had a great deal of respect, though I never had met him before, came to me, and wishing to ingratiate myself somewhat with him, I said:

"You see that I am running that fan with a three-inch belt."

He replied: Yes; but you have your opening wide open; you have no resistance! The discharge opening was three feet square. I closed the opening immediately and showed him that the fan would run away with itself.

The Chairman.—This conception which Mr. Snell speaks of is a very common one with regard to fan blowers. In a fan blower it is a question of velocity of current, not of pressure, and if you stop the velocity you lose all pressure, and by closing the mouth of the outlet pipe you stop the work of the fan except its friction. I had occasion to make a test recently of a small steam engine which illustrates what Mr. Snell has just said. Tests of the engine were required to be made which necessitated running it under varying conditions of speed and power, from almost a friction card to a card carrying steam almost full stroke, and in order to make these changes quickly and conveniently, and in order to furnish a resistance to the engine, it was belted to a large Sturtevant fan, and on top of the outlet pipe of that fan, which was turned vertically, I had a head fastened with a sliding gate by which I could vary the amount of aperture. This was so arranged that the opening was rectangular. Now, if the opening was equal to the whole area of the pipe for instance, the card will be almost rectangular; if you throttle the opening and reduce its area the card becomes somewhat smaller, and by varying the area of the opening of the discharge pipe of the fan the duty of the engine could be changed to any desired point.

Mr. Snell.—I recollect that a member of this Society once called

upon me and wanted me to visit his works and see if I could improve his arrangement for blowing his cupolas. He was using a number 7 Sturtevant pressure blower and objected to the amount of power he was using. I made some tests of his blower and told him the reason was that his blower was not large enough to work the most economically for doing his work, and he ought to have a number 9. He replied, as most men would, I wanted to sell him a blower. I told him no, I wanted to do him a favor and Mr. Sturtevant justice. He told me to make the change, which I did, and reduced the amount of power to do the same work about nine horse-power.

I would like to say in reference to the table which is circulated for the power to drive Sturtevant fans that that table is calculated only for one speed or number of revolutions per minute for each size of fan, and that the speed necessary to produce a pressure of air equal to two ounces per square inch; it is also calculated for a certain area of discharge and not the full size of the outlet of the fan. The area of discharge of the outlet of the Sturtevant fan is about twice what is technically called the "capacity of the fan." The object of making it larger is to lead people to use as large pipes as possible, thereby reducing the loss in pressure by friction when sometimes conveying air long distances.

The "power required" and "volume of air" given in the table are calculated on the basis of the "capacity of the fan" and a two ounce blast.

Prof. S. W. Robinson.—It seems to be correct in principle, that when no air is moved through the fan no work is done. The energy stored in the moving air is in proportion to the mass, and the square of the velocity; and of course that is nothing when the outlet orifice is stopped entirely, and in reasoning from that to the full mouthed opening, the energy of course stored in the air would vary from nothing to the full extent in varying from the zero orifice to the full sized orifice.

Mr. Snell.—There is another thing in this connection to which I would like to call the attention of the members of the society, amd get a little information from them.

My experiments ran also in the range of heating, and in connection with a fan, I used what might be called a tubular heater, placed on the inlet side of the fan, running the fan at varying speeds from 400 up to 2,100. This tubular heater was so arranged that I was drawing the air through the tubes and heating it with

steam that surrounded them. In running it about 1,100 revolutions per minute I found that the temperature of my issuing air from the discharge of the fan, which was about thirty feet from the heater, was about 161 degrees. The temperature of the water was, I think, 190—the condensed water produced from the exhaust steam used in heating the air. Wishing to know how hot I could heat the air by letting it remain longer in the pipes, I ran the fan down to as low a speed as 400 revolutions a minute. I found I didn't get it any hotter. Then I wanted to see the effect of going the other way and I speeded it up to 2,100 and I found I did get it a little hotter. One reason why I got it a little hotter may be because I increased the density of my air, increasing its capacity for heat, but why was I enabled to heat material as high when passing it through rapidly as when passing it at one-fifth the speed? Mr. Sturtevant told me he did not believe I did, and bet a hat on the subject. He lost, but never paid to my knowledge.

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APPENDIX VIII.

MEMORIAL NOTICES OF MEMBERS DECEASED DURING THE YEAR.

WILBUR HODGSON JONES

Was born in Wilmington, Del., May 31st, 1859. He was engaged for ten years in mechanical pursuits with Hilles & Jones and the Harlan and Hollingsworth Co. of that city, and with the Sherill-Roper Air Engine Co. and Otis Bros. & Co. of New York. He was superintendent of the Brooklyn works of the Air Engine Co., and designed and superintended construction of various kinds of engines and machinery; acted as draughtsman for the Daft Electric Co. of Jersey City, and for the Diamond State Iron Co. of Wilmington. It was while superintending the erection of the new finishing mill for the latter Company that he met an instant death by the fall of the unfinished building upon him during a heavy wind storm, July 29th, 1885. He was elected a member of the Society at the Pittsburgh meeting (IXth) May, 1884.

FREDERICK E. BUTTERFIELD.

Born at East Longmeadow, Mass., July 9th, 1853. In 1870 went to Knowles S. P. Works at Warren as machinist's apprentice; educated at Wilbraham and Springfield Academies for Worcester Institute, which he entered in 1874. On graduation in 1877 he was for two years and a half draughtsman at the Knowles S. P. Works, and for three years and a half head draughtsman of the Deane S. P. Co. January 1st, 1884, he became superintendent of the Goulds Manufacturing Co., at Seneca Falls, N. Y., and remained there till his death, on September 3d, 1885. His death was due to quick consumption, attributed to overwork. He became a member of the Society at the New York meeting (VIIIth) in October, 1883.

WILLIAM CLEVELAND HICKS.

Born in New York City, July 21st, 1829. His father was Rev. Dr. John A. Hicks, of Rutland, Vt. In 1844 he entered Middlebury College, Vt., and going thence to Trinity College he graduated in 1848; acted as draughtsman in Ballard Vale machine shop while still in college. After graduation assisted in laying out Rutland & Burlington R.R., and afterward in shop of Woodruff & Beach; thence made assistant superintendent Colt's Armory, and afterward city engineer of Hartford, laying out parks, inaugurating a sewage system, and building the first water works of that city. He acted as engineer for Col. Colt in reclaiming the South Meadows of Hartford by a dike to shut out the Connecticut River.

In 1855 he became superintendent of the Volcanic Fire Arms Co., and made many inventions and improvements in that line, among them the first extractor for the shells of cartridges for breech-loading guns. The Volcanic gun, as perfected by Mr. Hicks, is now known as the Winchester rifle. This experience fitted Mr. Hicks for the manufacture of the sewing-machine, and the "Singer Family" machine was his design. He also invented the "automatic take-up" used by Wilcox & Gibbs. At the outbreak of the war of 1861 his experience in special manufacture enabled him so to alter the gun machinery for governmental work as to raise the output from 500 to 1,000 guns per day.

The Hicks Engine was invented by him, and has been used abroad to some extent, and has been a basis for many modifications under other names. For twenty-five years he has acted as a patent expert, and for some time before his death was a special expert for the U. S. Government in its suits. He gave especial attention to electricity in its engineering applications. His health has not been good for more than a year, but his final sickness lasted but six weeks. He died at Summit, N. J., on October 19th, 1885. Mr. Hicks was one of the charter members of the Society, having come into it in 1880, before the first meeting.

DAUPHINE S. HINES.

Born at Stillwater, N. Y., January 28th, 1829. For several years after the family removed to Lonsdale, R. I., he worked in the cotton factories; had but nine months of continuous school opportunities; he learned the machinist trade with Thomas Hill in Providence, R. I., building cotton machinery, and showed his talent

by pointing out the incorrectness of the cone on the Hill spindles of that time. In 1849 he came to Williamsburgh, N. Y., and entered the shop of Worthington & Baker as journeyman. This shop was then building the Worthington pump, and he remained there four or five years, working part of the time on outside jobs in steamship work. When Mr. Worthington, in 1854, started the Hydraulic Works in South Brooklyn, Mr. Hines was selected to have charge of the office, drawing room and buying department, and after two or three years became general manager and superintendent. In 1865 he was taken into the firm of H. R. Worthington, and was a member of that firm in its reorganization after the death of the senior member. His health began to fail over three years ago and he was obliged to relinquish active participation and control in the business, and absent himself in pursuit of relief from pain. He died at his home in South Brooklyn, N. Y., on November 10th, 1885.

Mr. Hines was one of the founders of the Society, and attended its preliminary re-union on February 16th, 1880. He acted as member of the Finance Committee during the critical times of the first years of its life.

THEODORE BERGNER

Was born in Germany in 1844; came to America in 1849; in 1850 was apprenticed to the Franklin Iron Works of Philadelphia (J. T. Sutton & Co.), and was for four years there, mainly in drawing room. After an association with H. Howson as mechanical engineer and patent agent he became draughtsman for Wm. Sellers & Co. in 1857, remaining as chief draughtsman till 1874; acted as representative for his firm in the Expositions of 1867 at Paris, and of 1873 at Vienna; from 1874 to 1877 in business as mechanical engineer making brewery machinery a specialty; from 1877 to 1879 in Europe as representative for Hoopes & Townsend of Philadelphia, introducing special machinery and manufacture of bolts, nuts, and rivets into Austria. The Styrian irons were specially available for this. In 1880 designed and erected the brew house for Bergner & Engel in Philadelphia, and in 1882 the brewery for the Bemis & McAvoy Brewing Co. in Chicago, the latter having a capacity of over 1,600 barrels per day, with much ingenious and automatic machinery. Failing health rendered it necessary for Mr. Bergner to seek some sphere which made less calls on his endurance, and he accordingly turned his attention to patent soliciting, and endeavored to push

the manufacture of his special design of drawing-boards. A description of the mechanical features of this board is given in Volume VI. of the transactions of the Society, and is the only contribution Mr. Bergner ever made. His death was due to heart disease, and took place, January 5th, 1885. He was elected to the Society at the VIth meeting in New York, 1882.

EMILE FRANÇOIS LOISEAU

Was born in 1831. From 1857 to 1866 was General Agent for the United Collieries of the Lower Sambre, at Tanimes-sur-Sambre in Belgium. Came to America in March 1866, and made a specialty of the utilization of coal waste and iron-ore waste compressed into briquettes. From 1866 to 1869 was experimenting at Nashville, Tenn., with bituminous slack. In March, 1869, came to Philadelphia and tried with anthracite dust, at first on a small scale and later (1870 to 1873) on a larger scale, under the auspices of the Lehigh Coal and Navigation Co. at Nesquehoning, in Carbon Co., Penn. The machinery for these experiments was built by Albright & Stroh, at Mauch Chunk, and by Naylor & Co., Philadelphia. I. P. Morris & Co. and the Eagle Iron Works built the machinery for a large plant at Port Richmond, including a distributer, mixer, compressor, dryer and conveyers under Mr. Loiseau's designs, and the works were erected in 1874. In 1882, after improvements, the works were in running order, and the fuel product met with favor. These works were destroyed by fire in 1883 during the absence of Mr. Loiseau, enforced by blindness due to cataract on both eyes. A successful operation restored his eye-sight, and before his departure for Europe he was engaged in reorganizing his works, with improved machinery.

Mr. Loiseau returned to Belgium in August, 1885, carrying with him his apparatus, as recently made for him by I. P. Morris & Co., for the manufacture of the compressed fuel, and after some negotiation connected himself with the United Collieries of the Charleroi basin for the installation of his process. But just as the manufacture was to be begun the disturbances of the social order of the district broke out, and the new machines were buried under heaps of coal dust. Even Mr. Loiseau himself ran great risks during this troublous time. When the commotions had subsided, early in April, 1886, Mr. Loiseau had already begun to suffer from an aggravation of a catarrh and liver trouble which had already annoyed him, but was only confined to his bed for the last three

days of his life. He passed away on the 30th of April at his home. His son intends to carry on the business as left by the unexpected death of the father. Mr. Loiseau entered the Society at the Pittsburgh (IXth) meeting in May, 1884.

APPENDIX IX.

[NOTE.—At the Annual Meeting in Boston, November, 1885, addresses of welcome to the Society were delivered by Mr. Edward Atkinson, President of the Boston Manufacturers' Mutual Fire Insurance Co., and by Gen. Francis A. Walker, President of the Massachusetts Institute of Technology. The Publication Committee directed that these addresses be printed as an appendix to the Volume of Transactions.]

ADDRESS OF MR. EDWARD ATKINSON.

MR. PRESIDENT, LADIES AND GENTLEMEN:—I can only conceive that I am set up here as one of the plain people, to welcome you who touch the plain people and who come nearer to them than almost any other class of scientists. I know nothing myself of the physical sciences and have missed a scientific education. I can only speak to you of what I do not know, which, however, is perhaps a good qualification.

We welcome you here, because, as I said, you do touch the plain people more than any other class of scientists. You bring to our immediate use and to our immediate application the work of scientific research. What could it advantage us that the botanist should test the timber, that the chemist should explore the mine, or that the naturalist should tell us of the beasts of the field and of the forest, unless you came between and laid down the timber upon the ground, the rail upon the timber, constructed the engine and brought together the rich fruits of the country? Are you not the missing link between us,—whose brains are not equal to the higher mathematics and who do not know a cam from a cosine,—to unite us to the scientists whose heads are among the stars? That, it seems to me, is your function, and during the last twenty-five years what you have accomplished! I think I am safe in saying that more has been done than in any other equal period of the world's history.

I have been studying the statistics of food of late. To the average man, half the cost of living, measured in money, is even now the price of food, and in relieving him from a part of that arduous struggle to which he would have been subjected, here in New England and in many other places, what have you left undone? Never in the history of the world has the arduous struggle for life been so much relieved as during this recent period. You have abolished space and eliminated time. A day's work of a common mechanic places him next door to the prairies of the West; and it is by your work that this is done. You have made abundance where scarcity would have been. I have said that this has been accomplished in these recent years since the end of the civil war by which this nation was made a unit. In that period greater abundance has been secured and greater relief from arduous work has been given than in any other equal period in the world's history, and in this final application of science the mechanical engineer has been the chief agent.

We welcome you here, those of us who are qualified by what we don't know, to see what we are attempting to do for those who will follow us, and to instruct us in what more we can accomplish in the matter of technical education.

You have with you your sisters and your wives. To them also we extend all the welcome that they deserve. But who can measure their merit? What is their desert?

> "Our guardian angels,
> O'er our lives presiding,
> Doubling our pleasures
> And our cares dividing."

We will adopt you, ladies, as our cousins and our aunts and devote all our wives' relations to your entertainment when you cannot be with us on our excursions. And so to each and all we say, while you will find in our crooked city ways which are not plain, yet we will endeavor that you shall not escape from them until you have tasted of our hospitality.

ADDRESS OF GEN. FRANCIS A. WALKER.

MR. PRESIDENT, LADIES AND GENTLEMEN:—His Honor the Mayor has bidden you welcome to our good Puritan City of Boston, and it becomes my humbler task, as suits my more limited jurisdiction, to tender to the Society the use of the buildings of the Massachusetts Institute of Technology, and to express the great pleasure which the Corporation and Faculty of the Institute feel at receiving within their walls the American Society of Mechanical Engineers. This courtesy and hospitality we would gladly tender to any learned society; but we are peculiarly gratified to receive and to entertain an association whose aims and purposes are so closely congenial to our own; our instructors will highly value their opportunity, during the next three days, to point the students of the mechanical engineering department, the latest and yet already by far the largest of the departments of instruction, to those who have won the honors of the profession to which they themselves aspire; to those who, in actual practice, on the large scale, under the test of competition, subject always to economic conditions, have courageously and successfully applied the principles which these young men are engaged in acquiring, in abstract form, through the studies and exercises of the recitation and the lecture room, or are engaged in putting into practice, tentatively on the small scale, and under the more favorable conditions of the laboratory and the workshop. And we believe it will be no slight or transient inspiration which these young men will receive, as they behold the recognized leaders of their future profession, the masters in hydraulics and steam engineering, in mill work and steel construction, going in and out among them during the next three days. It is always well for the apprentice, the novice, the pupil, the cadet, to look up to those who have engaged in the strife, who have borne its heat and burden, who have carried themselves nobly there, and have won great names by high deeds well done. And on your part, gentlemen, we hope and trust that it will not be without interest and sympathy that you will regard these young men who are laboriously and patiently striving to qualify themselves to become your juniors, and in time your successors, in the profession to which you belong.

You have called this the sixth annual meeting of your association to assemble in that part of our land, where, I think I may make bold to say, your profession has been most highly honored in the past, and most strikingly and brilliantly illustrated in practice. The opinion has perhaps widely prevailed that here in Boston and in Massachusetts and in New England, consideration is paid chiefly to culture and the refinements of life or to acquirements in art and letters; but there could not be a more mistaken apprehension. Boston has striven hard to acquire

that wisdom which leads to expending wealth worthily and well, with what success let the parks and streets of our city and these institutions of beneficence, education and art, arising on every side, testify. But Boston is not more proud of the knowledge how to spend wealth worthily and well than of the knowledge how to produce wealth efficiently and rapidly. Harvard College and the Boston Athenæum and the Museum of the Fine Arts and Trinity Church are no more facts in Massachusetts than are its great manufacturing cities built up by the genius of its own sons; nor are they more characteristic of the peculiar and dominant genius of New England. The names of Quincy, of Channing, and of Felton are not more honored in Boston than are the names of Lowell and Amory, of Baldwin and Francis. Those men who on this bleak and barren shore have in seventy years built up the second largest and most important manufacturing district of the world, are entitled to rank with great captains in war and with the leaders of every art of peace. Assuredly if the plodding peasant, who makes two blades of grass to grow where but one grew before, is entitled to be called a benefactor of his species, the engineer who, by his profound knowledge of the principles of science, with boldness in conception, with fertility of resource, with undaunted patience in execution, hews his way through mountains to make a path for the traffic of States; who curbs the career of mighty rivers and chains them to the wheels of industry; who builds up cities, not with the plunder of bleeding nations, but with the spoils of vanquished nature; whose common work it is to lessen the pains and increase the fruits of others' labor; whose profession it is to find on every hand opportunities by which hundreds, thousands it may be, millions, perchance, of human beings may be able to eat their daily bread in decency and humble comfort—surely that man is not to be spoken of as practicing a low and materialistic art, of base and sordid aims. Matter it is that he deals with, indeed: but only that he may transform and transmute it by the power of the human soul, by knowledge, by patience, by industry, by skill and by the scientific imagination. He is the true and only wonder-worker of our age; and the most lame and halting narrative of the deeds he has wrought far transcends all the tales of Norse or of Oriental magic. He is the true philanthropist, for he does not distribute a charity that degrades and corrupts, but he makes places and opportunities, on every hand, where the industrious and self-respecting poor may earn their own livelihood. And so, Mr. Chairman, and gentlemen of the Institute of Mechanical Engineers, I might continue in this strain, descanting upon the dignity and the usefulness of your profession, so young and yet so full of lusty vigor and so big with the promise of things to come; but these things you know far better than I. The only thing that remains for me to say is one which you cannot know half so well, and that is, how heartily your are welcome to our city and to our school, to our homes and to our hearts. In the language of the Oriental host, but with all Occidental sincerity, I say, "The house, it is yours."

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